The (a)symmetric effects of income and unemployment on popular demand for redistribution[1]

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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.

Keywords: inequality; public opinion; political economy; self-interest; 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 emphasise 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.

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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 2021). 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 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 characterised 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 optimise 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 2021; 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).[2] 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)

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

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).

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 redistribution preferences 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 2021), 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 that 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 analyse 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 optimise 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 optimisation: 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 decreasing 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.[3] 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 labour 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 theorised 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).

[3] This blow may be softened by the market income generated by other household members.

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,[4] and the explanatory variable is binary (e.g., unemployment vs. employment) and only varies in the second period (e.g., all individuals are 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:

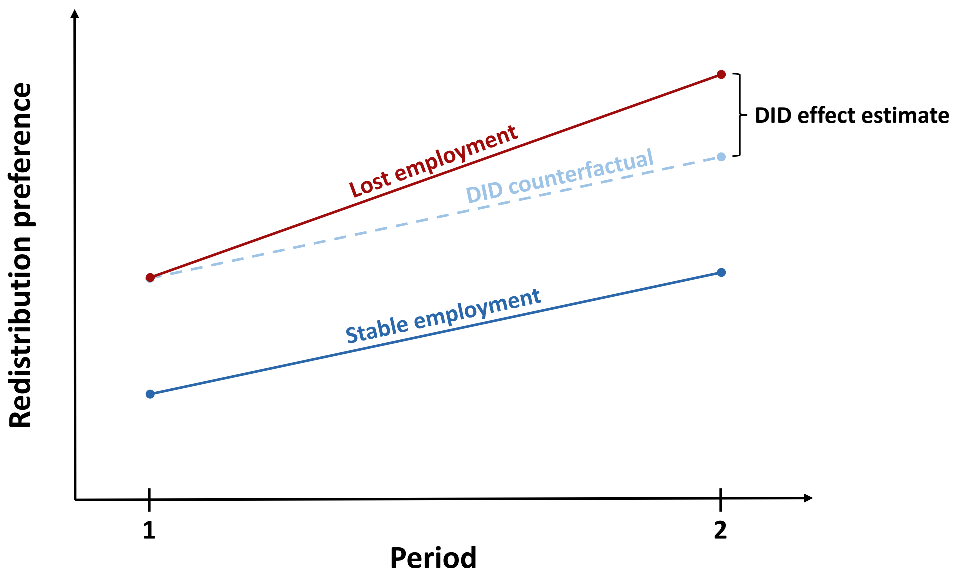
$$y_{i,t} = \alpha_i + \gamma_t + ß_1 T_i * \gamma_t + ß_k Z_{i,t} + \epsilon_{i,t} \qquad (1)$$

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 $e_{i,t}$ the error term. The parameter of substantive interest is $ß_1$, which indicates an average treatment effect on the treated (ATT) (Lechner 2010).

Figure 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 1.

[4] 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 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.

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.[5]

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).

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):

$$y_{i,t} = \alpha_i + \gamma_t + ß_1 X_{i,t} + ß_k Z_{i,t} + \epsilon_{i,t} \qquad (2)$$

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 1 (Brüderl and Ludwig 2015). It must be

[5] 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.

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.[6] 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.[7]

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 analysed 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.

DATA AND MODEL SETUP

This section outlines 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

[6] 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.

[7] 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).

contain all required variables[8], namely waves 1, 10, 11, 12, and 14.[9] 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.[10] 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 A1-A3 in the online appendix 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).[11] 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 income is shared within households.[12] 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.

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[8] 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.

[9] 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.

[10] 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.

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

[12] 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.

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.[13] 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 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 2020). 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.

