

The equality positions and policies of governments, 1970-2020

How egalitarian parties pursue equality – yet evade redistribution

Alexander Horn

University of Konstanz
alexander.horn@uni-konstanz.de

Leo Ahrens

University of Konstanz
leo.ahrens@uni-konstanz.de

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Abstract

Redistributive rhetoric is omnipresent in politics. But do parties really deliver equality policies once in office? We start with the assumption that parties are policy seeking. But while egalitarians generally pursue equality enhancing policies, they often avoid significant fiscal redistribution (*from the rich*). We argue that parties choose the policy path of least resistance and use equality policy that inflicts less concentrated costs on the upper (middle) class. Rather than increasing tax rates on high incomes, they focus on wage and welfare policy (*redistribution to*). We further expect that the link between equality rhetoric and redistributive policy depends on a party's voter base: the more affluent and educated a party's electorate, the less actual equality policies.

Based on the crowdcoding of 850,000 party statements and the first comprehensive database on government positions regarding economic (in)equality, we test if cabinet parties live up to the egalitarian ideals they evoke in manifestos. How does the extent to which parties positively emphasize economic equality and redistribution affect the three policy pathways through which parties affect (in)equality? Analyzing 1) fiscal redistribution (taxes on top incomes), 2) pre-distribution (minimum wages), and 3) and welfare state policies (unemployment insurance generosity) in 12 OECD countries from 1970 to 2020, we show that the promise of levelling often remains unfulfilled. Egalitarian parties pursue welfare state compensation (for the middle) and pre-distribution (for entry level wages) – but not more radical fiscal redistribution (from the rich). These results hold across left and right cabinets, econometric specifications, and they do not depend on a specific country.

Supplementary information (SI) at page 23

Questions:

What literature is still missing?

Are the arguments convincing?

Top PIT non-result is convincing?

Relegate H3 to robustness of H2?

Is it fair to speak of *selective egalitarianism*?

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INTRODUCTION

Do egalitarians strive for equalizing policies or is the omnipresent talk of equality predominantly cheap talk? As for voters, a growing literature shows that evoking equality while avoiding equalizing policy are not incompatible – especially when ideals and interests collide (Cavaillé 2023). However, for political parties and governments, no data on equality positions, however vague, is available so far. Based on new data on parties' egalitarian policy positions and their evolution between 1970 and 2020, we shed light on the egalitarian puzzle of omnipresent redistributive rhetoric that seems to contrast with high – and (largely) increasing – inequality.

To provide a meaningful test of the impact of governments, we measure the share of positive references that a government makes to economic equality instead of vague and undefined egalitarian lip service. Across polities, fuzzy warm statements such as “for the many, not the few”, “a place for everyone” or “no one left behind” (three slogans that Labor in the UK, Denmark, and Germany recently invoked, respectively) abound. Yet, it would surprise few if any scholars if such vague notions along the lines of “We are the party of equality” would *not* yield specific and robust policy responses to growing material divides in OECD countries. A more credible test of the hypothesis that parties exert (equality enhancing) policy effects must rely on party preference/position data that speaks directly to the pursuit of (greater) economic equality and redistribution. We conduct this test in this paper. Such data is lacking even from the most recent and most comprehensive assessments of the politics of inequality (Lupu & Pontusson 2023, Piketty 2022, Haselmayer & Horn 2024).

While the theoretical work horses in political science such as partisan theory or the mandate model (Budge 1994; Hibbs 1977) suggest that governments mostly do what they say and signal pre- election, we discuss a variety of reasons why such optimism about political steering may not apply regarding inequality reducing and/or buffering policies. More, specifically, we build on two arguments that may explain why the equality positions of parties may at best partially translate into egalitarian policy output and greater societal equality.

First, building on a concentrated costs framework (concentrated costs are unpopular among voters and avoided by parties, cf. Weaver 1986, Pierson 1994, Zacher 2024), we argue that costs for predistribution via minimum wages are diffuse, medium concentrated for social insurance and concentrated for taxing the highest earners more. Accordingly, we expect that egalitarianism translates into predistribution and (under

scope conditions addressed in the next paragraph) into welfare policies – but rarely into the kind of fiscally progressive policies that much of the debate since the “Occupy” protests (with chants such as “We stand with the 99%”, “tax the 1%”) and the books by Piketty (2014, 2020) or Atkinson (2015) focuses on.

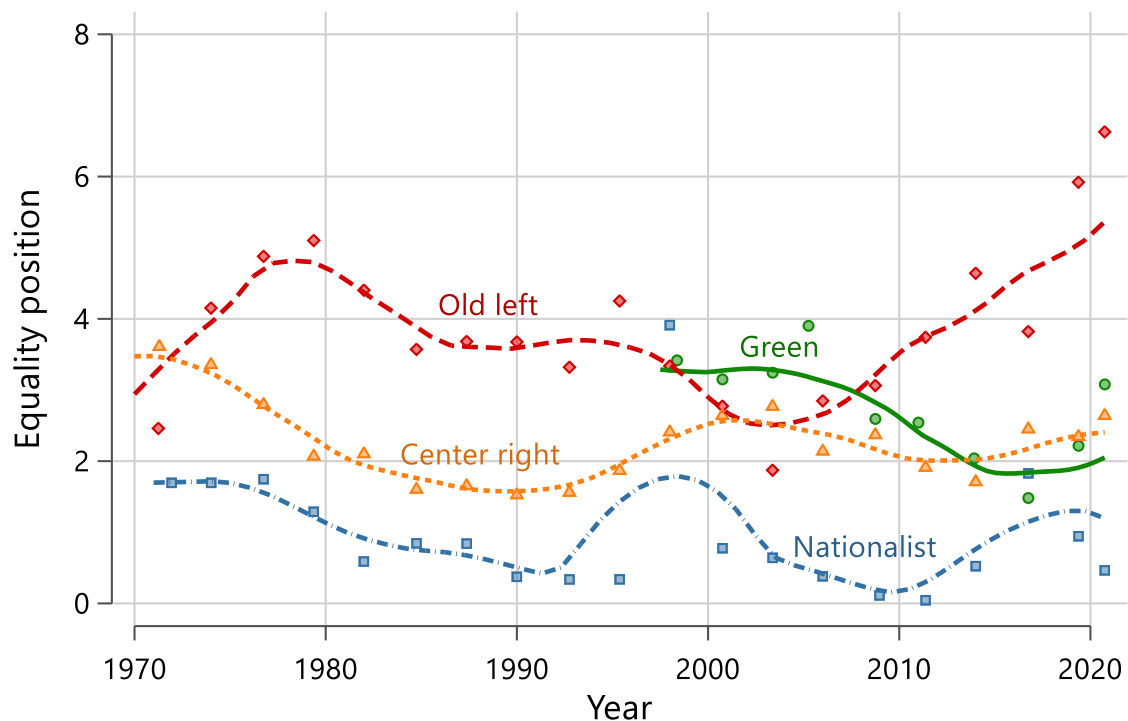
Second, building on this logic and considering the socio-structural characteristics of voters and parties’ specific electoral profiles, we argue that higher education and higher income pose a perceived constraint for egalitarian policies by shifting the balance of equality policies towards emphasizing their costs. In short, there is a constituency specific component to concentrated costs arguments that further constrains cabinet parties.

In focusing on these two constraints, we do not mean to suggest that well researched aspects such as left and right partisanship or coalitions are not important. In line with previous research on the politics matter question (Schmidt 1996, Horn 2017, Zohlnhöfer 2023), we acknowledge the continued relevance of such static historical party labels. It is thus important to show that the results we show hold across left *and* right cabinets. While economic equality positions in general exert effects and non-effects irrespective of traditional left-right party labels, such left and right ascriptions exert effects on equality policy in mixed cabinets. Drawing on issue ownership and credit taking arguments (Green-Pedersen & Mortensen 2015, Horn 2020) and the asymmetric credit-taking opportunities for policy in mixed (i.e., left-right) coalitions they imply, this is exactly what we would expect: when right parties find themselves in a coalition with nominally left parties after the election, they are more reluctant to translate pre-election positions into policy (because the issue owner may benefit).

In line with these skeptical expectations, we show that egalitarian positions of cabinets indeed yield equality enhancing policies—but by other means than the focus on taxation in the literature suggests. In short, egalitarians take the part of least budgetary and electoral resistance. Instead of taxing the rich, they push for more generous welfare state compensation for the middle and pre-distribution for the poorest. In the conclusion, we argue that these channels are by no means functionally equivalent but have vastly different distributional consequences. The policy channels for which we *do* find consistent egalitarian effects are also the ones’ that leave the most affluent the least afflicted and have the added advantage of costing little. As we summarize, even egalitarian parties are often evoking equality while eluding the redistribution that matters.

These findings speak to an emerging literature on what kinds of redistribution are politically feasible (Jensen & van Kersbergen 2016, Coady & Le 2020, Hacker et al. 2023, Müller & Fujimura 2024) and they help to better understand the tension between support for redistribution in surveys and the lack thereof in policy. They also help to shed light on specific examples, for instance the continued gulf between egalitarian rhetoric and the unwillingness to impose concentrated fiscal losses on the upper (middle) classes in the US by the Democrats.

