Political Trust and Redistribution Preferences

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Abstract

How does political trust influence policy preferences? A large literature posits that trust is vital for supporting governments in managing fundamental societal challenges and investing in long-term policy making. This paper investigates the relationship between political trust and policy preferences, specifically redistribution preferences. Through four pre-registered, original survey experiments conducted over two years in the UK and long-term panel data spanning 19 years in Switzerland, I demonstrate that political trust has an insignificant and negligible impact on individuals' preferences for redistribution, even when trust is experimentally manipulated under theoretically favourable conditions. By combining two designs with improved causal identification than the existing literature, these results challenge prevailing theories linking political trust and policy preferences and highlight the need for further examination of the complex dynamics between citizens' attitudes and support for government policy.

Accepted for publication in the *Journal of European Public Policy*. Please cite the published version.

Governments around the world are tasked with tackling fundamental societal challenges: climate change, rising social care costs, mass migration and, most recently, a pandemic, amongst many others. Dealing with these challenges requires policy action which is often costly, imposes limitations on individuals, and involves short-term costs to address often very long-term problems. Addressing these problems in democratic societies involves the public allowing - even supporting - governments to impose some costs on them in the pursuit of uncertain solutions. This, of course, is not an easy task, and understanding the factors that help or hinder such support is fundamentally important.

Public trust in governing institutions and actors is a long-proposed reason citizens accept or reject such solutions (e.g Easton, 1975; Miller, 1974; van der Meer & Zmerli, 2017). It is not just academics that posit this, as important as they may be: just before the 2024 General Election, soon-to-be Labour Prime Minister Keir Starmer argued that low trust was 'a strategy to sow disillusion; to convince people that things can't get better, government can't improve people's lives' [1] Such beliefs have direct policy consequences, such that the Organisation for Economic Co-operation and Development (OECD) advocates trust-building as a policy tool to deal with major governance challenges, from climate change to public investment policies (Brezzi et al., 2021), leading to trust-building being a core component of governance in OECD countries (Bouckaert, 2012).

Understanding whether and how political trust matters for policy action is therefore important, particularly in the context of mounting policy challenges and in declining or stagnating trust in most democracies (for overviews, see Carstens, 2023; Citrin & Stoker, 2018; Dalton, 2004; Zmerli & Hooghe, 2011; Zmerli & van der Meer, 2017). Building on the theory that political trust acts as a heuristic that increases policy support (Hetherington & Husser, 2012; Rudolph et al., 2017) as well as the literature on long-term policy making (Jacobs & Matthews, 2012, 2017), this paper studies how trust affects policy preferences, using the case of redistribution preferences, a policy area which involves leveraging costs (in the form of taxation) for uncertain and unevenly distributed future

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[1]Speech available at: https://www.theguardian.com/politics/2022/feb/03/pms-disregard-for-rules-is-damaging-democracy-says-keir-starmer.

benefits (Rueda & Stegmueller, 2019). The specific preferences studied in this paper include those that vary in their temporal return, level of redistribution, and the extent to which expected costs and benefits vary across different types of respondents (such as older respondents benefiting more immediately from greater spending on pensions).

I test the relationship between trust and these preferences using four original, pre-registered survey experiments conducted in the UK over two years with nearly 7000 respondents, and individual-level panel data from Switzerland over a 19-year period, with a total of 65000 person-year observations and 12600 unique individuals. These experimental manipulations and panel data converge on a core finding: contra established theory and expectations, political trust is *causally* and *longitudinally* unrelated to preferences over redistribution policy, though also finds that there is a persistent *correlation.* Experimentally manipulating trust in government has an insignificant and substantively negligible effect on redistribution preferences, even when treatment conditions are varied to provide a 'most-likely' case; that is, when respondents are posed with some sacrifice (in this case, higher taxes). This conclusion is robust to a variety of pre-registered estimation methods, sub-group analyses, and other robustness tests. Analyses of panel data also indicate that individuals' *changing* trust judgements have a precisely-estimated, negligible, and statistically insignificant relationship with redistribution preferences, even when taking into account temporal dynamics of the theory (such as effects unfolding over multiple years). However, the analyses also suggest that there is a persistent *correlational* effect, such that more trusting individuals are more supportive of redistributive spending than low trusting individuals. Thus, neither the panel nor experimental evidence provides evidence of a longitudinal, causal relationship between trust and redistribution preferences, but instead that there is an enduring cross-sectional relationship, consistent with existing work.

The paper provides two important empirical contributions. First, I combine two designs with plausible causal identification. Existing literature is largely based on cross-sectional observational designs, both on the link between trust and redistribution preferences

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(Barnes, 2015; Colombo & Ray, 2024; Gabriel & Trüdinger, 2011; Habibov et al., 2018; Hetherington, 2005; Hetherington & Globetti, 2002; Hetherington & Husser, 2012; Rudolph & Evans, 2005; Witko & Moldogaziev, 2023), and policy preferences generally (Fairbrother et al., 2021; Jacobs & Matthews, 2012; Macdonald, 2019; Paxton & Knack, 2012). The analyses presented here contribute both short-term, experimentally manipulated effects, and long-term within-individual effects, to this evidence base. Second, it advances both the extant experimental (Colombo & Ray, 2024; Fairbrother, 2019; Macdonald, 2021, 2023; Peyton, 2020) and panel evidence (Goubin & Kumlin, 2022). The experiments presented here randomise the *theoretically fundamental conditions* posited by the underlying theories, providing a more nuanced test of the underlying theory than in existing tests; the panel analyses meanwhile significantly extend the temporal horizon by using the longest available panel data, identifying both short-term and long-term effects over a period of nearly 20 years and at multiple lags.

The analyses have two theoretical implications. First, they call into question the long-standing theory linking trust and policy preferences, at least in the case of redistribution policy. Neither experimental conditions most likely to 'activate' trust as a mechanism nor panel data equipped to show long-term change provide evidence in support of the core theory. Second, however, they do replicate a robust cross-sectional relationship identified in previous work. This suggests an alternative theoretical account for the link between trust and (redistribution) policy preferences: one that relates trust to long-term, relatively stable features of individuals. I return to these implications in the concluding section. All told, however, this suggests that the source of the link between trust and policy preferences, and perhaps other consequences, may lie elsewhere than is currently being studied.

At their broadest, the results speak to the foundational literature on how citizens' attitudes, values, and beliefs shape the performance and policies of democratic governments (Almond & Verba, 1963; Putnam et al., 1993). Despite Easton (1965, 1975) developing and systematising these range of attitudes over half a century ago, the results here suggest

we have a long way to go in understanding how and under what conditions citizens' attitudes, values, and beliefs, shape their preferences and, ultimately, policies of democratic governments – including how they can tackle the most potent societal challenges.

Political trust and policy preferences

Why would trust matter for what people want from government? Two related literatures motivate this conjecture: the literature on the consequences of political trust, and that of the long-term policy making literature. Both start from a common understanding of *political* trust defined as people's basic evaluative and affective orientation to the institutions and actors governing their polity (Citrin & Stoker, 2018; Miller, 1974), where trust is a positive orientation that the trusted would produce preferred outcomes even if left unattended and where positive outcomes are uncertain (Easton, 1975). Political trust therefore consists both of a 'specific' evaluative and 'diffuse' affective component: it is expected to respond both to the (political) environment but to also consist of a relatively stable underlying latent trait (Devine & Valgarðsson, 2022).

The implication from both the political trust and long-term policy making literature is that trust provides a fundamental ingredient for overcoming uncertainty and risk in the policy process. The core hypothesis is, put simply, that those who are more trusting are more likely to support (expanding) government activity, such that higher trust overcomes the inherent risks and uncertainty in policy making. This section fleshes out this idea, focusing first on the trust-as-heuristic theory, then the long-term policy making literature.

Research developing from the political trust literature conceptualises trust as a decision rule - a heuristic - to help citizens decide 'whether to support or oppose government action' (Rudolph et al., 2017), with the implication that greater trust means greater support for government action. As noted by Peyton (2020), this is a linear causal model, positing that, as an individual's political trust increases, as does their support for more generous redistributive policy. This mechanism applies to the more general literature on

how and when political trust matters for policy preferences and other political outcomes, such as voting and informational participation (Devine, 2024; Zmerli & van der Meer, 2017).

Indeed, the trust-as-heuristic theory provides at least three moderating factors which would produce heterogenous effects of trust. First, trust becomes more important when there is risk in the form of a) material risk, such as accepting some financial costs; and b) ideological cost, such as when policy action requires some ideological sacrifice (for instance, conservatives supporting expanding redistribution) (Hetherington, 2005). Secondly and more recently, scholars have studied *policy costs*, where citizens are asked to sacrifice spending in some policy (say, education spending) for future benefits in another (say, pensions) (Garritzmann et al., 2023). In the case of redistribution, these general mechanisms have ample support, albeit almost entirely from the United States (e.g Chanley et al., 2000; Hetherington, 2005; Hetherington & Globetti, 2002; Rudolph, 2009; Rudolph & Evans, 2005).

A final moderating factor is issue salience. Citizens cannot plausibly consider all issues at the same time when thinking about trust in government (or any other actor), and they may use alternative sources of judgement. People may consider government trustworthy in general, but not on specific areas; it would be odd, for instance, for any given citizen to think a government is trustworthy on every single policy area, or be thinking of every single policy area when determining how much they trust government. Put more directly: if citizens are not *thinking about* governments' trustworthiness on redistribution (or any other policy area), there is less reason to expect trust would be related to preferences on those areas. Hetherington and Husser (2012) provide evidence for this in the United States showing that, following 9/11, political trust had no relationship with race or redistribution preferences but, instead, foreign and defence policy; yet, as salience over race issues increased, so did the effect of political trust on race-targeted welfare programs.

An orthogonal strand of literature is concerned with understanding public support for, and government action on, long-term policy. As this paper began with, many of the most

pressing problems are precisely those that entail costs in the short-term in the hope of tackling distant challenges; yet voters are myopic when it comes to social investment, and opt for lower short-term costs (Healy & Malhotra, 2009). This growing literature addresses many potential sources of these 'long-term policy preferences', of which political trust is one (Jacobs, 2016).

Future policy preferences are characterized by *uncertainty* regarding processes of long-term policy causation and long-term policy *commitments* (Jacobs & Matthews, 2012). Long-term policy making entails the governing authorities to not only spend the raised funds competently, but *not* to do so nefariously; there may be concerns that governments will divert funds to other preferred goals, or that they are lying about their true intentions. Political trust assuages these concerns: it facilitates citizens taking the leap of faith that governments will, despite uncertainty, spend the funds as promised and to do so competently (Busemeyer, 2023; Fairbrother et al., 2021).