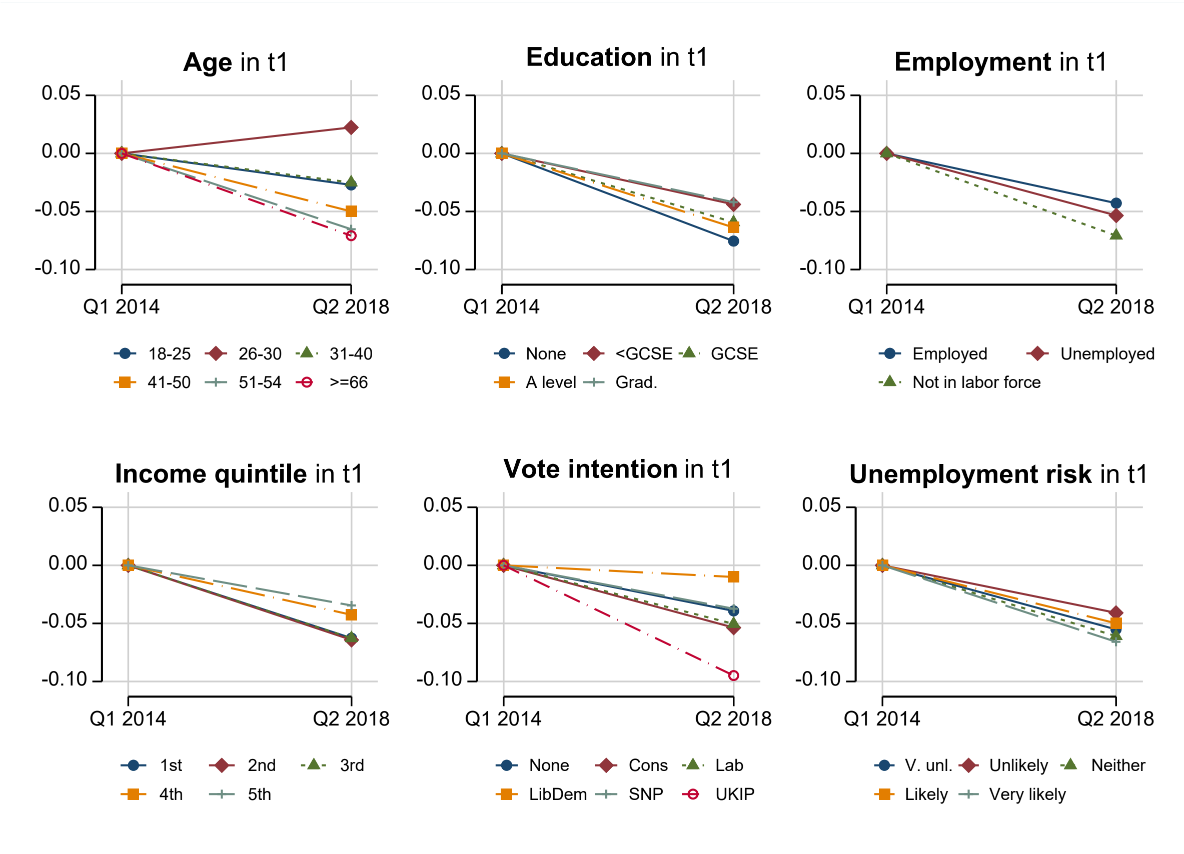
The matching weights for the DID regressions are estimated using pre-treatment (i.e. wave 1) values of the following characteristics: age, gender, education, income, employment situation, and perceived unemployment risk. In the assessment of the effect of income, it is also possible to match on respondents' 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 DID analyses is available in Table A4 of the online appendix. Furthermore, Figures A1-A6 of the online appendix compare the pre-matching distributions of included variables between the different treatment and control groups. They illustrate the necessity of

[13] 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.

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 2020; Shayo 2009).

FIGURE 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.

Figure 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.[14] 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

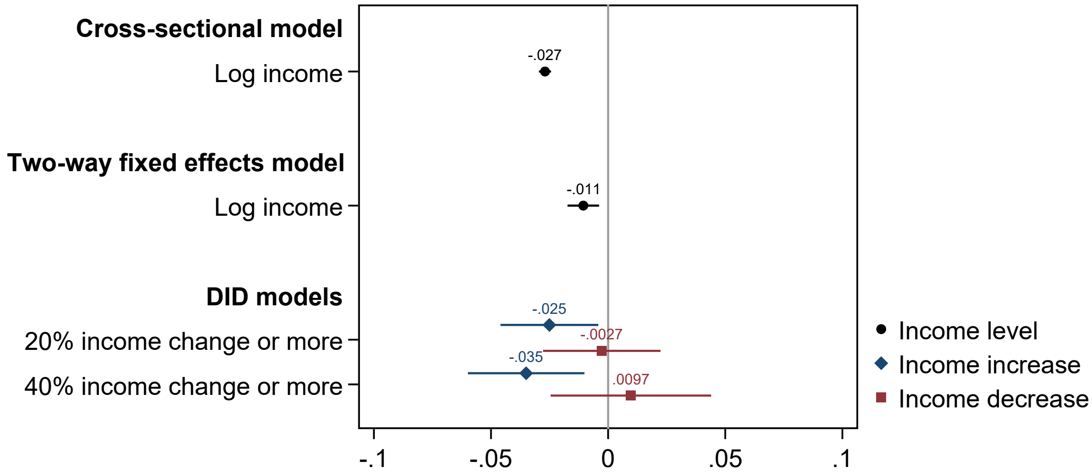
Figure 3 presents the results of regressions using income as the explanatory variable (full regression results are available in the online appendix). The cross-section and TWFE model use income in logged form due to its skewed distribution. The coefficients are rescaled so that they indicate the effect of increasing one's income by 50% to roughly bring them on the same scale as the DID coefficients.[15]