Figure 1: Evolution of equality positions of *government* parties



Note: Percentage share of statements in party manifesto devoted to economic equality by party family (cf. *Data and Methods*).

THE STANDARD VIEW: WHY SHOULD EGALITARIANISM AND POLICY OUTPUTS BE RELATED?

Classical political science theory expects that parties with more left-leaning policy positions should implement more redistributive policies in government. The literature most often invokes partisan theory (Hibbs 1992; Schmidt 1996), which expects that parties from different political camps have distinct policy preferences that they implement once in government. Accordingly, more left-leaning parties such as social

democrats should implement more equalizing policies. There are two reasons. First, politicians may be policy-oriented and have distinct policy goals. Second, parties represent electorates with distinct policy preferences because they are vote-seekers or because they have an inherent wish to represent certain social groups (Adams *et al.* 2004; Huber and Stephens 2014; Korpi 1983; Romeijn 2020; Stephens 1979; Stimson *et al.* 1995).

The problem with the perspective of classic partisan theory is an—often unwarranted—leap of faith from the historical roots of a certain party family to more contemporary policy positions and outputs. For example, it is assumed that left parties such as social democrats want more redistribution compared to other parties and will implement egalitarian policies once in government. While we consider this to be true historically, we can show that parties with a certain partisan coloring have varying egalitarian positions over time. For example, the data we gathered on economic equality emphasis indicates that social democratic government parties used to embrace economic equality, then deemphasized the issue since the 1990s, before a reemphasis of economic equality again in the 2010's. It is thus not a valid empirical proposition that social democrats consistently advocate(d) economic equality. To underscore this point and provide a first impression of the data we introduce below, Figure 1 plots the share of positive references to economic equality for the old Left, post-material green parties, the Center-Right, and the nationalist Right. While the graph, in line with our research question, only considers government parties (which is one reason why a more fine-grained disaggregation into party families is not useful), the U-shape (i.e., first high, then decreasing, then increasing equality emphasis) for the old Left is a trajectory we also find for social democratic parties overall.

In light of such variation (that is unaccounted for in previous studies), it is not surprising that there is no agreement on the effect of parties measured by left and right share in cabinets (Haselmayer and Horn 2024; Jensen and van Kersbergen 2017). And sometimes traditional roles are reversed, for example when left parties implemented policies such as welfare cuts or workfare that tend to weaken the weak further (O'Grady 2022). The necessity to look at fine-grained policy positions over time is supported as the main prediction from another theoretical work horse of political science—mandate theory—which expects and generally finds that, once in government, parties aim to implement policies in line with their rhetoric (Budge & Klingemann 1994, for a critical view, Mair 2008). Similarly, optimistic expectations can be derived from research on pledge keeping. Here, the general finding in most studies is that promises were – usually – kept (Thomson *et al.* 2017).

In sum, instead of equating the Left with equality policy, we consider parties' economic equality positions to be the most instructive predictor of what parties do in government. The simplest empirical expectation therefore is that more economically egalitarian positions are also associated with more egalitarian policies.

H1: *Mandate theory/partisanship hypothesis: Parties/cabinets with stronger [weaker] emphasis on economic equality on their party manifestos implement more [less] egalitarian policies in government.*

SELECTIVE EGALITARIANISM: WHY EGALITARIANISM AND POLICY OUTPUTS MAY BE UNRELATED

In this section, we discuss several developments that question the strength and prevalence of the supposed relationship between egalitarian positions and equality enhancing policies. Importantly, we do not mean to reiterate well known facts about institutional constraints (Schmidt 1996), but focus on two specific aspects. Each of these two aspects yields an expectation that serves as a qualification for H1 in its above/general form.

Logic of concentrated costs and selective egalitarianism

Ever since Pierson (1994) published his seminal work on the reasons why welfare state retrenchment against the vested interests of groups that would endure concentrated losses as a result of reforms, such arguments are vital for the analysis of reforms. Yet, to the best of our knowledge, they have been marginal in the analysis of equality-enhancing policies – or rather – the relative lack of equality enhancing policies in unequal societies.

Drawing on work on the difficulty to allocate concentrated costs (Pierson 1994, Ross 2007) with diffuse benefits we expect that the extent to which economic egalitarianism translates into policies is conditional on how concentrated the costs of such reforms are. We start with the policy with least concentrated costs and move towards more concentrated costs of policies. First, predistribution via higher minimum wages may moderately increase the cost of labor (at the low end) purchased by strata that do not benefit from it. Yet, while this additional cost is low, many groups above the minimum wage may benefit from an increased wage floor that exerts upward pressure (so called *ripple effects*, BER 2023), so few groups outright reject a (higher) minimum wage (PEW 2021). Employment effects, which could be concentrated as well, have been shown to

be neutral or positive (IAB 2021). Overall, the view that the costs are dispersed is ground in theory and evidence (Popp 2021). Likewise, the cost of more generous unemployment insurance does not fall on a particular group that is disadvantaged, but creates upward pressures on wage levels and automatic stabilizers in economic downturns (Horn 2017, Abraham et al. 2019). To the contrary, increasing top-income taxes creates a clearly defined group of voters with concentrated costs, as top income taxation yields tangible gains and losses that are highly visible for the group that has to pay it (or, transaction costs to minimize or to evade it, cf. Lundberg et al. 2019). In sum, we expect to find effects on policy, on average and *ceteris paribus*, for wage and welfare policy, but not for the top (marginal) income tax rates often at the heart of the inequality debate.

H2: *Parties/cabinets with stronger [weaker] emphasis on economic equality on their party manifestos implement more [less] egalitarian policies in government if they do not inflict concentrated costs. In terms of concentrated costs, we assume that predistribution < insurance < taxation of highest incomes.*

Class structure of a party's electorate as constraint

As a result of the structural transformation of party systems, some parties are more constrained than others. In particular, the transformation of party systems and electorates suggests that even parties with egalitarian positions may not pursue fundamentally different equality policies compared to their less egalitarian counterparts due the growing affluence and education of left voters as part of a broader *Brahmanization* or *Embourgeoisement* (Kitschelt 1994; Piketty 2020). A parallel stream of literature that is less strongly linked to the European cleavage literature points to the growing affluence of the center-left coalition in the US (“metro coalition”, Hacker et al 2023).

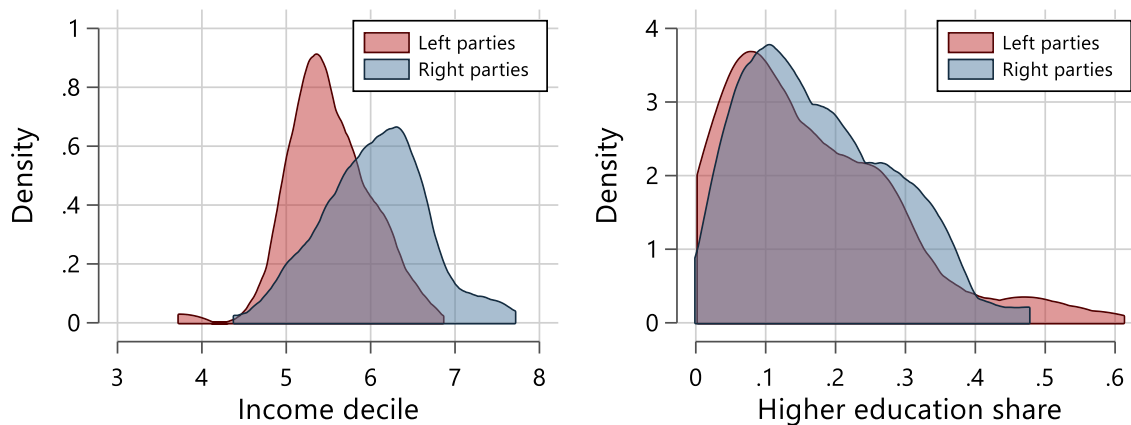
As with the mapping of equality positions on partisanship, and despite the strong focus in the literature on how the left electorate changed in terms of class structure, we emphasize the broad profiles and the variation in the constituencies of left and the right government parties. As Figure 2 shows, the actual class structure of a party's electorate *cannot* be approximated via partisanship, even though this strategy is less misleading for income. This matters because the cost-benefit calculation for equality enhancing policies should become

more negative the more affluent and educated the electorate of a party becomes – up to a point where such embourgeoisement eventually deters risk averse parties from anything but knee-deep egalitarian policies (note that our concern is with redistribution policies and not with particularism vs. universalism and that it is not what voters say in surveys/conjoints but politician’s perception that matter, cf. Fastenrath and Marx 2024).

The support bases of traditionally redistributive and pro poor left parties have become richer and better educated (as Figure 2 shows, the overlap and lack of two distinct curves is more clear for education). This has dampened the distributional rich vs. poor conflict historically associated with the left-right divide (Gethin *et al.* 2022). While high-status voters of left parties do not oppose redistribution per se, esp. the highly educated (Zollinger *et al.*, Häusermann and Fossati 2014), they may still assign less importance to levelling as they are not net beneficiaries (Gelepithis and Giani 2020, Horn *et al.* 2024). This may help to explain some setbacks of green parties when they prioritized progressive taxation. For instance, the most fiscally progressive platform the German Greens launched thus far was blamed for a major electoral setback 2013 (e.g., Blätte 2013).