Of course, the extent to which policies are 'long-term' and 'redstributive' is contested and lies along a spectrum (e.g., Jacobs, 2016, p. 437). There are certainly long-term policies that are *not* redistributive, and redistributive policies that are *not* long-term. As the above has illustrated, however, the theoretical predictions are similar: since they both imply costs for uncertain or uneven benefits, trust should matter for policy support. As I shall return to, to bridge the literatures on political trust (which have typically used redistributive policies as a dependent variable) and long-term policy making (which typically vary the temporal trade-offs), the policies chosen in this paper aim to strike a balance between those that are long-term and those that are redistributive.

Existing empirical evidence

These two strands of literature approach the relevance of political trust from different perspectives: the first, to understand and explain the consequences of political trust; the second, to understand long-term policy preferences. Yet both converge on the propos-

ition that political trust enables support for expanding government action, and this is particularly the case when the policy entails greater risk and uncertainty.

Evidence, primarily from the United States, strongly supports these propositions in the case of redistribution policy. There is evidence that political trust bolsters support for race-targeted redistribution and particularly amongst those who do not benefit financially (Hetherington & Globetti, 2002), as well as increasing spending on a range of policy areas (Chanley et al., 2000; Hetherington, 2005). Rudolph and Evans (2005) argue that this is also ideologically assymetrical, with political trust having a larger effect on redistribution support amongst conservatives, who are not ideologically disposed to such policy (see also Hetherington & Rudolph, 2015); on the flip-side, political trust influences support for tax *cuts*, but only amongst those ideologically disposed to greater spending (Rudolph, 2009). In a range of post-communist countries, Habibov et al. (2018) also find political trust is related to higher support for redistribution spending and greater taxation, whilst Barnes (2015) uses data from advanced democracies and finds political trust increases demands for large government, but decreases support for progressive taxation (see also Svallfors, 1999, 2002; Witko & Moldogaziev, 2023). In Germany, meanwhile, Gabriel and Trüdinger (2011) find that political trust is importantly in generating support for welfare reforms, but the direction of effects varies, and Colombo and Ray (2024) find that, in Italy during the COVID pandemic, trust was positively related to transfer generosity.

However, this work is almost all based on cross-sectional data.[2] Whilst excellently conducted, it only explains one form of variation, that is, variation *between* individuals. Yet there is also temporal, 'life-cycle' variation: do individuals' *changing* trust judgements over time have a relationship with preferences over redistribution spending? If one changes trust judgements, do redistribution preferences respond? In addition, it has a more general limitation in telling us whether this is a causal relationship. The bulk of existing work overlooks – often out of data limitations – the potential for temporal change and is vulnerable to common issues of omitted variable bias.

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[2]Whilst some (e.g Fairbrother, 2019; Jacobs & Matthews, 2017) experimentally manipulate the outcome (such as the cost of a policy), only two, to my knowledge, manipulate *trust* itself.

Studies leveraging other forms of variation - longitudinal or experimental - find less support for the theory. Two studies, to my knowledge, manipulate respondents' levels of trust; whilst one finds no relationship between trust and redistribution spending in the United States (Peyton, 2020) another finds some evidence that redistribution preferences can be manipulated (Kuziemko et al., 2015). There are many potential reasons for the differences in effects between these two studies, with the most likely being a different treatment: whilst Peyton (2020) provides a vignette, Kuziemko et al. (2015) provides a bundled treatment requiring respondents to state their level of agreement with various negatively-valenced statements about government and view a graph about the (lack of) transparency in federal government. Notably, however, the effect was only on items relating to transfers to the poor, and was relatively small in size. An experimental study conducted cross-nationally in Europe finds that trust measured pre-treatment moderates policy support in one case of a particular policy trade-off, though not in many others (Garritzmann et al., 2023). Distinct from the studies previously noted, Garritzmann et al. (2023) does not manipulate trust, but treats it as a moderator, which may explain the differences in effects. Those that adopt panel studies to address the temporal relationship find small and inconsistent results (Goubin & Kumlin, 2022). Overall, those designs that depart from cross-sectional data have heretofore found either conflicting, inconsistent, or null results on the link between trust and preferences on redistribution policy. Putting it more directly: there is substantial uncertainty and minimal evidence on cause and effect relationships in this area.

In the remainder of the paper, I build on this recent spate of experimental and panel evidence. Using four pre-registered survey experiments (N = 7000), I replicate recent work which has manipulated trust to understand its downstream consequences on redistribution preferences (Peyton, 2020) and build on this by randomly exposing respondents to conditions in which trust is most likely to be activated - namely, introducing *costs* in the form of taxation, and priming greater issue *salience*. That is, I test for the first time the effects of experimentally manipulated trust on redistribution preferences conditional on strongly-theorised mechanisms; this provides a more theoretically-nuanced test of the hy-

potheses posited. Meanwhile, I go beyond existing panel evidence which has been limited to short time periods of often only a year or two (Goubin & Kumlin, 2022; Haugsgjerd & Kumlin, 2020; van Elsas et al., 2020), and thus unable to test how trust and redistribution preferences evolve over time, by employing a 19-year panel study of approximately 12600 unique individuals, and test how trust may impact redistribution preferences at a lag of multiple years. I discuss the design and results of these two approaches sequentially.

Evidence from four randomised survey experiments

Design

The experimental component is a series of four pre-registered, nationally-representative survey experiments from the United Kingdom, fielded online with YouGov and Ipsos Mori between 2020 and 2022.[3] It is worth highlighting that one of these waves (December 2020) was early into the pandemic, and I discuss specific events around the fieldwork periods in Appendix A.

In all experiments, respondents were randomly assigned to either treatment vignettes which described the experience of an 'anonymous scientist' who has worked with government, writing in a 'respected and independent national news organisation', or a placebo vignette, which described the experience of working in a bookshop which was also written in a 'respected and independent national news organisation'. A 'scientist' was chosen as it is consistently one of the most trusted professions in the UK, with approximately 83% of the UK population saying they trust scientists, as of 2022 (when the last survey was fielded).[4]

Treatment vignettes were either 'positive' (to manipulate trust upwards) or 'negative' (downwards) about the experience of working with government. These were identical ex-

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[3]Ethical approval was granted by the Social Sciences and Humanities Interdivisional Research Ethics Committee at [University Name] (Ref: SSH/DPIRC1A21026)

[4]See the Ipsos Veracity Index here: <https://www.ipsos.com/en-uk/ipsos-veracity-index-2022>

cept for the wordings being positive or negative (e.g., 'competent' versus 'incompetent'). All vignettes were between 130 and 170 words and designed to closely follow previous experimental manipulation of political trust (Macdonald, 2021; Peyton, 2020), and concern issues of competence, benevolence, and integrity, as core parts of trustworthiness (Mayer et al., 1995). Full wording of the vignettes are in Appendix A.1. After seeing the vignettes, respondents were asked basic recall of the vignettes as an attention check, and then asked questions measuring their trust and redistribution preferences. The analyses do not condition on attention check responses, given evidence on the problematic consequences of doing so (Aronow et al., 2019).

I follow the British Election Study (BES) wording for political trust, specifically trust in government: 'How much of the time, if at all, do you think you can trust the government in Westminster to do what is right?', with response scales of 'just about always', 'most of the time', 'only some of the time' and 'almost never'. Whilst there are a number of ways to measure political trust and different political objects (such as parliament, politicians, parties, etc), I opted for this question for two core reasons. First, it is common in the BES and American National Election Study (ANES), and is therefore used in the majority of studies on this question. Second and substantively, this question is also more volatile than other types of trust questions (Cook & Gronke, 2005), and therefore provides a 'most likely' case to be manipulated.

Redistribution preferences are measured as spending references on three policy areas: pensions, the national health service (NHS), and welfare for the unemployed. These spending areas are diverse, target different subgroups, and have different temporal horizons. Welfare for the unemployed is a compensatory policy that implies a shorter time horizon; pensions, however, involve a much longer time horizon (at least for most of the respondents). These policies are chosen to bridge the literatures that focus on trust and policy preferences (specifically redistribution) (see, for example, those that use similar items Goubin & Kumlin, 2022; Hetherington, 2005; Peyton, 2020) and those on long-term policy making (Jacobs & Matthews, 2012). As Jacobs (2016) documents, studies

typically ignore the time horizon of policies in favour of their (re)distributive mechanisms even when there are 'intertemporal trade-offs for societies and social groups — such as old-age pensions'. The policies chosen here provide a compromise between redistributive and temporal trade-offs. All of this said, the theoretical expectations are much the same, as documented above: they impose costs for uncertain and uneven returns, whether that is in time or across society.

The specific question is: 'Thinking of what taxpayer money could be spent on, do you think spending should be decreased, kept the same, or increased for the following?'. Response categories are 'increased', 'kept the same' and 'decreased'. Response categories and policy areas are randomised for each respondent to avoid item order effects. This question is similar to other questions on redistribution preferences in this field (see, for example, Barnes, 2015; Garritzmann et al., 2023; Goubin & Kumlin, 2022; Habibov et al., 2018; Hetherington, 2005; Peyton, 2020), and has been used in the American National Election Study since 1984[5], and is used in the core studies on trust and redistribution preferences (Hetherington, 2005; Peyton, 2020). I provide a comparison to other survey questions in Appendix A, and return to issues of question wording in the concluding discussion.

To test if the effect of trust is moderated in the ways the theory expects, I randomly assign respondents to variations of the survey which emphasise *salience* and *costs*; these conditional effects are central to the theory yet so far untested in experimental work. For the former, I primed respondents specifically on public spending policy in a vignette otherwise identical to the others - practically, this includes a line in the vignette stating that politicians are 'competent and transparent when handling public money – for example, spending on the NHS, welfare for the unemployed and pensions'. The idea here is to test whether priming respondents about the trustworthiness of governments specifically on redistribution has a greater effect than just increasing trust in general.

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[5]The specific policy areas have been changed to reflect differences between the US and UK welfare regimes. For instance, the US version includes food stamps and 'programs that assist Blacks and other minorities', which either are not applicable (food stamps) or would not resonate (minority-targeted welfare) in the UK.