Figure 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.

[14] 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).

[15] 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: 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]

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.

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 the online appendix). 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 *equivalised* household income are used to define the treatment and control groups.[16] The treatment groups are given by those who increased/decreased their equivalised household income by 20% and 40% or more. Equivalization was not applied in the

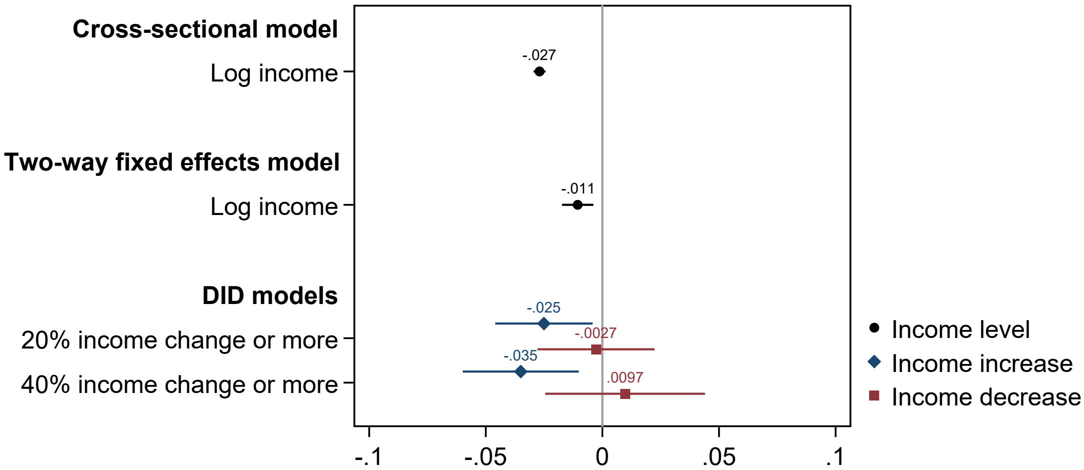
[16] Equivalence income scales aim to facilitate the comparison of consumption potential between households with different composition because households with more members benefit less from a given income amount than households with less members. Equivalization is applied by dividing household income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

main specifications because household size does not necessarily affect the self-interest calculus regarding redistribution.[17] 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 included. Nevertheless, the results of the main specification replicate.[18] 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 4 presents the results of regressions using unemployment as the explanatory variable (full regression results are available in the online appendix). 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.

FIGURE 4: The effect of unemployment 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]

[17] 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.

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

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 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 analysing 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 2021; 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 characterise 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 theorised mechanisms explain the asymmetry in the effect of income. Under the assumption that negativity bias drives asymmetry, it can be theorised 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 analysed 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 analysed panel waves were temporally closer.

Lastly, the generalisability 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.[19] Furthermore, the generalizability across countries is also unclear. The analysed 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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[19] 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 A1-A6 in the online appendix 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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Online Appendix

to

The (a)symmetric effect of income and unemployment on popular demand for redistribution

Leo Ahrens

West European Politics

Table A1: 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: 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 A3: 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 A4: 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-A6

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 A1: Distribution of matching variables before matching – 20% income gain analysis