H3: *Class structure of the electorate hypothesis: The extent to which the governments’ egalitarian positions explain their policy outputs decreases if affluence and education of the electorate are particularly high.*

Figure 2: Parties and class: Distribution of income and education among voters of government parties



Source: Gethin *et al.* (2022).

While the idea that the class composition of the voters conditions whether parties engage in incisive policy change is a plausible qualification for H2, we want to emphasize that what matters for policy effects are – ultimately – not the voters or their preferences but what politicians think about their voters. On this, we know little. Some studies conclude that parties know little about voters (Walgrave et al. 2022), others that parties correctly factor in that the support for fiscal redistribution is rather superficial (Fastenrath and Marx 2023).¹

The role of left and right partisanship

In general, we expect that the effects outlined regarding the constraints apply for left and right governments. However, it is exactly when left and right parties share government responsibility, when party labels matter. Traditionally, the Left has owned issues evolving around welfare state expansion and equality. Although welfare and workfare reforms have led to a partial loss of this (issue) advantage (Horn 2021), it is still the norm that – in comparison with right parties – left parties are associated by voters with pro-equality stances (Green Pedersen 2019). Thus, if right parties find themselves in mixed (i.e., left *and* right) government coalitions, this could make them cautious regarding egalitarian reforms. Due to relatively stable issue ownership advantage of the Left (Seeberg 2017) when it comes to issues of social justice, welfare and equality, there is a palpable risk that it is easier for left coalition partners to take credit for equality enhancing reform than for right parties. In sum, while we do not see how left or right party labels should affect the policy effect of *pre-election* egalitarianism in pure right and pure left governments, from an issue ownership perspective, it is a plausible qualification that parties may factor in issue ownership if they find themselves in mixed cabinets *post-election*.

¹ Fastenrath and Marx (2024), in a case study that compares core group views on taxation and party elites' perceptions, find that the support for redistribution among voters is superficial, and that (SPD) party elites' factor that in since they fear that voters are susceptible to (anti-redistributive) political countermobilization.

DATA AND METHOD

We use a panel-of-countries dataset with yearly values that links countries' policy outputs with the equality positions of their federal governments. We first introduce the new crowd coded party position data before turning to the details on how we link it to governments and our three subtypes of equality enhancing policies.

A new dataset on the economic equality positions of parties

Economic equality: "Labour will introduce a tax on wealth above £100,000." (1974)

Equal rights: "Labour stands with people from ethnic minority backgrounds." (2019)

The two real world statements above illustrate why it is crucial to measure economic equality emphasis. While economic equality statements include positive references to economic leveling or criticize material inequality, the second statement on equal rights pertains to the extension of rights to and equal treatment of minority groups. These minority groups are primarily woman and racial minorities and to a lesser extent disabled people and—increasingly—LGBTQI groups. In the few works that try to approximate equality preferences of parties, these dimensions are conflated despite being not empirically related or even negatively related in some contexts (Pontusson and Rueda 2010; Tavits and Potter 2015). We argue that it undermines a fair test of the partisan hypothesis to consider the second example in a measure that is used to explain redistribution and redistributive policies—especially because critics (such as Thomas Piketty 2020 or Nancy Fraser 2017) argue precisely that equal rights issues are crowding out policy against economic equality.

We use parties' election manifestos to obtain information on party preferences. Such manifestos have frequently been used to measure parties' issue emphasis (e.g., Green-Pedersen 2007, 2019; Wagner and Meyer 2014). However, existing datasets based on manifestos, notably the Manifesto Project (MARPOR) and Comparative Agendas Project, do not code if parties emphasize economic equality or equal rights. We therefore coded the relevant party manifestos ourselves. To do so in a reliable and replicable way, we use crowd-coding, a *proven* solution to scale expertise for tasks so semantically complex that it is still hard to automate them reliably (Benoit *et al.* 2016; Haselmayer and Jenny 2017; Horn 2019; Lehmann and Zobel 2018).

The digitized manifestos were retrieved from the MARPOR database or collected manually from different sources such as (online) archives and libraries. The data collection pipeline looks as follows. We use a sequential binary classifier, trained on MARPOR data, to splice the manifestos' raw text into quasi-sentences. We then used a regular expression to identify sentences that might have been misallocated, which were forwarded to research assistants to check and—if necessary—revise the segmented quasi-sentences. This process segmented all party manifestos into quasi-sentences.

Second, we use another binary classifier to identify statements that likely concern (in)equality broadly conceived. The classifier was trained on MARPOR data to detect statements referencing positive mentions of equality (category 503). The classifier was fine-tuned to reliably identify most relevant statements at the expense of false positives, thereby trading off precision for recall. This extensively validated reduction process left us with about 55,451 quasi-sentences (out of 850,000 in the text corpus overall) that speak to equality and social justice, which considerably decreases the workload for and costs of human coders.

Third, we forwarded the retained statements to crowd-coders on Amazon Mechanical Turk between February and June 2022, a platform that has been successfully used for collecting data (Berinsky *et al.* 2012; Skytte 2022; Sumner *et al.* 2020). After having read a short instruction and having passed a qualification test, coders were presented individual statements and some context and were asked to judge whether it includes a positive reference to equality and, if so, which of the specific categories applies. The qualification test uses 2,904 *unanimous* coding decisions of three experts (postdoctoral researchers familiar with the topic) as reference ("gold standard"). Based on previous research (Horn 2019) and our pre-tests, we collected five codings per unit. This ensured that aggregated results align with expert ratings while enabling fast data collection at low costs. To warrant fair remuneration, we made sure that our workers were able to attain local minimum wages. The coding scheme is listed in the supplementary material that describes the crowd coding pipe line.

Fourth, we aggregated individual codes based on majority vote. This approach reflects the idea of the 'wisdom of the crowd' and is anchored in social choice theory (Condorcet 2014 [1785]). We obtain a very high agreement between the crowd's judgment and the unanimous expert coding. A Krippendorff's alpha of 0.72 exhibits good agreement even by conventional standards of content analysis. Details are provided in the

appendix, which shows reasonably high levels of validity across countries. This means that the crowd can match – and scale – expert judgments of complex tasks, even at the sentence (i.e., below manifesto) level.

For each party at each election, this gives us the share of economic equality statements relative to the overall party manifesto, along with data for other equality concepts and above-mentioned vague references. There is substantial variation across parties and within parties over time. Face validity is high. Consider the often-debated cases of the US Democrats under Clinton in 2016 and the Danish Social Democrats under Mette Fredriksen in 2019. While the Danish Social Democrats focused on material leveling (8%) and neglected equal rights (1,9%), the US-Democrats campaigned with a focus on equal rights (7%) rather than levelling (4%).²

Measuring policies

We require several measures for redistribution to capture the wide range of policies governments may use to level inequality. “Redistribution” is a wide term that, in the loosest sense, refers to policies that make the income or wealth distribution more equal than it would have been without the policy (for a stocktaking exercise, cf. Atkinson 2015). A helpful categorization for types of equalizing policies first distinguishes between *pre-* and *redistributive* policies. Predistributive policies act via equalizing the distribution—primarily of income—on the market (Gornick and Smeeding 2018). In contrast, redistributive policies “correct” market outcomes by changing the distribution post market allocation. These can be further categorized into social policies that distribute transfer income and tax policies aimed at collecting taxes from citizens (Guillaud *et al.* 2019). This leaves us with a classification into predistributive policies, social policies, and tax/fiscal policies.

To measure **predistributive policies**, we use the Kaitz index that quantifies the strength of minimum wages, which we source from the OECD. It indicates the ratio between the federal minimum wage and the median wage in a country. Minimum wages are an ideal measure for predistribution because econometric evidence reliably shows that they reduce inequality while having mostly negligible employment effects (Autor *et al.* 2016; Cengiz *et al.* 2019; Dustmann *et al.* 2021). They function via boosting the wages of workers below and even somewhat above the minimum wage threshold. However, the downside is that the measure is only

² As a final point on cabinet ideology, we want to make explicit what we deem obvious: while we agree that it is instructive to assess mixed coalitions (as we do below), for the purpose of our explanandum policy change, cabinets are in effect unitary actors. The underlying decisions in favor or against certain changes cannot be disaggregated for the 50-year time span we capture here.

available for countries with a minimum wage in effect, which excludes countries such as Denmark that rather rely on strong union bargaining power to set a wage floor for specific sectors rather than the national level.