Experiments 1-2

Vignette assignment

Placebo   Positive   Negative   Positive (Salience)

↓

Recall (attention) checks

↓

Trust question

↓

Redistribution questions
(No trade-off)

Experiments 3-4

Vignette assignment

Placebo   Positive   Negative

↓

Recall (attention) checks

↓

Trust question

↓

Redistribution questions

No trade-off   Trade-off
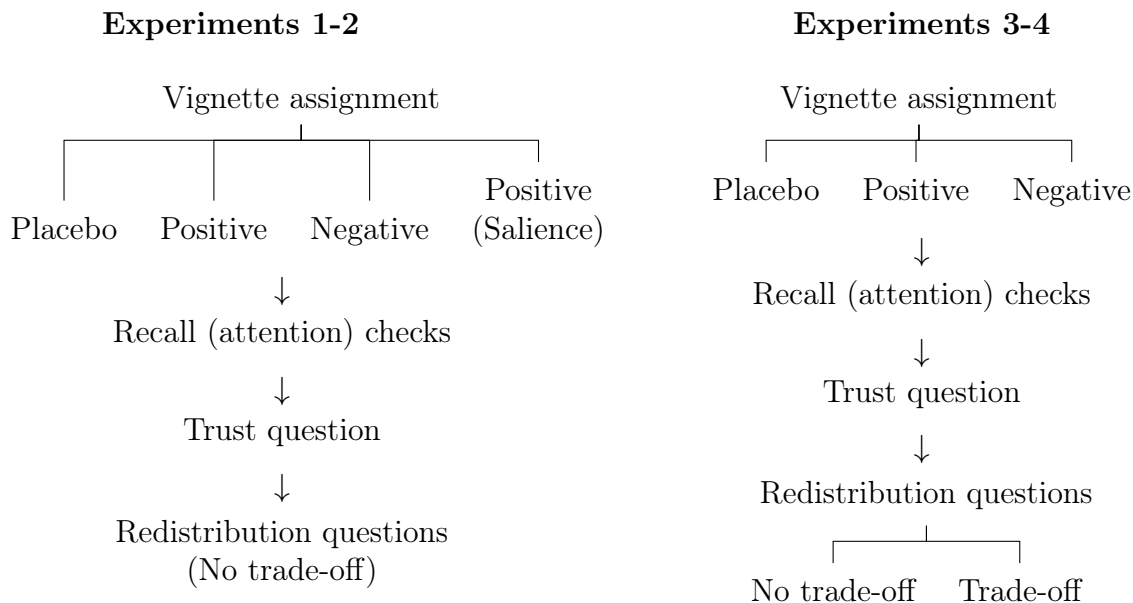
Figure 1: Survey flow for the two waves, showing treatment splits

For the moderating effect of *costs*, I randomly allocate respondents to two versions of the outcome question on redistribution preferences. Whilst one is the same as above, the second follows the question with '[...] if increasing spending means higher taxes?', which follows a similar design by Garritzmann et al. (2023). In this case, respondents are posed with a clear fiscal trade-off: increased spending leads to increased taxation. Whilst there are many potential trade-offs, it is not feasible to test all of these, and taxation is a widely-understood, low-effort trade-off for respondents to understand. In the case of Garritzmann et al. (2023), they also randomise *policy* trade-offs (for example, increasing education spending at the cost of reducing pension spending). I return to this difference, and its implications, in the concluding section.

To summarise the experimental structure, Figure 1 presents the survey flow for experiments 1-2 (on the left panel) and 3-4 (on the right panel), making it clear that a) the salience variation occurs in the first two experiments and b) the trade-off variation occurs in the second two experiments. These had to be split to ensure statistical power. Additional details, such as power analyses, fieldwork dates, and sample size, are provided in the pre-registration documents and in Appendix A.

Hypotheses

Following the literature discussion, I posit the following (pre-registered) hypotheses:

Treatment hypothesis (H1): Assignment to the 'positive' vignette leads to higher political trust compared to those assigned to the placebo and the 'negative' vignette.

Trust-redistribution hypothesis (H2): Assignment to the 'positive' vignette leads to preferences for greater redistribution compared to those assigned to the placebo or the 'placebo' vignette.

Trade-off hypothesis (H3): Trust has a larger effect on spending preferences when provided with a cost than when the spending is unconditional.[6]

Salience hypothesis (H4): Assignment to the 'positive (salience)' vignette leads to preferences for greater redistribution compared to other treatment groups.

Results

Figure 2 presents the results of vignette assignment on trust (left panel) and redistribution preferences (right panel), with the coefficient indicating the effect of assignment relative to the placebo vignette. Experiments are pooled, estimated with OLS regression, without covariate adjustment, and with heteroskedasticity-consistent (HC2) standard errors. To be able to clearly specify both significance and effect size, I transform the coefficients to Glass's Δ which scales outcomes by the standard deviation in the placebo group. This allows me to compare effect sizes for differently scaled outcomes (the trust variable, specific policy areas, and their additive index).

The results indicate that assignments had a statistically significant ($\alpha = 0.05$) *effect on trust* in all treatment groups, which was substantively meaningful in the *positive* and

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[6]reviewer correctly highlighted that this is unclear compared to H1, H2, and H4. To clarify, the benchmark group is the 'Placebo', and thus we would expect that the effect of assignment to 'Positive' is larger in the trade-off than no trade-off condition.

negative treatments but not in the *positive (salience)* treatments. This indicates that there was successful experimental manipulation of political trust, upwards for positive treatment and downwards for negative treatment, confirming H1.
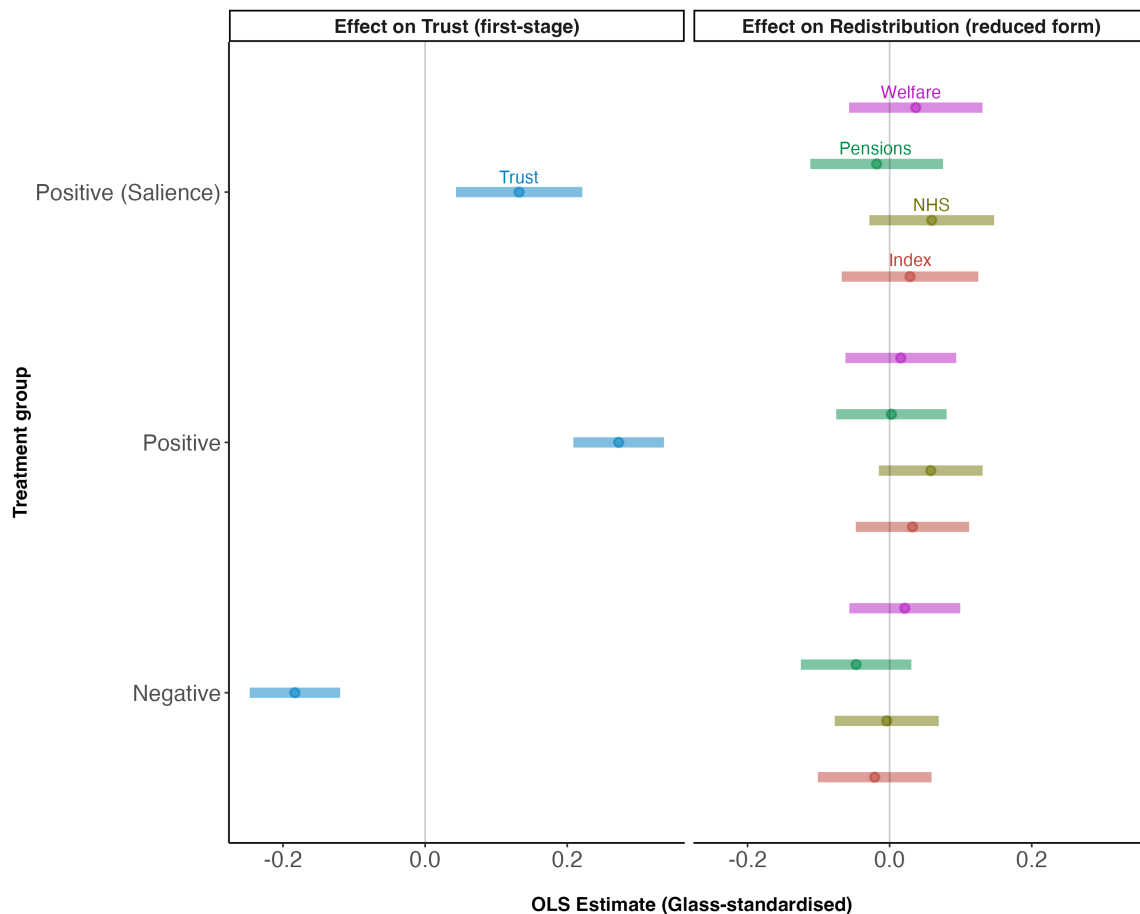
Figure 2: Effect of treatment assignment on trust and redistribution preferences. Tabled results available in Appendix A.5, Table 5.

However, the effect of treatment assignment *on redistribution preferences* is statistically insignificant across all outcomes. These results do not confirm H2 or H4 - there is no treatment effect on redistribution preferences. In Appendix A.4.4, I present alternative reference groups (e.g., effects relative to the 'positive' treatment, rather than placebo), which indicate substantively identical results.

To put these effect sizes into context, the effect of experimentally-manipulated trust on redistribution preferences is, at most, 7-10% the size of the partisan difference between Labour (typically pro-redistribution) and Conservative (typically anti-redistribution) voters,

indicating an effect size, at best a tenth of partisan difference. However, the effect of treatment on trust is, on average, a third of the size of the difference in trust between Labour and Conservative voters in the placebo group.[7]

These results indicate successful experimental manipulation of political trust, but no downstream effects on redistribution preferences. The conclusion is that (experimentally-manipulated) political trust does not have a statistically significant nor a substantively meaningful effect on redistribution preferences. This is even the case when respondents are primed to think specifically about redistribution (the *positive (salience)* treatment).

Yet, so far untested in experimental work is the effect of *costs*, and this is a core prediction of the literature on long-term policy making and political trust. I test for this by providing half the sample with an explicit trade-off (increasing taxes), and the other half with the previously-used 'unconditional' (or no trade-off) preferences. Figure 3 shows the results of vignette assignment on trust (lower panel) and each of the redistribution outcomes, split by whether there are trade-offs or not. A hypothesis test is included for the *Index* outcome, but no others: this is because there are no statistically significant differences between the effect of the vignettes (compared to Placebo) in the two conditions. As noted in Figure 1, the salience condition was not included in these experiments and so there are only two treatment groups - 'positive' and 'negative'.