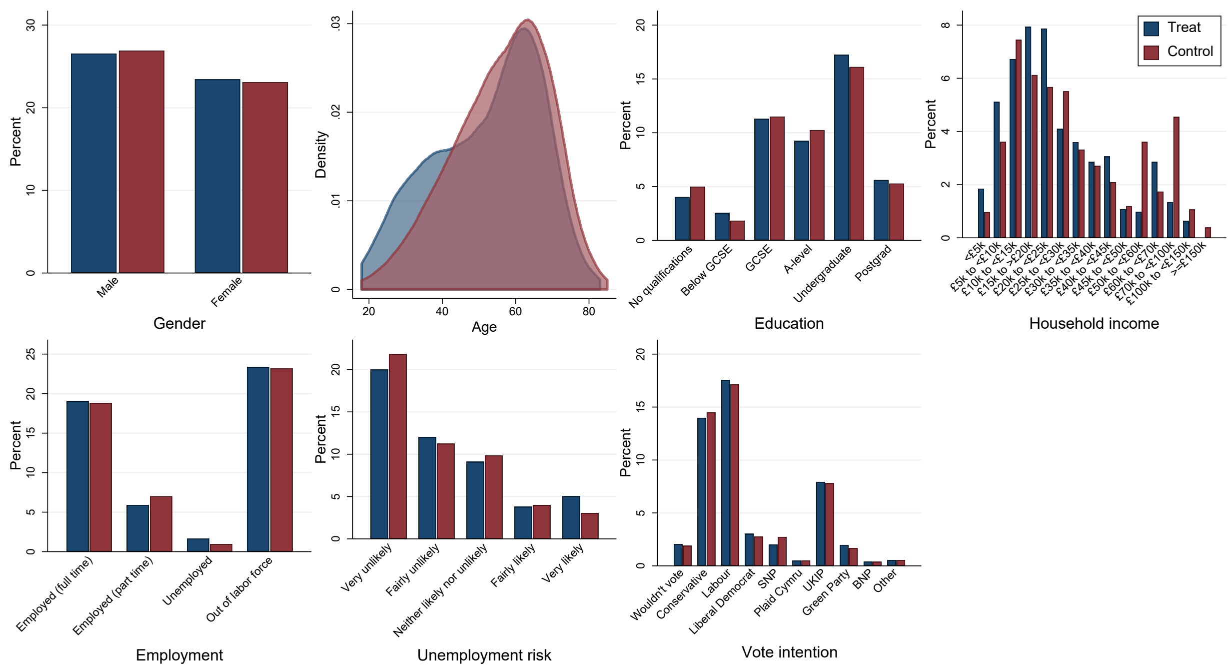
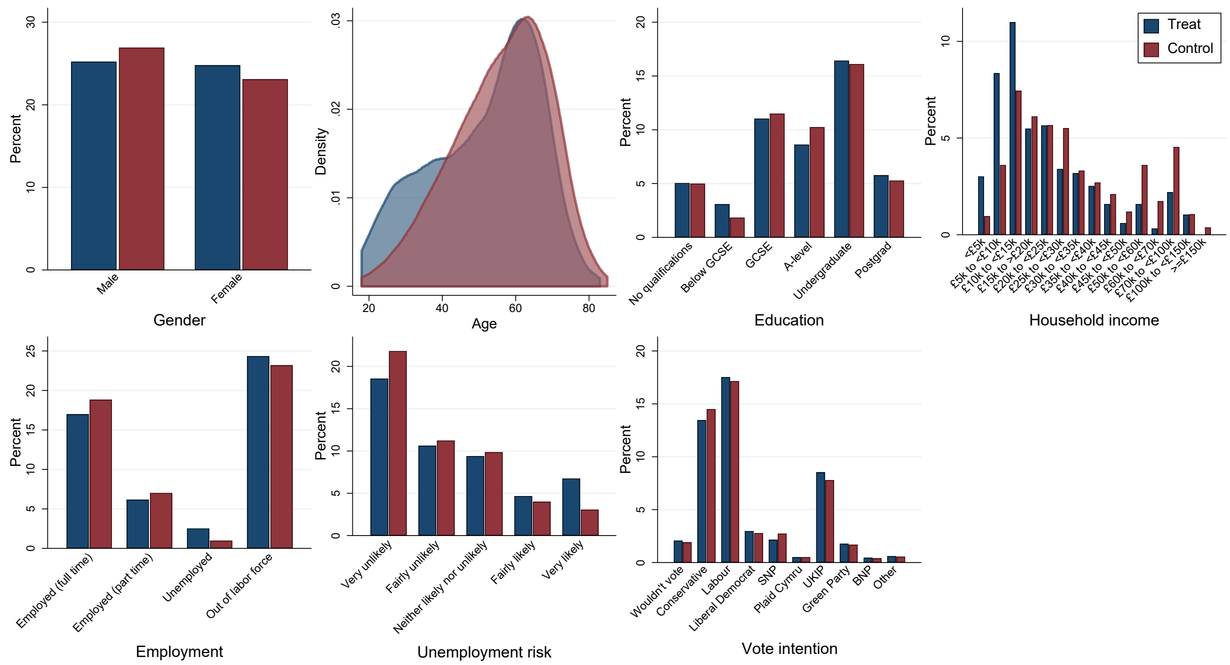