To capture **welfare state/social policies**, we rely on the standard measure of unemployment insurance generosity, using the Comparative Welfare Entitlements Dataset (CWEP) by Scruggs (2022). We chose unemployment benefits because this is in line with a large proportion of previous studies on partisan effects to the welfare state (Bandau and Ahrens 2020). Already in the classic work on Power Resources (Korpi 1993), unemployment insurance is considered as a source of decommodification and equalization thus oft seen as a *pars pro toto* for the welfare state at large (Horn 2017). Unemployment benefits are inherently redistributive, distributing from those with to those without labor market access in the short term and from those with riskier employment profiles to those in more stable employment in the long term. They also tend to set the wage floor (reservation wage) and exert upward pressure on medium high wage levels. Unemployment benefits are therefore a very likely – if not most likely – case for party effects that derive from different equality profiles. Despite this role as neuralgic point in welfare states, as with most welfare programs, generous unemployment schemes primarily serve an insurance purpose and very few would consider them a radical Robin Hood Policy.

As for progressive **taxation**, we rely primarily on the top marginal income tax rate sourced from the OECD, which proxies taxation of the rich (Hope and Limberg 2022). While there are many other approaches, the very broad availability over time and across countries paired with functional equivalence, the fact that we are looking at a policy output rather than an outcome, and the intuitive interpretability of the measure are a mix of advantages that make it hard to find indicators that perform equally well on all of these crucial dimensions. Unless there is an equally fitting indicator with equally good coverage of the OECD and our long time series (that we have overlooked, which we deem highly unlikely), we are bound by this indicator. Again, this is not to say that other, less tangible and less broadly available, aspects, such as effective taxation (Egger et al. 2019) are politically irrelevant. We are aware that top rates may be higher than what the rich pay, but a (non) effect on top rates is logically prior to loopholes, enforcement and implementation (for which we do not have data).

Control variables

We include several controls variables to account for possible confounders in our regression models: GDP per capita, the share of industry employment, inflation, unemployment, the share of citizens not in working age, and capital account openness. If not stated otherwise, we use constantly updated data from the Comparative Political Dataset (Armingeon et al. 2023). At the end of the analysis, we assess if historical party labels matter.

Empirical setup

We use a panel-of-countries dataset with yearly values. The party position data is available on a country-election level. We restrict the data to government parties and convert them into a country-year structure using the *Parties, Institutions & Preferences (PiP)* code infrastructure by Jahn et al. (2022). The resulting dataset records governments' positive mentions regarding economic equality for each country and year. Values are weighted according to the number of cabinet seats within coalition governments. If multiple governments served within the same year, the values are weighted according to the time in office. To test our hypotheses, we use a fixed effects (FE) regression model of the following form, estimated via ordinary least squares (OLS):

$$\mathbf{policy}_{ct} = \beta \mathbf{equality}_{ct-1} + \delta \mathbf{cntrl}_{ct-1} + \alpha_c + \varepsilon_{ct} ,$$

That is, we regress a policy measure (e.g., level of minimum wages) of country c in year t on the equality position of their government in the preceding year, as well as several controls and country dummies.

We perform several variable transformations. The policy variables are always standardized based on their within-country variance in the dataset to make results comparable across the policies. Furthermore, some variables in our models have unusual values that may have an outsized influence on the results because OLS regressions are strongly influenced by unusual values. We use the natural logarithms of governments' equality position and GDP per capita due to their right-skewed distributions. We additionally winsorize³ several variables to bring outliers closer to what would be expected under a normal distribution: the logged equality

³ Winsorizing is a technique that limits the minimum and/or maximum values of a distribution. For example, winsorizing at the 1st percentile entails that values below the 1st percentile are recoded to the 1st percentile. However, this is not decisive for the results.

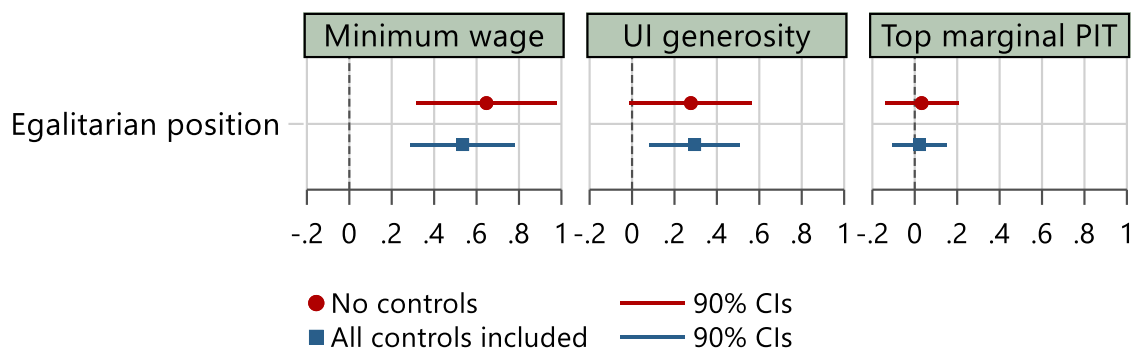
positions and the generosity of the unemployment insurance are winsorized at the 1st percentile; and economic growth and inflation are winsorized at both the 1st and 99th percentiles.

We use wild cluster bootstrap, clustered by countries, to estimate the standard errors. The approach effectively deals with the problems associated with the data setup, namely a clustered dataset with few clusters (Cameron *et al.* 2008). We require clustered standard errors because of serial autocorrelation within countries. However, the usual cluster-robust standard errors require a large number of countries (at least 50) and optionally a large number of observations within these clusters. Violations of this assumptions typically lead to underestimation of uncertainty. Wild cluster bootstrap is an alternative that remains valid with a small number of clusters, such as in our case. Not using wild cluster bootstrap yields essentially the same findings.

RESULTS

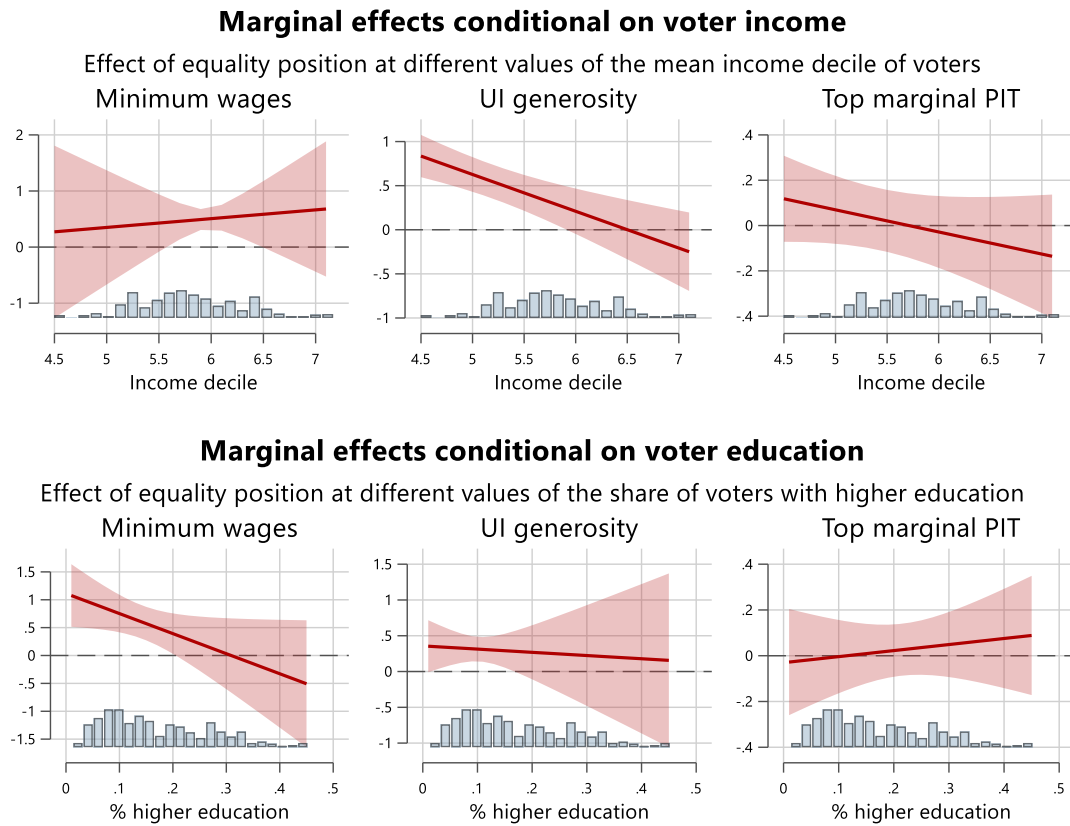
We begin the empirical analysis with the headline results before turning to subgroup analyses and the extensive robustness analyses we conducted. Figure 3 shows the results across the whole analysis sample. It becomes apparent that, overall, the classic mandate theory hypothesis finds only partial support. In line with expectations, parties with a more egalitarian platform strengthen the minimum wage (in countries and years where a minimum wage is in effect). The effect size is considerable, with a 50% increase in the egalitarian position being associated with a .32 standard deviation higher minimum wage.⁴ The effects on unemployment insurance generosity are significant when controls are included, but somewhat less pronounced. Results for Top marginal income taxation are – in line with our pessimistic expectation – essentially non-results, neither significant nor substantive. If we were to redo this analysis with the *much* more restricted effective taxation sample, we obtain the same non-results. This offers first support for the no progressive taxation hypothesis.