Consistent with Figure 2, then, there is no statistically significant downstream effect of trust on redistribution preferences whether given a trade-off or not. The estimated effect sizes are minimal, and the differences are not statistically different from zero. There is potential that assignment to the 'negative' group reduces spending preferences when there are trade-offs compared to when there are not, but this difference is very minimal and not statistically significant. Thus, there is also no support for H3.

All told, there is little experimental evidence for the proposed relationship between trust and redistribution preferences, even when manipulated with theoretically-favourable con-

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[7]If the 'positive' treatment is the base group, the effect of the 'negative' treatment on trust is almost the entirety (90%) of the difference between Labour and Conservative voters.
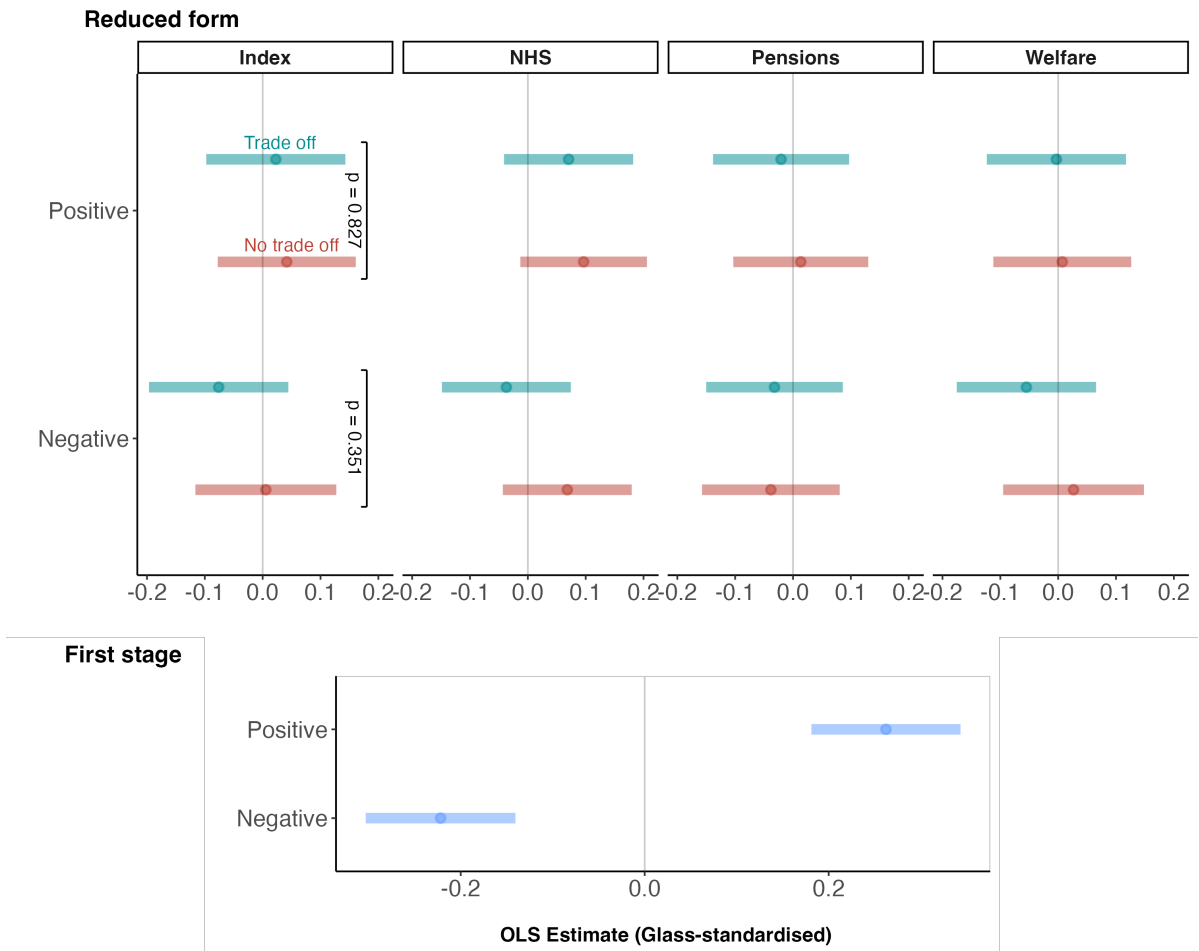
Figure 3: Effect of treatment assignment on trust and redistribution preference index with trade-offs.

ditions. To test the robustness of these results, three additional analyses are presented in Appendix A. First, I conduct a pre-registered instrumental variables regression which uses the randomly assigned treatment as an instrument. This is plausible given random assignment and large first-stage effects. These results are consistent with the main results. Finally, I investigate heterogeneous effects amongst respondents using generalised instrumental random forests, which takes a vector of respondent covariates (age, education, party identification and left-right ideology) and searches for effect heterogeneity (Green & Kern, 2012). There is minimal evidence of effect heterogeneity. I supplement this with multiplicative interactions, and find consistent results.

These results also pose questions. Experimental manipulation of trust may not have the downstream consequences that 'real world' change may do; consequences may unfold

over time, rather than instantaneously. The external validity of the results are also unknowable: do they generalise outside of the specific vignettes, questions used, and country context? Is other variation than that which is induced by short-term information provision more important?

Evidence from long-run panel data

Design

To overcome these issues, I turn to long-run panel data. Specifically, I use the Swiss Household Panel Study (SHP). This is the *only* panel study that contains trust and redistribution questions in the same waves over a suitably long period of time that allows me to observe *within* individual change over time; and it does so over a 19-year period (1999-2018), thus unrivalled in its temporal coverage. Other panel studies which contain trust and redistribution (such as the British Election Study and German Longitudinal Election Study) do so in only a few waves.

I use the two measures for redistribution preferences which are available in the SHP. The first asks, 'Are you in favour of a decrease or in favour of an increase in federal social spending?', with a three-point response scale options of 'in favour of an increase', 'neither' or 'in favour of a decrease'. The second asks, 'Are you in favor of an increase or in favour of a decrease in the tax on high incomes?' with the same response scale as the first. Particularly the first question broadly reflects the experimental questions, tapping preferences over spending in general. The second, however, addresses another aspect of redistributive policy, namely preferences over tax progressivity (Barnes, 2015). This provides the opportunity to understand the consequences of trust for multiple dimensions of redistribution.

Trust is measured through a question asking, 'How much confidence do you have in

the federal government, if 0 means "no confidence" and 10 means "full confidence"?', with the response scale as stated in the question.[8] An important point here is that the question refers to the 'federal' government. In Switzerland, the federal government is a grand coalition and does not control all social spending, but does control unemployment, health insurance, and subsidies for the poor; education is largely at the canton level. As such, the expectation is that the questions on trust and redistributive spending are about equivalent to the ones in the experimental study, which used healthcare, unemployment, and pensions.

The primary modelling strategy is a within-between multilevel model for change. The 'within-between' aspect decomposes the effect of predictor variables into the effect of differences between individuals and the effect of changes over time within individuals[9] (Fairbrother, 2014); the multilevel model for change allows me to take into account that time is nested within individuals and that individuals have different trajectories of change. In practice, this means that each predictor variable has two variables (one for between and one for within variation) and that time has a random slope for each individual. Since the SHP is sampled at the household level, a level is added to account for individuals clustered within households. I control for basic demographics in all presented models: age, income, education, occupational status and gender, as well as left-right ideology and political interest. Alternative modelling strategies, such as testing for additional time dynamics (for example, whether trust in previous time periods matters for contemporary redistribution preferences), are presented in the Appendix, with results consistent with this strategy.

The equation for the model is:

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[8]Whilst the question in English is translated to 'confidence', the word for 'trust' and 'confidence' is the same in the original languages of the survey and does not pose a problem.

[9]The between effect is essentially the mean for each individual, and the within effect is the individual variation from their own mean value.

$$Y_{itj} = \beta_0 + \underbrace{\beta_1 \bar{X}_i}_{\text{between}} + \underbrace{\beta_2 X_{ij}}_{\text{within}} + \overbrace{\beta_3 Wave_{ijt}}^{\text{wave FEs}} + \overbrace{Wave_{ijt} * b_{1i}}^{\text{wave slope}} + \underbrace{b_{0i}}_{\text{individual intercept}} + \underbrace{b_{2j}}_{\text{household intercept}} + \epsilon_{itj}$$

Y_{itj} indicates the outcome - redistribution preferences - and ϵ_{itj} is the error term. Substantively, the within ($\beta_2 X_{ij}$) and between ($\beta_1 \bar{X}_i$) parameters of trust are those of particular interest.

There is suitable within-variation for these types of analyses and the data has been used for similar longitudinal analysis trust (Devine & Valgarðsson, 2022) and redistribution preferences (O'Grady, 2019) (though not the relationship between one and the other). Descriptive statistics, reported in the Appendix, show that whilst the majority of the variation in the variable is between individuals, but there is also reasonable standard deviation of the within variation: 1.26 for trust and 0.5 and 0.44 for the two dependent variables. Thus whilst preferences are indeed stable generally, there is variation in the extent to which people change.

Hypotheses

If the dominant theory in the literature is correct, we would expect both within and between effects for trust, leading to the following (non-registered) hypotheses:

Within hypothesis (H5): Individuals increasing in trust leads to preferences for greater redistribution.

Between hypothesis (H6): Individuals with higher trust have preferences for greater redistribution.