Figure A2: Distribution of matching variables before matching – 40% income gain analysis



Note: The figure shows the distribution of the variables used in the matching procedure within the 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 A3: Distribution of matching variables before matching – 20% income loss analysis

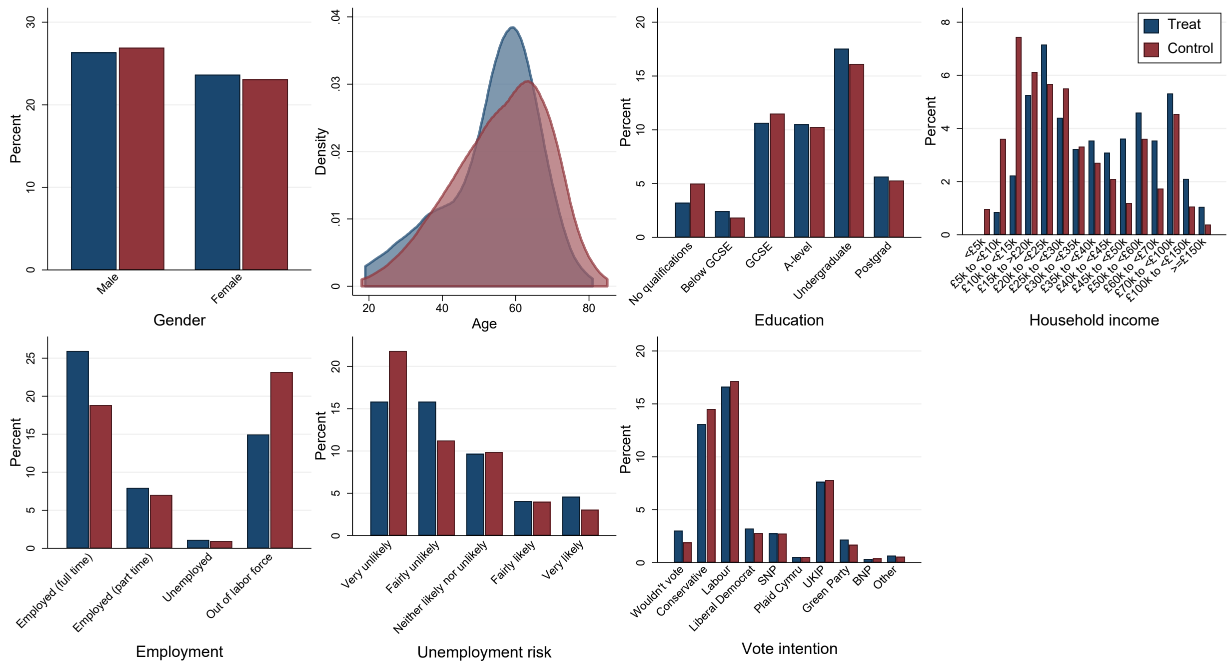


Figure A4: Distribution of matching variables before matching – 40% income loss analysis