Figure 3: Main regression results



Note: The control variables are GDP growth, log GDP per capita, unemployment, inflation, the proportion of the work force working in industry, the proportion of elderly, and capital account openness. All models include country and time fixed effects (five-year intervals). The standard errors are estimated via wild bootstrap clustered by countries (as shown below, results are robust across specifications). Full regression models in the online appendix (Model 1 without controls, Model 2 with all controls).

⁴ The independent variable is logged. The effect size of a p% increase in the independent variable can thus be calculated via $\beta \cdot \log([100+p]/100)$ (see Benoit 2011).

Figure 4: Interaction results

Note: The control variables are GDP growth, log GDP per capita, unemployment, inflation, the proportion of the work force working in industry, proportion of elderly, and capital account openness. All models include country and time fixed effects (five-year intervals). Standard errors are estimated via wild bootstrap clustered by countries. Regression tables in the online appendix (Model 3: income, no controls; Model 4: income, all controls; Model 5: education, no controls; Model 6: education, all controls).

We continue our analysis with interaction regression models (see Figure 4) to assess how conditional these effects and non-effects are on constituency composition (calculated at the party level, aggregated for the cabinet). As the regression tables show, we look at similar models as before, but now we gauge the interaction of egalitarianism with either income or education levels of the electorate of the respective party. In our reading, Figure 4, which shows the marginal effect, conveys three main messages regarding the effect of economic egalitarianism among government parties: First, the higher the share of the lowly educated (understood as non-tertiary education) in a government's electorate, the more positive the effect on redistribution policies. Second, the higher the share of the less affluent (understood as around or below

median) in a government's electorate, the more positive the effect on welfare state insurance policies. Third, and perhaps most importantly, there is no discernible (interaction) effect of equality and constituency composition on Top marginal PIT. In other words, while a party's class profile constrains egalitarian governments regarding predistribution and welfare, it does not condition the non-effect regarding top (income tax) rates. As we show below, even via a number of complex additional test, the non-effects remain.

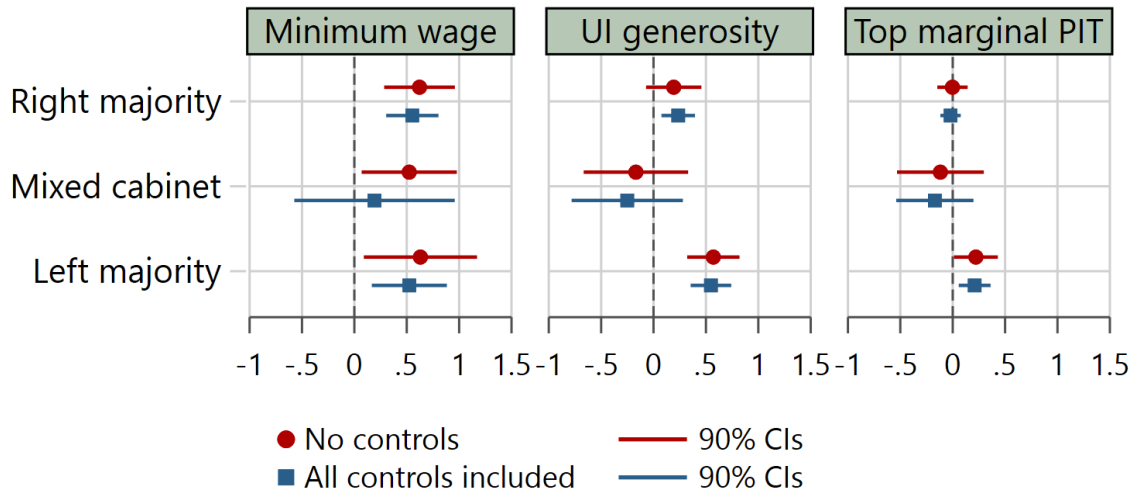
ROBUSTNESS: THE ROLE OF LEFT AND RIGHT PARTISANSHIP, SPECIFICATION, INDIVIDUAL COUNTRIES

While the point of departure of this analysis (and a point that is obvious from Figure 1) is that historical party labels are of limited use if the question is if and how egalitarian governments legislate equality policies, we do not want to gloss over a question that many scholars habitually ask: how does this depend on party labels?

A simple way to check this is to control for the share of left parties in government. The results are shown in figure X in the SI and the pattern is in line with the headline results above. However, as discussed, while we had no reason to expect different results for left vs. right parties at a given level of economic egalitarianism, it is less convincing to disregard partisanship in mixed governments in which issue ownership arguments would suggest that left coalition partners could benefit disproportionately from delivering equality policies. This asymmetry, or even perceived threat, may disincentivize the non-left coalition partner.

Therefore, we also look at left and right coalition governments as well as mixed governments that consist of left *and* non-left parties. In figure 5, we thus compare right, mixed and left majority cabinets. The first thing to note in Figure 5, leaving aside that confidence intervals are bound to be broader due to the lower n , is that the previously stated findings apply to left and right majority cabinets. Results are similar to the specification shown in Figure 3. The second aspect to note, however, is that mixed cabinets (that consist of left and non-left parties) show no significant or substantive effect for any of the three policy dimensions. Even for minimum wages and unemployment insurance generosity, equality stances do not translate into policies.

Figure 5: Main results and partisanship



Note: The control variables are GDP growth, log GDP per capita, unemployment, inflation, the proportion of the work force working in industry, the proportion of elderly, and capital account openness. All models include country and time fixed effects (five-year intervals). The standard errors are estimated via wild bootstrap clustered by countries. A full regression table is available in the online appendix in Table 7.

We conducted several additional analyses to ensure that our results are robust to alternative specifications. Two test strategies are worth highlighting – since they are not usually presented and discussed with the same degree of visual openness we opted for here. First, while we stand by our critiques of some econometric practices and the modeling choices we motivated in detail above, it is still worth noting – and readers may find it reassuring – those alternative choices such as Praise Winston with PCSE or the use of (Standard) Robust Standard Errors yield similar results (shown in SI Figure 2 and 3). Second, we test and visualize the impact of all 12 individual countries using jackknife methods (explained and shown in detail in the SI Figure 4, with our countries on the y axis). No individual country seems to exert an outsized influence on the results we present.

Q: Is there a must have variable/analysis that you would like to see in addition to the robustness tests we list?

CONCLUSION: SELECTIVE EGALITARIANISM – EVOKING EQUALITY WHILE ELUDING REDISTRIBUTION

Egalitarianism leads to pro-equality policies, but even egalitarian cabinets are reluctant regarding fiscal redistribution. Instead, they address market inequalities and lift the wage floor via (wage) pre-distribution and ameliorate labor market risks via welfare policies. While predistribution is a crucial yet oft-neglected pathway, it is also tilted towards low-end inequality. Similarly, why egalitarian governments are inclined to strengthen insurance schemes with partly equalizing effects, it is – primarily – the middle class that benefits.

Taken together, our results point to a policy approach to fighting inequality that evades top end inequality and instead provides compensation to the middle class while buffering the most visible downsides of low-end-inequality via predistribution. To exaggerate only mildly, our results point to a political strategy that embraces everyone rhetorically, ameliorates market shocks, harms no one fiscally, and costs relatively little. We label this pattern *selective egalitarianism*. Whether it qualifies as conscious strategy is an open question.

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SUPPLEMENTARY MATERIAL

The equality positions and policies of governments, 1970-2020

How egalitarian parties pursue equality yet evade redistribution

Alexander Horn

University of Konstanz
alexander.horn@uni-konstanz.de

Leo Ahrens

University of Konstanz
leo.ahrens@uni-konstanz.de

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A Descriptive statistics

Table 1: Summary statistics

| Variable | Countries | Years | N | Mean | Median | SD | Min | Max |
|-------------------------------|-----------|-----------|-----|-------|--------|-------|-------|-------|
| Egalitarian position | 12 | 1970-2019 | 537 | -3.78 | -3.78 | 0.70 | -5.96 | -1.78 |
| Minimum wage | 8 | 1972-2019 | 251 | -0 | -0 | 2.01 | -3.76 | 4.43 |
| Unemployment generosity score | 12 | 1970-2019 | 535 | 0 | 0 | 1.88 | -3.23 | 4.49 |
| Top marginal income tax rate | 12 | 1970-2019 | 537 | 0 | 0 | 1.39 | -3.12 | 4.49 |
| GDP growth | 12 | 1980-2007 | 537 | 2.56 | 2.56 | 2.21 | -4.24 | 9.15 |
| Log GDP p.c. | 12 | 1980-2007 | 537 | 10.4 | 10.4 | 0.38 | 9.33 | 11.5 |
| % industry employment | 12 | 1970-2019 | 537 | 0.28 | 0.28 | 0.085 | 0.05 | 0.64 |
| Inflation | 12 | 1970-2019 | 537 | 4.12 | 4.12 | 4.02 | -0.48 | 17.9 |
| Unemployment | 12 | 1970-2019 | 537 | 6.41 | 6.41 | 2.96 | 0.10 | 16.8 |
| % elderly | 12 | 1970-2019 | 537 | 14.4 | 14.4 | 2.89 | 7.62 | 21.6 |
| Capital account openness | 12 | 1970-2019 | 537 | 0.87 | 0.87 | 0.23 | 0 | 1 |