Results

The results are presented in Figure 4.[10] The coefficients unstandardised OLS estimates. Most crucially, the *within* effect (that is, an individual changing from their mean value) is not statistically different from zero and is of a very small magnitude: on average, when trust changes, redistribution preferences do not respond, or at least do so a trivial amount. It is plausible that there is a positive effect of trust on preferences for greater spending on social programmes, but this would be extremely small; for taxes on higher incomes, the estimated effect is precisely zero. The results therefore reject H5. As noted, I also test for *long-term* effects of trust on redistribution preferences – for example, whether trust in time periods $T-1$ and $T-2$ are related to redistribution preferences in time T – which provides substantively identical results.
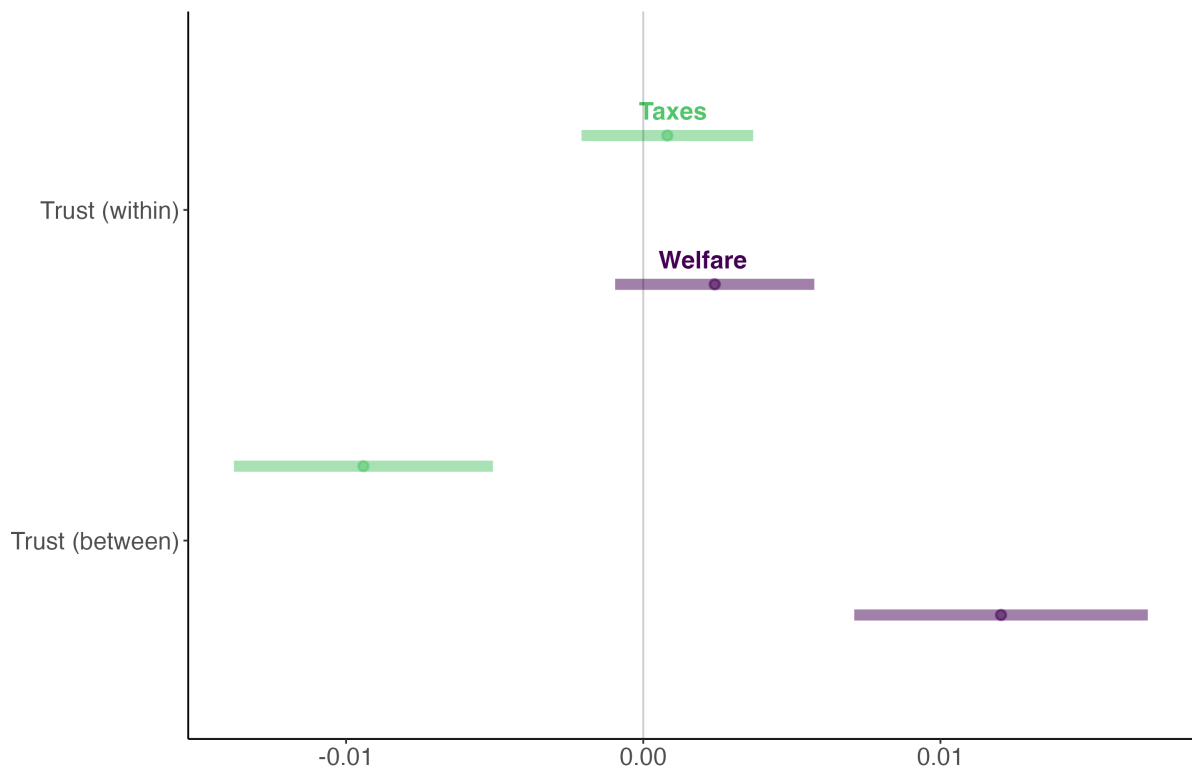
Figure 4: Coefficients from within-between panel models in Switzerland . Full tabled results are available in Appendix B.2.1.

The results do indicate, however, that the *between* effect has a statistically significant

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[10]The sample is limited to those who have completed *two or more* waves. I present other samples, descriptive statistics, and modelling strategies in the Appendix.

effect on social expenses (positively) and taxes on high incomes (negatively). These are of relatively small magnitudes: going from least to most trusting would lead to a change of 0.1 on a 3-point scale, a 3% change. This is partial confirmation of H6 - trust has different effects on spending (in the expected direction) and tax progressivity (in the opposite direction). Whilst unexpected by the theory, Barnes (2015) finds a similar effect in a study of 17 countries, concluding that whilst trust does lead to preferences for greater taxation it also *reduces* preferences for a progressive (pro-poor) structure. Similarly, Svallfors (2013) shows that quality of government conditions attitudes to taxes on the welfare and also moderates the effect of egalitarianism attitudes on wealth taxes. Both of these could be at play in the Swiss case.

A range of substantive and statistical robustness tests are presented in Appendix B. Given the theory outlined, I test for subgroup heterogeneity in the effect of trust amongst those who incur ideological or material costs, as the theory predicts. These show somewhat mixed results. There's no evidence for the *material* costs – trust has the same (null) effect amongst those at the top or bottom of the income distribution. There is some evidence for *ideological* costs: trust increases redistribution preferences amongst right-wing respondents, but *reduces* it among left-wing respondents. It is difficult to conclude either way regarding ideological costs, but with a clear rejection for the general theory for material costs. Additionally, I test for the effect of trust at multiple time periods - for instance, the effect of trust at time T-2 on redistribution preferences at time T - using a cross-lagged panel model in the structural equation modelling framework (Figures 11 and 12). This indicates that trust has no effect on redistribution preferences at multiple lags and, if anything, has a negative effect.

Discussion

Governments are tasked with dealing with fundamental social challenges, yet doing so is often costly and its benefits uncertain; it involves imposing immediate financial costs,

restricting behaviour, or asking for other individual sacrifices. A range of literature posits that this is easier to do if the government is trusted (e.g Fairbrother et al., 2021; Hetherington, 2005; Jacobs, 2016), and has theorised the mechanisms that moderate this relationship. This paper has tested these mechanisms in the case of redistribution preferences as a case of long-term policy, and one which is most studied in the political trust literature (e.g Garritzmann et al., 2023; Goubin & Kumlin, 2022; Hetherington & Globetti, 2002; Jacobs & Matthews, 2012; Peyton, 2020; Svallfors, 1999). Contra the existing evidence and strongly-theorised mechanisms, the results of experimental and panel analyses presented in this paper find minimal to no support for a causal link between trust and redistribution preferences. Yet, consistent with previous work, there is a persistent *cross-sectional* relationship. What to make of this?

Most importantly, this challenges the long-standing theory linking trust and policy preferences, particularly in the case of redistribution preferences. Neither short-term experimentally manipulated effects, even under strongly-theorised conditions, nor long-term effects (over a period of nearly 20 years), provide evidence in favour of it. This is consistent with other research that aims to gain causal traction (Garritzmann et al., 2023; Goubin & Kumlin, 2022; Peyton, 2020). There are theoretical and empirical reasons that might explain this discrepancy. Theoretically, perhaps the theory is more nuanced than previously developed. Most basically, trust may encourage status quo bias: if one is trusting, perhaps one thinks that the current situation must be okay? That trust is related to greater spending is also inconsistent with work which shows trust being positively related to reform generally (Goubin & Kumlin, 2022, e.g., []). A second concern, not contradictory but at least inconsistent with the theory tested here, is that individuals have *competing trusts*; that is, they may trust (or not trust) numerous actors and institutions. In this case, what happens when the messages of the trusted are competing? Is trust in government, or other actors, related to higher spending if a related actor is advocating for lower trust or lower spending? Future research on this topic could take into account these competing trust claims. Finally, and as noted, some research (Garritzmann et al., 2023) finds trust-related effects for policy trade-offs, but not unconditioned effects

nor for fiscal trade-offs. This is also a point worth developing more theoretically and empirically. The theory does not make a distinction between types of trade-off, and it is not clear why, theoretically, these trade-offs should have different consequences for the trust-redistribution relationship.

Yet, the persistent cross-sectional relationship discovered here and in much other previous work (Devine, 2024) suggests that there is something going on. There are a number of possibilities that explain these different findings. First, it may well be that political trust and redistribution preferences are relatively stable and formed in early adulthood or younger (Devine & Valgarðsson, 2022; O'Grady, 2019) which may establish an enduring correlation that exists throughout the life-course. Thus, *generational* changes in trust may make an aggregate difference to policy support. A second explanation is that other, so far undiscovered variables which are relatively stable over time, confound the relationship between trust and redistribution preferences. Candidates include political morality (Ansolabehere et al., 2008), ideological identification (Kiley & Vaisey, 2020), and personality traits (Tepe & Vanhuysse, 2020) which are shown to be stable and are plausibly related to both political trust and redistribution preferences. Long-term data, which often 'controls away' for such stable dispositions, would control for the confounder, unlike cross-sectional data; meanwhile, experiments designed to manipulate trust in the short-term may be missing the mark by not manipulating the relevant confounder. These designs would not then find an effect that is identified in cross-sectional work that does not control for the confounder. A third explanation may lie in the measurement of trust and redistribution. Political trust is usually measured as government, parliament, or other institutions; yet people take (trust) cues from many actors, as noted. Moreover, these are specific institutions, and may not address a more 'diffuse' institutional trust that may have different consequences. For redistribution, this paper has followed most existing work by asking about spending preferences, but it may be that trust is only activated when the object is mentioned in the question about redistribution (for example, should *government* spending be increased). Future research should explore whether these differences in questions have consequences.

There are important scope conditions to this paper. First, like all experimental manipulations, results may be conditional on question wording, as detailed above; however, it is important to recognise that the measurement in this paper is consistent with previous research on both trust and redistribution preferences, and is therefore comparable to previous studies. It is also worth noting that other work (Garritzmann et al., 2023) finds evidence for the 'trade-off' hypothesis using *policy* (rather than fiscal) trade-offs in four of the six trade-offs. However, there are some important differences in design: that paper does not experimentally manipulate trust but rather the policy outcome and the predictor variable is not trust but satisfaction with how government is 'doing its job'. Nonetheless, it is worth keeping in mind that support may have been found for policy trade-offs and this is an important extension for future work. Likewise, Colombo and Ray (2024) find a causal relationship between (updates in) social trust and support for welfare generosity in Italy. Whilst there are differences in design between the two, a broader study on *why* (if at all) there are differences between social and political trust would be worthwhile.

A second important set of scope conditions are temporal and spatial. The experiments were conducted in the UK (a liberal welfare state, distinct from its European neighbours), at a particular period of time (in the wake of the COVID-19 pandemic). Yet, the results are internally consistent: the results in December 2020 are the same as those in March 2022, despite a tumultuous period in UK politics. The results are also consistent with other experimental data from the United States and Europe, which suggests this is a broadly applicable finding. In addition, the panel analyses yield the same result, from a very different political system. And whilst it would be valuable to include a greater range of countries in the panel data, the Swiss Household Panel Study is the *only* study which includes comparable redistribution questions over a meaningful length of time, and therefore is the best option for this question, despite the drawback of it being a single (wealthy) country.[11] The fielding of additional panel studies and experiments testing

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[11]The Dutch Panel Study (LISS) asks whether 'differences in income should increase' or 'decrease'; the British Election Study asks whether 'government should make much greater efforts to make people's incomes more equal'. The German Socio-Economic Panel does not ask trust and redistribution questions together.

question wording differences and alternative manipulations is encouraged. Finally, the specific context here is a purely national one; there is also evidence that trust assessments of international organisations are related to support for international redistribution and foreign aid (Bauhr & Charron, 2020; Paxton & Knack, 2012). These findings may extend to these contexts, but I don't claim that here.