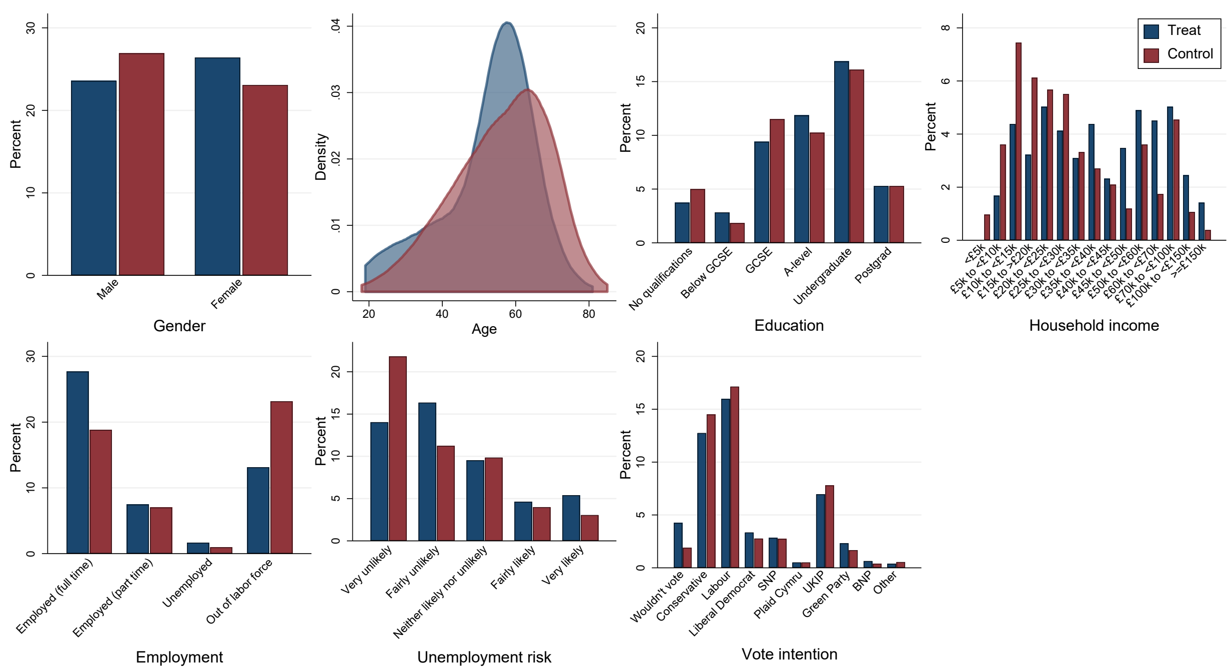


Figure A5: Distribution of matching variables before matching – losing employment analysis

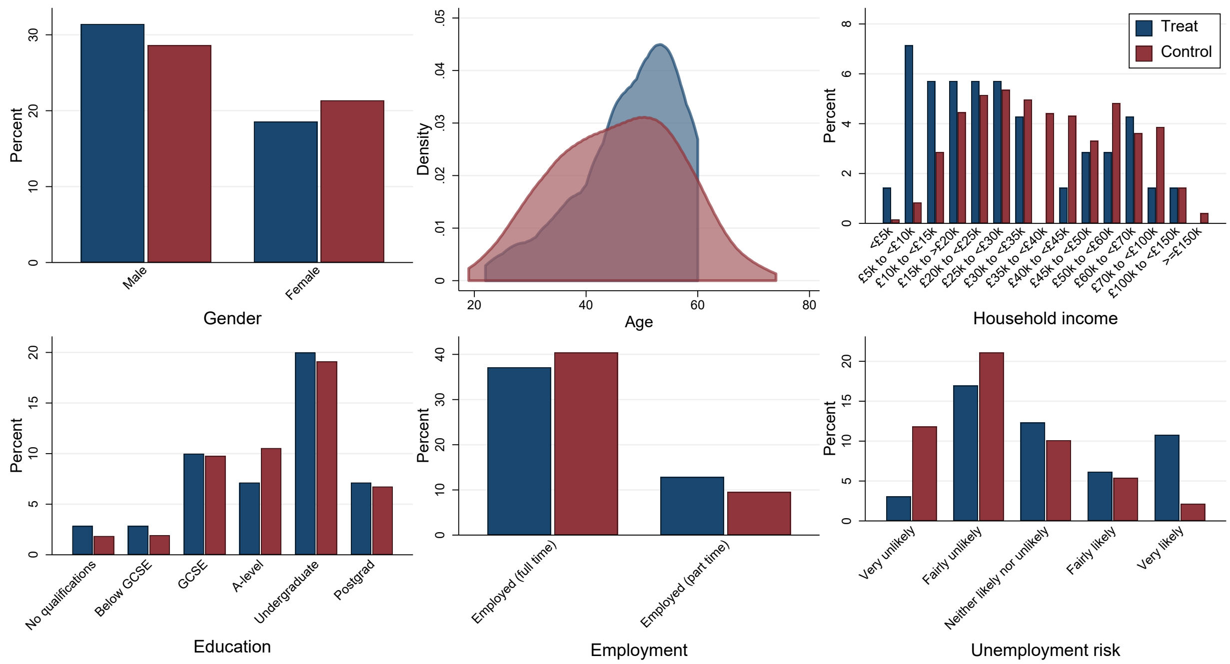


Figure A6: Distribution of matching variables before matching – gaining employment analysis