B Supplementary information on crowdcoding

Pre-processing, segmentation and classification of manifesto statements

The raw text of parties' manifestos hardly lends itself to be analyzed for conceptions of equality without further pre-processing. Rather than the entire text – and much in the spirit of the MARPOR (Volkens *et al.* 2020) – our data elicitation instead zooms in on quasi-sentences as the unit bearing semantic meaning. Ideally, each of those quasi-sentences only contains one relevant statement regarding policy. Unfortunately, the data retrieved from MARPOR is only pre-segmented in quasi-sentences from 1998 onwards, which means the other manifestos must still be segmented. Ideally, this segmentation retains the underlying logic of quasi-sentences developed by the MARPOR. To this end, we developed an automated segmenter that was trained on two-thirds of the pre-segmented party manifestos available from the MARPOR. Essentially, the segmenter is a binary classifier (logistic regression) that was trained using the popular Python-library scikit-learn. It learns to predict whether a given word is the beginning of a new statement, based on the lemmatized surrounding words, punctuation, and the shape of the lemmatized focal word. Using this information, the segmenter can splice a long raw string of text into quasi-sentences that adhere to the MARPOR logic of quasi-sentences. Subsequently, we used a regular expression to identify quasi-sentences, that might have been mis-segmented. These can either be statements that have been incorrectly split or statements that the classifier failed to split into separate statements. In a final step, those flagged quasi-sentences were forwarded to research assistants to check and, if necessary, revise the segmented quasi-sentences. This process essentially segmented all party manifestos into 850,000 quasi-sentences.

While those quasi-sentences could, in principle, be forwarded to the crowd, this would both unduly increase the workload on crowdcoders and raise the costs of content-coding parties' equality conceptions. Therefore, the next step consists of identifying statements that concern equality. The central idea of this step is to discard obviously irrelevant statements to lower the workload for and, hence, costs of human coders. For this task, we again capitalize on the fact that MARPOR supplies quasi-sentence level data that has already been content-coded. Using this information, for each country we trained a logistic regression classifier on quasi-sentences that pertain to category 503 (labeled by the MARPOR as "Equality: Positive"), which contains statements describing the "concept of social justice and the need for fair treatment of all people". Importantly, the hyper-parameters of the classifier were fine-tuned to reliably identify the vast majority of relevant statements at the expense of many false positives, i.e., we trade off precision for recall. Generally, our trained classifiers achieve a minimum recall of 0.80, meaning they successfully retain at least 80% of all potentially relevant statements about equality. Using these classifiers, we flagged each statement as either relevant or irrelevant with regard to equality. All statements flagged as relevant were subsequently forwarded

to trained research assistants who checked the automated decision. This reduction process left us with about 55,451 quasi-sentences that speak to equality and social justice.

Additional information on crowdcoding: process, ethical considerations, remuneration

a) Process

All coders had to pass an entry test of ten units taken from a country's manifestos. All test units have been unanimously coded by three experts (post-doctoral researchers) to assure that they are neither too challenging to exclude 'sincere' coders, nor too obvious to enable spammers (workers seeking to maximize profits through random responses) or unqualified workers (that do not meet minimum quality criteria) to contribute data. As there are six answers to each coding unit (five inequality concepts and "NA" for units that do not deal with the topic), we settle on a minimum of 60% agreement thresholds according to country-specific pre-tests (accordingly, the odds of passing by guessing are below 0.002%). Moreover, given smaller crowd sizes for the two languages Danish and Swedish, we relied on machine translation via GoogleTranslate to access the larger English-speaking crowd. As additional quality check, we add five percent of expert-coded units to all our tasks. This enables us to monitor and validate coder performance during the task and assures that qualified coders maintain their accuracy. Based on extensive pre-tests, we collect five coding decisions per unit. This number guarantees that aggregated results can match expert ratings and at the same time, ensures fast data collection at reasonable costs.

b) Ethical considerations and fair remuneration

On April 4, 2020, at the 2020 Spring Council Meeting, the APSA Council approved and adopted the "Principles and Guidance for Human Subjects Research". We will now discuss the coding of equality statements in light of some potentially relevant aspects touched upon in the principles. In particular, we discuss deception, consent, the danger of trauma, coder satisfaction, and (the fairness of) remuneration.

Unless involving survey experiments or surveys, our institution deems online crowdcoding similar to classic coding by research assistants, which was confirmed by the data officer at our institution, who therefore issued an "ethics waiver" (which we uploaded to the submission platform as "other" material).

As described in SI A, we did not run a survey or experiment via Mechanical Turk, but conducted a very transparent coding task that did not involve any type of deception of the coders. The title of the jobs (advertised to coders) and the description of the task itself clearly communicated the actual aim of the study (i.e., to code/categorize equality statements) and the original statements of the parties were not changed. We informed the coders that we are from the research project XXXX, they voluntarily chose our job descriptions, and they could have exited the coding process at any point. Importantly, they would have still been paid for the work already done. Finally, since the coded text is extracted from party manifestos written to appeal to voters, inappropriate language is highly unlikely, and, while few older political platforms potentially contain formulations that may be considered offensive today, we did not identify any potentially traumatizing passages. In sum, we conclude that no information that would have led to coders withholding consent was kept from them.

Since we also conducted pre-tests with other platforms such as Appen (previously Crowdfunder), some of which provide the possibility to evaluate tasks and comment on them, we were able to respond to all questions and to minimize potential sources of frustration (usually, lack of task clarity). In line with previous assessments on the satisfaction of crowdcoders (Horn 2019), users were satisfied. The confidentiality of all coders was maintained at all stages of the data gathering process and afterwards.

To warrant fair remuneration, we made sure our workers were able to attain local minimum wages based on extensive pre-tests assessing the average coding duration per unit. Table SI-A1 provides detailed information on coder pay. The last two rows indicate the pay per coding necessary to reach local minimum wages and the amount we payed. Finally, we pay significantly more per decision than the studies we cite (Benoit et al. 2016, Horn 2019, with a pay of 1 and 5 US-cent respectively) and the average duration used to calculate the wage levels was deemed very realistic by all of our student coders.

Table 2: Local minimum wages and coder pay

| Country | Wage (€) | Duration /unit (sec) | Estimate \$/decision | Payment \$/decision |
|--------------------------|----------|----------------------|----------------------|---------------------|
| Australia | 13.86 | 20 | 0.08 | 0.08 |
| Austria ⁺ | 9.07 | 20 | 0.05 | 0.08 |
| Canada [#] | 11.01 | 20 | 0.07 | 0.08 |
| Denmark ⁺ | 15.83 | 20 | 0.09 | 0.09 |
| France | 10.57 | 20 | 0.06 | 0.08 |
| Germany | 9.82 | 20 | 0.06 | 0.08 |
| Ireland | 10.50 | 20 | 0.06 | 0.08 |
| New Zealand | 13.24 | 20 | 0.08 | 0.08 |
| Sweden ⁺ | 10.34 | 20 | 0.06 | 0.10 |
| Switzerland [#] | 19.59 | 20 | 0.11 | 0.11 |
| United Kingdom | 11.45 | 20 | 0.06 | 0.08 |
| United States | 7.25 | 20 | 0.04 | 0.08 |

+ based on collective bargaining agreements for the service sector (countries without federal minimum wages)

based on averaging local/regional minimum wages

Validation

293 workers contributed to the full task and the modal worker contributed 180 units. To assess their quality, we compare their performance against the expert coding. We aggregate individual codings per unit based on majority vote. This approach reflects the general idea of the ‘wisdom of the crowd’ and is well anchored in social choice theory (Condorcet 2014 [1785]). Doing so, we obtain a Krippendorff’s alpha of 0.72 between the crowd’s judgment and the experts based on 2,904 coded units at the unit level, which exhibits very good agreement even by conventional standards of quantitative content analysis. Table SI-A2 shows that unit-level alpha was very good across all countries. These results confirm that crowds are able to replicate expert judgments of complex tasks.

Table 3: Unit-level agreement of expert coding and aggregated crowd (majority vote)

| Country | N (gold) | % agreement | Krippendorff’s α | % w/o majority |
|----------------|-------------|-------------|-------------------------|----------------|
| Australia | 171 | 75 | 0.66 | 7.60 |
| Austria | 447 | 76 | 0.69 | 7.16 |
| Canada | 144 | 79 | 0.69 | 11.81 |
| Denmark | 325 | 73 | 0.65 | 14.77 |
| France | 159 | 80 | 0.73 | 10.06 |
| Germany | 509 | 87 | 0.82 | 4.32 |
| Ireland | 271 | 74 | 0.66 | 12.18 |
| New Zealand | 195 | 71 | 0.60 | 10.26 |
| Sweden | 161 | 80 | 0.71 | 12.42 |
| Switzerland | 159 | 90 | 0.85 | 3.77 |
| United Kingdom | 203 | 79 | 0.71 | 9.36 |
| United States | 160 | 80 | 0.69 | 9.38 |
| <i>Overall</i> | <i>2904</i> | <i>79</i> | <i>0.72</i> | <i>8.99</i> |

English version of coding instructions (Amazon Mechanical Turk)

Does the statement include a positive reference to equality, social justice, and/or equal treatment of all people?