This paper's findings challenge the conventional wisdom regarding the relationship between political trust and redistribution preferences. The lack of causal effects observed in experimental manipulations and panel data analyses suggest that the influence of trust on preferences may be more nuanced than previously thought. These results call for a reevaluation of the role of trust in shaping policy preferences, particularly in the context of long-term and uncertain policy challenges. Further research is needed to better understand the complex interplay between citizens' attitudes and policy preferences, and how citizens can be mobilised to support the challenges government face in confronting significant policy dilemmas.

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Appendix

Political trust and redistribution preferences

A Survey experiments

A.1 Pre-registration

The pre-registration for all studies can be found at the end of this document. Unfortunately, due to an error with one of the three registrations, it is not possible fully guarantee anonymity in the uploaded and time-stamped versions. The registrations included at the end of this document are verbatim versions and time-stamped versions will be included upon conclusion of the anonymous review process.

A.2 Vignettes

Details on the fieldwork dates, companies, and where the conditions were fielded, are available in Table 1.

The periods of fieldwork obviously cover the pandemic period in the UK. The first period of November-December 2020 covered a shift in rule changes, including a second lockdown. In January 2022, there were also scandals relating to behaviour around parties in Downing Street. In March 2022, there were minimal UK-based events and COVID restrictions had eased.

| Company | Date | T_n | n | Conditions | |
				Salience	Trade-off
Ipsos Mori	Nov-Dec 20	4	1500	✓	✗
YouGov	Jan 22	4	1668	✓	✗
YouGov	Mar 22	6	1871	✗	✓
YouGov	Mar 22	6	1871	✗	✓

Table 1: Experiment fieldwork and treatment details

A.2.1 Preambles

The preambles below precede the placebo and treatment vignettes respectively

Placebo: What follows is an excerpt from a short description of someone's experience of working in a bookshop, written as an opinion piece about career choices in a respected and independent national news organisation. Try to read it closely, as you'll be asked two questions about it after.

Treatment: What follows is a short description of someone's experience of working with UK politicians, written as an opinion piece in a respected and independent national news organisation. Try to read it closely, as you'll be asked two questions about it after.

A.2.2 Placebo

We often hear that books are being replaced by other media, e-books, audiobooks, and so on. But research indicates that many people of all ages still prefer reading on paper, and over the last few months book sales have soared.

I left my job as a research scientist to work in, and eventually own, a bookshop, after reading other descriptions of the career in books like 'The Diary of a Bookseller'. I have seen the continued and rising popularity of print books. In our lives dominated by screens – a computer, TV, or phone – books offer a different experience and more clearly separate work from leisure time that I don't think can be replaced.

Yet the career should not be taken lightly. Whilst rewarding, it is a lot of work, and the profit margins are slim. Often, we rely on volunteers and donations alongside our 'best sellers' to make ends meet. The market is dominated by big high street chains. Despite this, I would not have had it any other way.

A.2.3 Positive

We often hear about trust in politics and politicians these days.

I've worked with governments of all colours in Westminster over many years, advising them on the most important issues of the day. Most of the politicians I have worked with keep their promises, have the interests of their constituents and the country at heart, and do their job competently. It is only the negative news we hear about, which does not reflect my experiences so far.

My experiences lead me to think that politicians are broadly competent, transparent and have the interests of the public at heart – no matter which party they're from. In other words, politicians are trustworthy.

The Author is an anonymous scientist who has worked as an external advisor to Conservative and Labour governments for decades, and advised other parties.

A.2.4 Negative

We often hear about trust in politics and politicians these days.

I've worked with governments of all colours in Westminster over many years, advising them on the most important issues of the day. Most of the politicians I have worked with fail to keep their promises, hold their own interests to heart instead of their constituents and the country, and do their job poorly. The negative news we hear about, unfortunately, reflects the reality of my experiences so far.

My experiences lead me to think that politicians are broadly incompetent, lack transparency and have their own interests at heart – no matter which party they're from. In

other words, politicians are untrustworthy.

The Author is an anonymous scientist who has worked as an external advisor to Conservative and Labour governments for decades, and advised other parties.

A.2.5 Positive (Salience)

Note: only in surveys 1 and 2

We often hear about trust in politics and politicians these days.

I've worked with governments of all colours in Westminster over many years, advising them on the most important issues of the day. Politicians have proved competent and transparent when handling public money – for example, spending on the NHS, welfare for the unemployed and pensions. Politicians of all parties have their constituents' and country's interests at heart. It is only the negative news we hear about, which does not reflect my experiences so far.

My experiences lead me to think that politicians are broadly competent, transparent and have the interests of the public at heart – no matter which party they're from. In other words, politicians are trustworthy.

The Author is an anonymous scientist who has worked as an external advisor to Conservative and Labour governments for decades, and advised other parties.

A.2.6 Attention checks

These immediately follow the vignettes

What was the previous occupation of the author of the previous article?

<1> Teacher

<2> Engineer

<3> Scientist

<4> Retail manager

<5 fixed> Not sure

What is the occupation of the author of the previous article?

<1> Teacher

<2> Engineer

<3> Scientist

<4> Retail manager

<5 fixed> Not sure

(If placebo) Was the article broadly positive or negative about their career choice?

<1> Positive

<2> Negative

<4 fixed> Not sure

If treatment Was the article broadly positive or negative about politicians?

<1> Positive

<2> Negative

<3 fixed> Not sure

A.2.7   Trust and redistribution questions

How much of the time, if at all, do you think you can trust the government in Westminster to do what is right?

<1> Just about always

<2> Most of the time

<3> Only some of the time

<4> Almost never

<5 fixed> Don't know

In experiment 1-2: Thinking of what taxpayer money could be spent on, do you think spending should be decreased, kept the same, or increased for the following?

- Welfare for the out-of-work

- The NHS and healthcare

- Pensions

<1> Decreased

<2> Kept the same

<3> Increased

<4 fixed> Don't know

In experiment 3-4: Thinking of what taxpayer money could be spent on, do you think spending should be decreased, kept the same, or increased for the following?

- Welfare for the out-of-work

- The NHS and healthcare

- Pensions

<1> Decreased

<2> Kept the same

<3> Increased

<4 fixed> Don't know

In experiment 3-4: Thinking of what taxpayer money could be spent on, do you think spending should be decreased, kept the same, or increased for the following, **if increasing spending means higher taxes**?

- Welfare for the out-of-work

-The NHS and healthcare

- Pensions

<1> Decreased

<2> Kept the same

<3> Increased

<4 fixed> Don't know

A.2.8 Comparison of Redistribution Questions

Table 2 shows question wording for measurements of redistribution preferences for this and other studies that look to understand the effect of trust on redistribution preferences. Primarily, this indicates that the question wording is consistent with previous studies in terms of its specificity. No studies here are more specific than the question wording here, with the possible exception of Garritzmann et al. (2023), which ask about training programs for the unemployed. Others are less specific (Goubin and Kumlin (2022) ask about 'social benefits and public services') whilst others are very similarly.

Study	Question Wording
This Study	'Thinking of what taxpayer money could be spent on, do you think spending should be decreased, kept the same, or increased for the following? [if increasing spending means higher taxes?]'
Barnes (2015)	On the whole, do you think it should or should not be the government's responsibility to . . . [e.g] Provide health care for the sick.
Peyton (2020)	Should federal spending on X be decreased, kept the same, or increased (for, e.g., 'programs that assist Blacks and other minorities')
Garritzmann et al. (2023)	'The government should increase spending on education.' and 'Now imagine there is a fixed amount of money that can be spent on tackling unemployment. Would you be against or in favor of the government spending more on education and training programs for the unemployed at the cost of reducing unemployment benefit?'
Goubin and Kumlin (2022)	'Many social benefits and public services are paid for by taxes. In a choice between on the one hand increasing taxes in order to expand benefits and services, and on the other hand reducing taxes and spending less on benefits and services, what should in your opinion be prioritized'
Rudolph and Evans (2005)	'should the federal government spend more money on this, the same as now, less, or no money at all?'
Habibov et al. (2018)	'Would you be willing to give part of your income or pay more taxes, if you were sure that the extra money was used to [improve education/health/climate/help the needy]'

Table 2: Comparison of redistribution questions between this and similar studies

Table 3: Mean trust and redistribution preferences by experiment

Experiment	N	Trust	Welfare	NHS	Pensions
1	732	2.2	2.2	2.8	2.5
2	827	2.0	2.2	2.8	2.6
3	1970	2.0	2.2	2.7	2.6

Table 4: Descriptive statistics for redistribution and trust questions, by experimental group

		Placebo (N=2006)		Positive (N=2034)		Negative (N=2037)		Positive (Salience) (N=809)	
		N	Pct.	N	Pct.	N	Pct.	N	Pct.
NHS	Decreased	65	3.2	41	2.0	59	2.9	26	3.2
	Kept the same	248	12.4	245	12.0	251	12.3	135	16.7
	Increased	1034	51.5	1027	50.5	996	48.9	614	75.9
Unemployment Welfare	Decreased	257	12.8	252	12.4	240	11.8	136	16.8
	Kept the same	607	30.3	579	28.5	589	28.9	337	41.7
	Increased	419	20.9	424	20.8	417	20.5	260	32.1
Pensions	Decreased	57	2.8	54	2.7	61	3.0	36	4.4
	Kept the same	460	22.9	449	22.1	472	23.2	294	36.3
	Increased	783	39.0	763	37.5	729	35.8	419	51.8
Trust	Almost never	527	26.3	378	18.6	633	31.1	162	20.0
	Only some of the time	899	44.8	886	43.6	972	47.7	355	43.9
	Most of the time	390	19.4	555	27.3	263	12.9	205	25.3
	Just about always	28	1.4	59	2.9	14	0.7	26	3.2

A.3 Descriptive statistics

Table 3 presents the mean values of trust and redistribution preferences by experiment. Here for sake of brevity I collapse experiments 3 and 4 into the same as they were fielded in the same week and with the same design.

Table 4 shows a tabulation of the four key variables (redistribution and trust) by treatment group, not divided by experiment.

Finally, table 5 shows covariate balance for age, left-right position, party ID and education across the four treatments. There are some small differences for the positive (redistribution) treatment, but this was only fielded in the first two experiments. This shows the successful randomisation of the treatments.