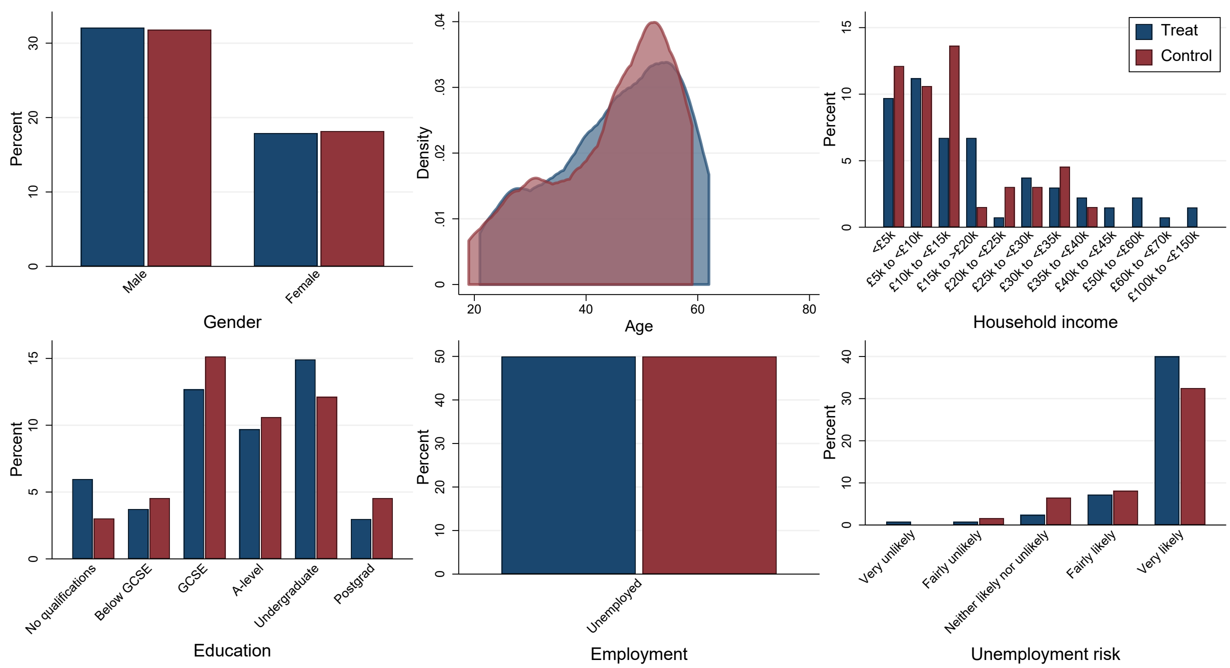


Table A5: Income and redistribution support (cross-section and FE/TWFE models)

	Cross-section	TWFE
Log income	-0.03***	-0.01***
	(0.00)	(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***	0.00
	(0.01)	(0.03)
GCSE	-0.04***	0.03
	(0.01)	(0.03)
A-level	-0.04***	0.03
	(0.01)	(0.03)
Undergraduate	-0.01	0.02
	(0.01)	(0.03)
Postgrad	0.03***	0.08**
	(0.01)	(0.04)
Employment (ref.: employed (full time))		
Employed (part time)	-0.01	-0.02
	(0.01)	(0.01)
Unemployed	-0.01	-0.00
	(0.01)	(0.03)
Out of labor force	0.01	-0.03***
	(0.01)	(0.01)
Unemployment risk (ref.: very unlikely)		
Fairly unlikely	0.03***	0.00
	(0.01)	(0.01)
Neither likely nor unlikely	0.06***	0.00
	(0.01)	(0.01)
Fairly likely	0.08***	0.02*
	(0.01)	(0.01)
Very likely	0.10***	0.04***
	(0.01)	(0.01)
Constant	1.11***	0.87***
	(0.04)	(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.