- If YES, which of the following categories (1 to 5) applies?
- If NONE apply, select category 6.
- If more than one category applies, decide which ONE you think is most clearly emphasized.
- Please read the six categories carefully!

1) Economic equality: In favor of more financial and economic equality. Examples: we criticize that ordinary citizens are doing poorly; economic inequality is on the rise in our country; we must redistribute more from the top to the bottom; the rich should bear a larger burden; society is drifting apart; we have to narrow the

gap between the rich and the poor; wealth tax now; we should share the (economic) benefits of globalization fairly; unevenly distributed wealth threatens social cohesion; mobility/energy/housing/health care must be affordable for all.

2) Equal chances/social mobility: equal education, economic, and social opportunities. Examples: good education for all, higher education should be (more) accessible for low- and middle-income students; a child's social background/economic status should not determine their future; everyone must be given a fair chance - regardless of their parents' income.

3) Equal Rights/anti-discrimination: Against discrimination based on disability, race, color, religion, sex, gender; LGBTQ friendly. Includes statements on equal pay for women (gender pay gap). Examples: we are the party of diversity and inclusion; we want a diverse society; we support a quota for women on supervisory boards.

4) Non-specific mention of equality, (social) justice, and solidarity: example: we are the party of equality (or equity); we stand for (more) solidarity; towards a more just society.

5) Other: A reference to equality, but the statement does not fit into any of the previous categories. Example: global justice now; more development aid/international aid; reduce regional inequality (e.g., in terms of mobility, internet, environment), no region(s) left behind, climate and environmental justice, intergenerational justice.

6) No: no positive reference. (e.g.: equality before the law, fair application of the rule of law, physical integrity, immigration/asylum, noise mitigation, or crime).

C Regression tables

Table 4: Minimum wages

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--|-------------------|--------------------|-----------------|--------------------|------------------|-------------------|
| Egalitarian position | 0.65*** (0.18) | 0.54*** (0.13) | -0.71 (3.20) | -0.43 (3.29) | 1.09* (0.48) | 1.11*** (0.31) |
| Egalitarian position * Voter income decile | | | 0.22 (0.54) | 0.16 (0.55) | | |
| Egalitarian pos. * % higher educ. voters | | | | | -2.70 (2.72) | -3.60* (1.89) |
| Voter income decile | | | 0.39 (1.95) | 0.15 (1.93) | | |
| % higher education voters | | | | | -11.1 (9.68) | -13.5* (5.82) |
| GDP growth | | 0.00067 (0.043) | | -0.0039 (0.036) | | 0.0067 (0.035) |
| Log GDP p.c. | | -0.38 (1.76) | | -0.35 (1.63) | | -0.075 (1.70) |
| % industry employment | | -3.37 (7.51) | | -6.15 (8.16) | | -1.56 (7.22) |
| Inflation | | -0.030 (0.042) | | -0.021 (0.026) | | -0.020 (0.038) |
| Unemployment | | -0.0036 (0.094) | | 0.0094 (0.092) | | 0.024 (0.086) |
| % elderly | | 0.16 (0.13) | | 0.18 (0.14) | | 0.24* (0.10) |
| Capital account openness | | 1.02** (0.38) | | 0.52 (0.40) | | 0.89 (0.55) |
| Constant | 4.21*** (0.73) | 6.77 (18.9) | 1.58 (11.5) | 6.28 (23.7) | 5.99** (2.03) | 4.43 (18.8) |
| Observations | 247 | 247 | 247 | 247 | 247 | 247 |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes |

Note: Standard errors clustered by countries in parentheses (estimated with wild cluster bootstrap using 10,000 repetitions and Rade-macher weights). Two-tailed tests, *<.1, **<.05, ***<.001).

Table 5: Unemployment benefit generosity

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--|----------------|---------------------|------------------|--------------------|------------------|---------------------|
| Egalitarian position | 0.28 (0.16) | 0.30** (0.12) | 2.51** (1.14) | 2.71*** (0.61) | 0.27 (0.26) | 0.36 (0.22) |
| Egalitarian position * Voter income decile | | | -0.39* (0.20) | -0.42*** (0.12) | | |
| Egalitarian pos. * % higher educ. voters | | | | | -0.017 (1.73) | -0.45 (1.93) |
| Voter income decile | | | -1.53 (0.94) | -1.67** (0.59) | | |
| % higher education voters | | | | | 1.95 (6.20) | -0.59 (6.76) |
| GDP growth | | -0.019 (0.022) | | -0.022 (0.019) | | -0.015 (0.025) |
| Log GDP p.c. | | 2.55** (0.84) | | 2.65*** (0.78) | | 2.42** (0.93) |
| % industry employment | | 9.62** (4.02) | | 9.11** (3.50) | | 9.74** (3.96) |
| Inflation | | 0.087*** (0.024) | | 0.077** (0.026) | | 0.088*** (0.025) |
| Unemployment | | 0.0070 (0.056) | | -0.0040 (0.052) | | 0.0068 (0.058) |
| % elderly | | 0.11 (0.13) | | 0.12 (0.13) | | 0.10 (0.13) |
| Capital account openness | | -1.06 (1.07) | | -1.15 (1.11) | | -0.98 (1.14) |
| Constant | 0.35 (0.85) | -30.0*** (9.43) | 9.19 (5.39) | -21.0* (9.85) | 0.20 (1.26) | -28.6** (10.2) |
| Observations | 525 | 525 | 524 | 524 | 524 | 524 |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes |

Note: Standard errors clustered by countries in parentheses (estimated with wild cluster bootstrap using 10,000 repetitions and Rade-macher weights). Two-tailed tests, *<.1, **<.05, ***<.001).

SUPPLEMENTARY MATERIAL

Table 6: Top marginal income tax rate

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--|------------------|---------------------|------------------|---------------------|-----------------|----------------------|
| Egalitarian position | 0.033 (0.096) | 0.023 (0.071) | 0.73 (0.47) | 0.56 (0.44) | 0.10 (0.20) | -0.030 (0.13) |
| Egalitarian position * Voter income decile | | | -0.13 (0.089) | -0.098 (0.079) | | |
| Egalitarian pos. * % higher educ. voters | | | | | -0.62 (0.88) | 0.26 (0.55) |
| Voter income decile | | | -0.56 (0.35) | -0.48 (0.27) | | |
| % higher education voters | | | | | -2.79 (3.53) | 0.65 (2.62) |
| GDP growth | | -0.0023 (0.0090) | | -0.0013 (0.0072) | | -0.00011 (0.0098) |
| Log GDP p.c. | | -0.29 (0.53) | | -0.33 (0.48) | | -0.35 (0.62) |
| % industry employment | | 0.78 (2.64) | | 0.39 (2.50) | | 0.50 (2.72) |
| Inflation | | 0.037 (0.023) | | 0.031 (0.022) | | 0.034 (0.021) |
| Unemployment | | 0.011 (0.050) | | 0.0068 (0.050) | | 0.0091 (0.049) |
| % elderly | | -0.036 (0.067) | | -0.032 (0.072) | | -0.035 (0.068) |
| Capital account openness | | -0.81 (0.67) | | -0.86 (0.65) | | -0.85 (0.66) |
| Constant | 1.63** (0.57) | 4.73 (5.50) | 4.80** (1.82) | 7.90 (5.85) | 1.95* (0.90) | 5.31 (6.10) |
| Observations | 525 | 525 | 524 | 524 | 524 | 524 |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes |

Note: Standard errors clustered by countries in parentheses (estimated with wild cluster bootstrap using 10,000 repetitions and Rade-macher weights). Two-tailed tests, *<.1, **<.05, ***<.001).