Table 5: Balance of covariates by experimental group

		Placebo (N=2006)		Positive (N=2034)		Negative (N=2037)		Positive (Salience) (N=809)	
		Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Age		49.8	16.8	50.3	16.8	49.9	17.0	50.0	16.5
Left-right		0.5	0.3	0.5	0.3	0.5	0.3	0.5	0.3
		N	Pct.	N	Pct.	N	Pct.	N	Pct.
Party ID	Conservative	665	33.2	667	32.8	680	33.4	241	29.8
	Labour	513	25.6	491	24.1	484	23.8	216	26.7
	Other (inc no vote)	828	41.3	876	43.1	873	42.9	352	43.5
Education	High	649	32.4	634	31.2	618	30.3	320	39.6
	Low	495	24.7	549	27.0	514	25.2	211	26.1
	Medium	523	26.1	542	26.6	567	27.8	214	26.5
	Other	339	16.9	309	15.2	338	16.6	64	7.9

Table 6: Full table for Figure 2

	Welfare	NHS	Pensions	Index	Trust
Positive	0.016	0.058	0.002	0.032	0.273***
	(0.040)	(0.037)	(0.040)	(0.041)	(0.039)
Negative	0.021	−0.004	−0.047	−0.021	−0.173***
	(0.040)	(0.037)	(0.040)	(0.041)	(0.039)
Positive (Salience)	0.037	0.059	−0.018	0.029	0.144**
	(0.048)	(0.045)	(0.048)	(0.049)	(0.047)
Experiment FE	−0.037+	−0.020	0.099***	0.013	−0.141***
	(0.019)	(0.018)	(0.019)	(0.020)	(0.019)
Num.Obs.	4517	4741	4577	4338	4650
RMSE	1.00	0.96	1.00	1.00	0.99

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

A.4 Tables for main analyses

Table 7: Full table for Figure 2 (*without* filtering by trade-off condition in experiments 3-4)

	Welfare	NHS	Pensions	Index	Trust
Positive	0.016	0.058	0.002	0.032	0.272***
	(0.040)	(0.037)	(0.040)	(0.041)	(0.033)
Negative	0.021	−0.004	−0.047	−0.021	−0.183***
	(0.040)	(0.037)	(0.040)	(0.041)	(0.032)
Positive (Salience)	0.037	0.059	−0.018	0.029	0.132**
	(0.048)	(0.045)	(0.048)	(0.049)	(0.045)
Experiment FE	−0.037+	−0.020	0.099***	0.013	−0.111***
	(0.019)	(0.018)	(0.019)	(0.020)	(0.016)
Num.Obs.	4517	4741	4577	4338	6352
RMSE	1.00	0.96	1.00	1.00	0.99

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table 8: Full table for Figure 3

	Welfare	NHS	Pensions	Index	Trust
Positive	0.007	0.096+	0.013	0.042	0.257***
	(0.061)	(0.056)	(0.060)	(0.061)	(0.058)
Negative	0.027	0.068	−0.038	0.005	−0.230***
	(0.062)	(0.057)	(0.061)	(0.062)	(0.058)
Trade off	−0.097	−0.154**	−0.285***	−0.242***	0.064
	(0.062)	(0.057)	(0.060)	(0.062)	(0.059)
Positive * Trade off	−0.010	−0.026	−0.034	−0.019	0.007
	(0.086)	(0.080)	(0.085)	(0.086)	(0.083)
Negative * Trade off	−0.081	−0.105	0.006	−0.082	0.012
	(0.087)	(0.080)	(0.086)	(0.087)	(0.083)
Num.Obs.	3321	3468	3371	3173	3443
RMSE	1.02	0.96	1.00	0.99	0.99

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

A.5 Additional analyses

A.5.1 Instrumental variables

Following Peyton (2020) I also estimate the results with an instrumental variable regression using two-stage least squares. Since the instrument (assignment to treatment) is random and there is a large effect on trust (the first stage effect), it is a plausible instrument to estimate the effect of trust on redistribution preferences. These results are in Figure 5, which show a non-significant effect across the board, though since the 95% CIs cross the 0.2 threshold, I cannot rule out them being substantively meaningful. Yet, given the previous results and the very minimal point estimate, I don't think this is likely.
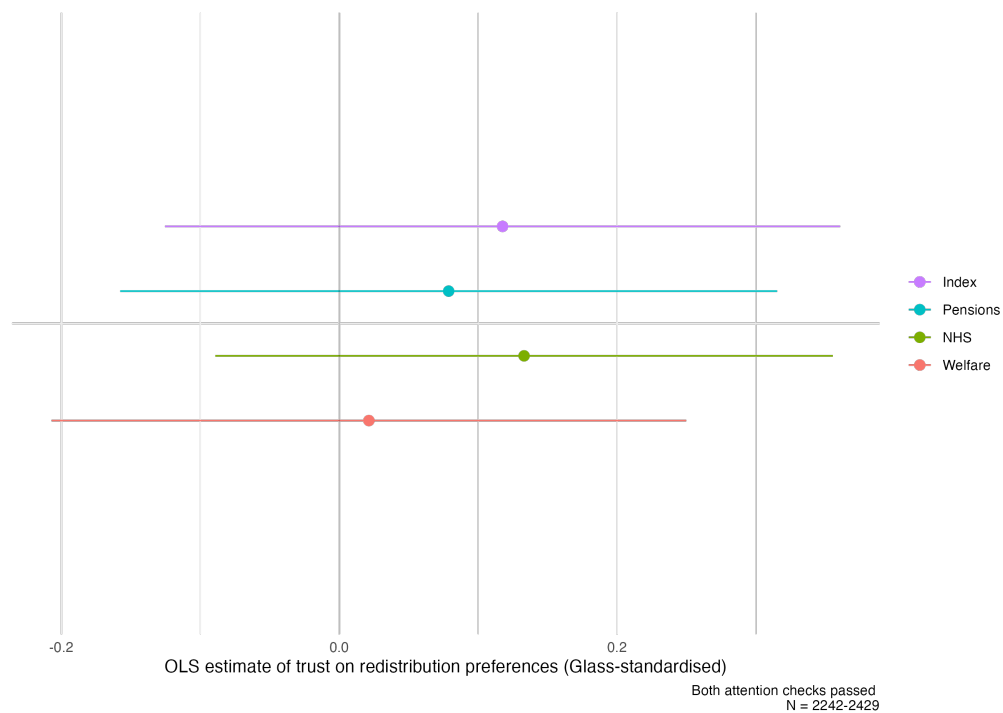
Figure 5: Estimates from an instrumental variable regression

A.5.2 Instrumental generalised random forests

Following previous work (Green & Kern, 2012; Guess & Coppock, 2020; Peyton, 2020), I use generalised random forests (GRFs) to automate the search for treatment effect heterogeneity based on specified covariates (here, education, party identification, left-right position, age, and a fixed effect for the experiment). This serves to address the hypothesis that trust is particularly relevant for some sub-groups, specifically those that must make material or ideological sacrifices. The conditional average treatment effects (CATEs) (derived from the instrumental variable estimation in figure 5) are presented with 95% credibility intervals in figure 6. Just under 3% of the estimated treatment effects include zero; of these, 20% are positive, meaning approximately 0.06% of the treatment effects (conditional on the noted covariate profiles) are significant and in the expected direction.

Exploring heterogenous treatment effects
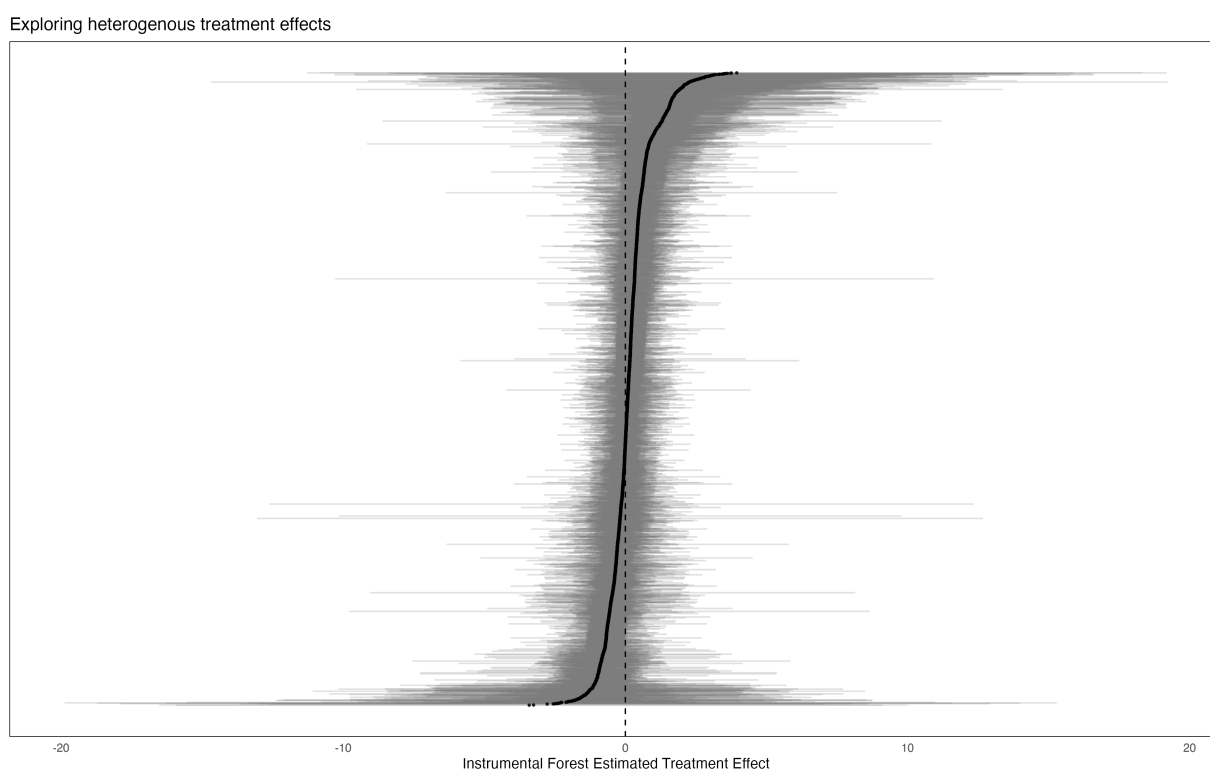
Figure 6: Estimates from an instrumental generalised random forest estimation (CATEs)

I repeat this analysis for the *trade-off* experiments in Figures 7 to 8. Results are broadly consistent. As in the main analyses, trust matters more for those given a trade-off condition, with approximately 7% of coefficients significant and in the expected direction (positive) (90% are non-significant). This is *not* the case for those *without* a trade-off, where the effects are almost identical to above (97% of coefficients cover zero, 2.5% do not cover zero and are positive).