Table A6: 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***	-0.04***	-0.04***	-0.04***
	(0.01)	(0.01)	(0.01)	(0.01)
Education (ref.: no education)				
Below GCSE	-0.03	0.05	-0.03	0.05
	(0.04)	(0.04)	(0.05)	(0.06)
GCSE	-0.00	0.08**	-0.02	0.09**
	(0.04)	(0.03)	(0.05)	(0.05)
A-level	-0.01	0.06	-0.04	0.05
	(0.05)	(0.04)	(0.06)	(0.05)
Undergraduate	-0.01	-0.02	-0.03	-0.03
	(0.05)	(0.05)	(0.06)	(0.07)
Postgrad	0.07	0.09	0.01	0.10
	(0.06)	(0.06)	(0.07)	(0.09)
Employment (ref.: employed (full time))				
Employed (part time)	-0.01	0.00	-0.02	0.02
	(0.02)	(0.02)	(0.02)	(0.02)
Unemployed	-0.01	0.03	-0.01	0.05
	(0.04)	(0.05)	(0.04)	(0.05)
Out of labor force	-0.03	-0.00	-0.04*	0.01
	(0.02)	(0.02)	(0.02)	(0.02)
Unemployment risk (ref.: very unlikely)				
Fairly unlikely	-0.01	0.00	-0.01	0.02
	(0.01)	(0.01)	(0.01)	(0.02)
Neither likely nor unlikely	-0.00	0.01	0.00	0.02
	(0.01)	(0.01)	(0.01)	(0.02)
Fairly likely	0.01	0.02	0.02	0.04*
	(0.02)	(0.02)	(0.03)	(0.03)
Very likely	0.05**	0.02	0.05**	0.00
	(0.02)	(0.02)	(0.02)	(0.03)
Constant	0.65***	0.59***	0.70***	0.58***
	(0.04)	(0.04)	(0.05)	(0.05)
Observations	6,076	4,624	4,922	3,878

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

Table A7: 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***	-0.04***	-0.04***	-0.03**
	(0.01)	(0.01)	(0.01)	(0.02)
Education (ref.: no education)				
Below GCSE	0.00	0.09*	-0.02	0.05
	(0.05)	(0.05)	(0.10)	(0.09)
GCSE	0.02	0.10**	0.03	0.07
	(0.05)	(0.04)	(0.08)	(0.07)
A-level	-0.01	0.09*	-0.04	0.06
	(0.05)	(0.05)	(0.08)	(0.08)
Undergraduate	-0.03	-0.04	-0.11	-0.12
	(0.06)	(0.07)	(0.09)	(0.10)
Postgrad	0.04	0.08	-0.02	0.04
	(0.07)	(0.08)	(0.10)	(0.11)
Employment (ref.: employed (full time))				
Employed (part time)	0.01	-0.01	0.03	-0.00
	(0.02)	(0.02)	(0.03)	(0.03)
Unemployed	0.03	0.04	0.01	0.09
	(0.05)	(0.06)	(0.06)	(0.09)
Out of labor force	-0.02	-0.01	-0.02	-0.00
	(0.02)	(0.02)	(0.03)	(0.03)
Unemployment risk (ref.: very unlikely)				
Fairly unlikely	-0.02	-0.00	-0.05**	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)
Neither likely nor unlikely	-0.02	0.01	-0.03	0.02
	(0.01)	(0.02)	(0.02)	(0.02)
Fairly likely	0.01	0.01	0.01	-0.02
	(0.02)	(0.03)	(0.03)	(0.04)
Very likely	0.03	-0.00	-0.00	-0.05
	(0.03)	(0.03)	(0.04)	(0.04)
Constant	0.63***	0.57***	0.70***	0.59***
	(0.05)	(0.05)	(0.08)	(0.08)
Observations	5,210	4,344	3,938	3,406

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

Table A8: Income robustness check: DID models using wave 1 & 10 data and equivalized income

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

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

Table A9: Unemployment and redistribution support (cross-section and FE/TWFE models)

	Cross-section	TWFE
Unemployed (ref.: employed)	0.09***	0.03
	(0.01)	(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**	0.05
	(0.02)	(0.04)
GCSE	-0.05***	0.04
	(0.01)	(0.03)
A-level	-0.06***	0.06
	(0.01)	(0.04)
Undergraduate	-0.05***	0.03
	(0.01)	(0.04)
Postgrad	-0.03**	0.04
	(0.01)	(0.05)
Constant	0.52***	0.58***
	(0.03)	(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 A10: 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***	0.06
	(0.01)	(0.04)
Education (ref.: no education)		
Below GCSE	0.04	0.13
	(0.03)	(0.16)
GCSE	0.05	0.08
	(0.05)	(0.14)
A-level	0.05	-0.01
	(0.06)	(0.14)
Undergraduate	0.19***	-0.19***
	(0.07)	(0.04)
Postgrad	0.20***	-0.25***
	(0.07)	(0.05)
Constant	0.53***	0.78***
	(0.06)	(0.07)
Observations	4,294	200

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