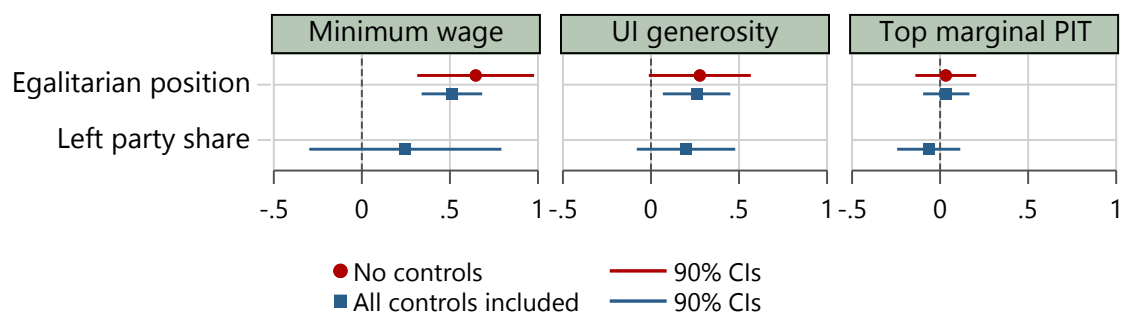
Table 7: Effect heterogeneity between right majority, mixed, and left majority cabinets

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--------------------------------------|-------------------|--------------------|------------------|---------------------|--------------------|---------------------|
| Egalitarian position | 0.62** (0.18) | 0.55*** (0.13) | 0.19 (0.15) | 0.24** (0.089) | -0.0022 (0.081) | -0.020 (0.054) |
| Egalitarian position * Mixed cabinet | -0.098 (0.21) | -0.36 (0.39) | -0.36 (0.34) | -0.49 (0.35) | -0.12 (0.22) | -0.15 (0.20) |
| Egalitarian position * Left majority | 0.0084 (0.35) | -0.029 (0.22) | 0.38** (0.14) | 0.31** (0.12) | 0.22* (0.12) | 0.23** (0.099) |
| Mixed cabinet (rel.: Right majority) | -0.44 (0.79) | -1.45 (1.40) | -1.02 (1.26) | -1.52 (1.27) | -0.40 (0.85) | -0.52 (0.78) |
| Left majority (rel.: Right majority) | 0.30 (1.37) | 0.078 (0.83) | 1.51** (0.55) | 1.33** (0.45) | 0.76 (0.47) | 0.85* (0.39) |
| GDP growth | | -0.0027 (0.043) | | -0.022 (0.023) | | -0.0044 (0.0096) |
| Log GDP p.c. | | -0.46 (1.64) | | 2.68*** (0.73) | | -0.25 (0.48) |
| % industry employment | | -3.94 (8.43) | | 9.53** (3.81) | | 0.52 (2.38) |
| Inflation | | -0.032 (0.044) | | 0.086*** (0.025) | | 0.034 (0.020) |
| Unemployment | | -0.011 (0.097) | | 0.0098 (0.061) | | 0.0070 (0.046) |
| % elderly | | 0.17 (0.14) | | 0.11 (0.13) | | -0.031 (0.070) |
| Capital account openness | | 0.99** (0.35) | | -1.24 (1.00) | | -0.88 (0.62) |
| Constant | 4.03*** (0.81) | 7.77 (18.3) | -0.076 (0.79) | -31.3*** (9.01) | 1.53** (0.51) | 4.23 (5.06) |
| Observations | 247 | 247 | 525 | 525 | 525 | 525 |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes |

Note: Standard errors clustered by countries in parentheses (estimated with wild cluster bootstrap using 10,000 repetitions and Rademacher weights). Two-tailed tests, * $<.1$, ** $<.05$, *** $<.001$).

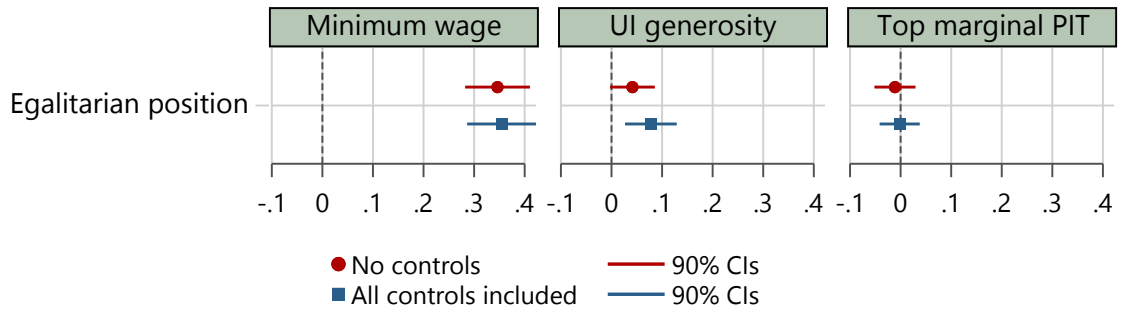
D Results of further analyses

Figure 1: Share of left parties in government added as control



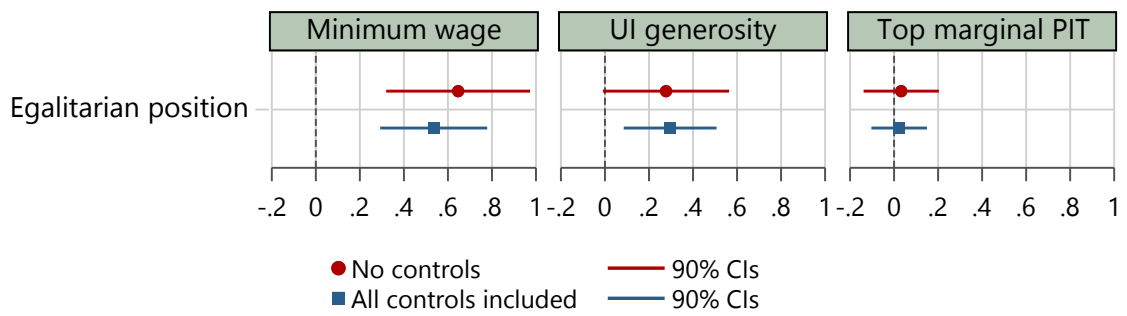
Note: The standard errors are clustered by countries (estimated with wild cluster bootstrap using 10,000 repetitions and Rademacher weights).

Figure 2: Model with Prais-Winsten one-year lag autocorrelation and PCSE



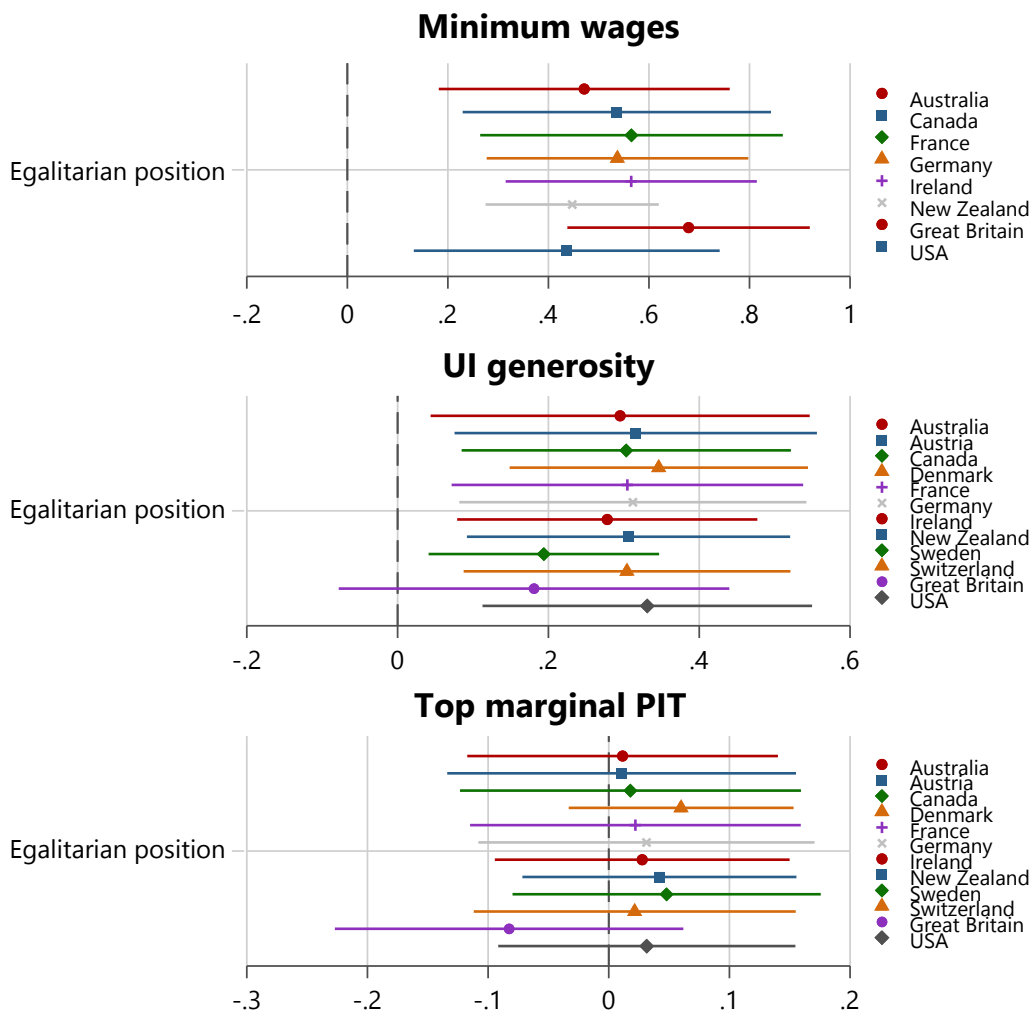
Note: Panel-corrected standard errors (PCSE).

Figure 3: (Standard) clustered standard errors



Note: The standard errors are clustered by countries (estimated with the standard adaption of the Huber/White sandwich estimator, which is likely biased due to the small country-level sample size).

Figure 4: Jackknife robustness test



Note: The robustness test replicates all main models, dropping one country at a time from the estimation samples. What country is dropped in the respective models is displayed in the legend. Every displayed coefficient is estimated from a different regression model. The standard errors are clustered by countries (estimated with wild cluster bootstrap using 10,000 repetitions and Rademacher weights).

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