Exploring heterogenous treatment effects - no trade-off condition
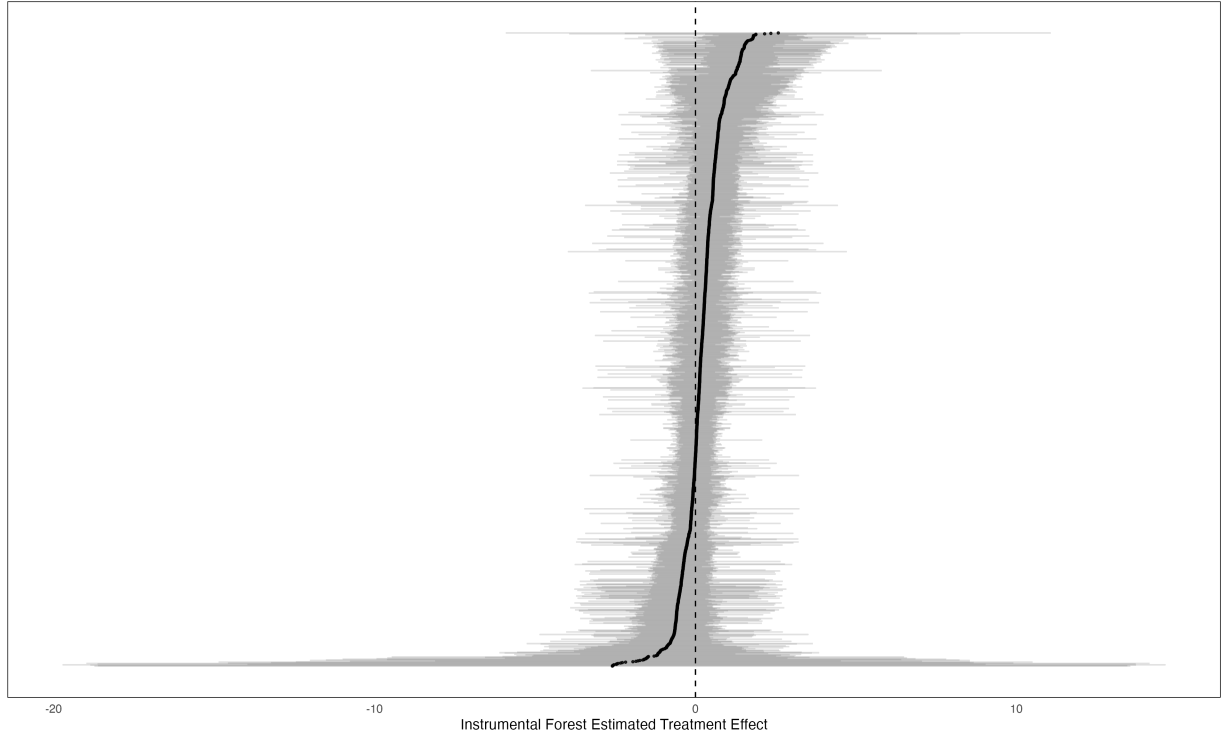
Figure 7: Estimates from an instrumental generalised random forest estimation for the no trade-off conditions (CATEs)

A.5.3 Additional interaction analyses

To increase confidence in these results, Tables 9 to 13 report multiplicative interaction regression models for the most-likely cases: left-right position (continuous, rescaled to between 0 (left) and 1 (right)), party identification (Labour, Conservatives, Other/None), age (categorical), age (continuous), and age again for the trade-off splits. These are most-likely because there are easily identifiable beneficiaries and 'losers': in the case of ideology (in the UK case, also equivalent to government opposition and support), it is any greater spending (versus lower spending); in the case of age, one is more likely to be a 'winner' in terms of pension (and, to a lesser extent, NHS) spending as one ages, but less so welfare for the unemployed.

None of these provide evidence for the hypotheses, and most interactions are non-significant. Indeed, in the case of age, the opposite is observed: assignment to the 'trust-increasing' (Positive) vignette *increases* support for pension spending for *older* people. If the results were consistent with the hypothesis, we would expect it to increase support for pension spending for *younger* people, as the 'losers' (or at least, not beneficiaries for some time) of the policy spending. This is still the case when provided with trade-offs (Table 13) - in fact, the significant results are not present in the trade-off conditions.

Exploring heterogenous treatment effects - no trade-off condition
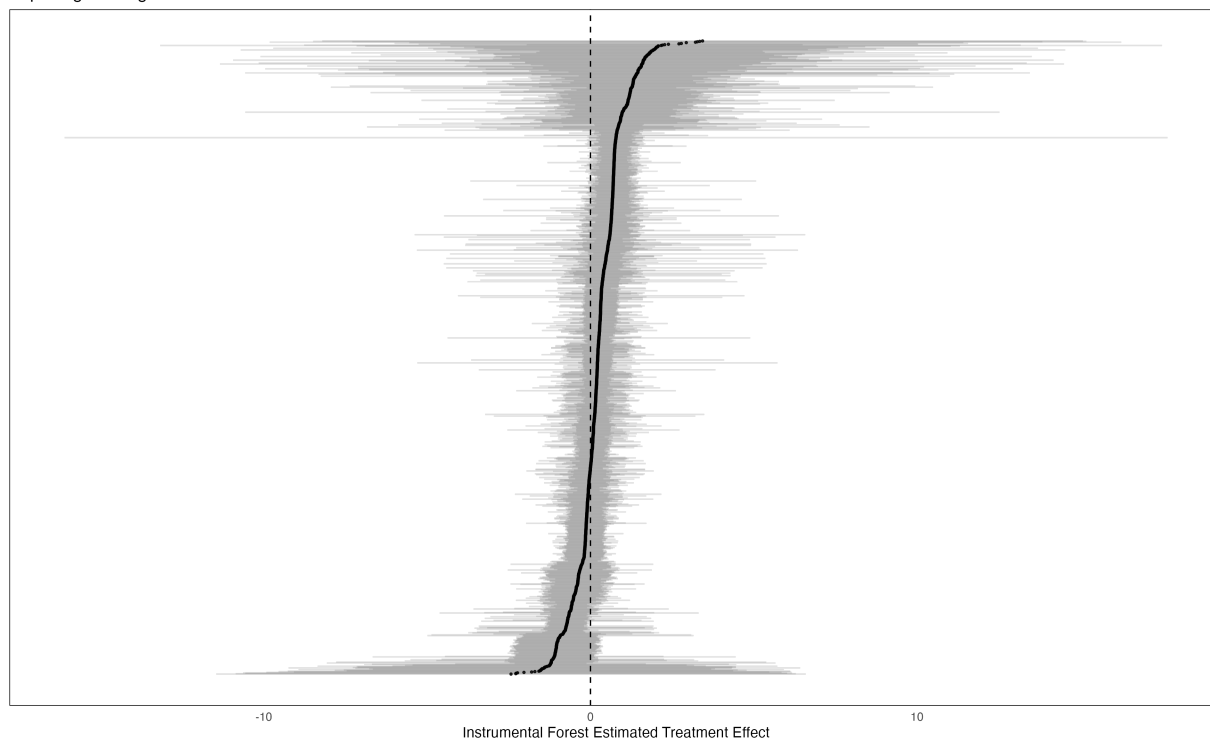
Figure 8: Estimates from an instrumental generalised random forest estimation for the trade-off conditions (CATEs)

Table 9: Experiment and left-right interaction

	Welfare	NHS	Pensions	Index	Trust
Positive	0.110	0.079	−0.002	0.068	0.261***
	(0.077)	(0.076)	(0.081)	(0.081)	(0.065)
Negative	−0.084	−0.046	−0.075	−0.102	−0.024
	(0.078)	(0.077)	(0.083)	(0.083)	(0.066)
Positive (Salience)	−0.086	0.026	0.024	0.000	0.059
	(0.092)	(0.090)	(0.097)	(0.097)	(0.089)
Left-right scale	−1.096***	−0.446***	−0.010	−0.807***	0.613***
	(0.092)	(0.089)	(0.096)	(0.097)	(0.077)
Experiment FE	−0.006	0.004	0.115***	0.048*	−0.136***
	(0.019)	(0.019)	(0.020)	(0.020)	(0.017)
Positive * Left-right	−0.165	−0.039	−0.020	−0.067	0.019
	(0.128)	(0.125)	(0.134)	(0.135)	(0.108)
Negative * Left-right	0.225+	0.069	0.046	0.160	−0.319**
	(0.131)	(0.127)	(0.137)	(0.138)	(0.108)
Positive (Salience) * Left-right	0.283+	0.065	−0.098	0.067	0.124
	(0.158)	(0.153)	(0.164)	(0.167)	(0.151)
Num.Obs.	4264	4471	4320	4096	6107
RMSE	0.95	0.96	1.00	0.98	0.98

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table 10: Experiment and party identification interactions

	Welfare	NHS	Pensions	Index	Trust
Positive	0.010	−0.008	0.000	−0.005	0.264***
	(0.065)	(0.063)	(0.067)	(0.067)	(0.052)
Negative	0.047	−0.090	−0.031	−0.062	−0.369***
	(0.066)	(0.064)	(0.068)	(0.068)	(0.052)
Positive (Salience)	−0.004	0.039	0.030	−0.007	0.089
	(0.079)	(0.077)	(0.082)	(0.082)	(0.072)
Labour	0.772***	0.385***	0.027	0.593***	−0.920***
	(0.069)	(0.067)	(0.072)	(0.072)	(0.056)
Other party	0.432***	0.176**	−0.002	0.285***	−0.710***
	(0.063)	(0.060)	(0.065)	(0.065)	(0.051)
Experiment FE	−0.031+	−0.018	0.099***	0.016	−0.122***
	(0.018)	(0.018)	(0.019)	(0.019)	(0.015)
Positive * Labour	0.057	0.115	0.021	0.095	−0.068
	(0.100)	(0.097)	(0.105)	(0.104)	(0.080)
Negative * Labour	0.006	0.166+	−0.019	0.093	0.305***
	(0.099)	(0.096)	(0.104)	(0.103)	(0.080)
Positive (Salience) * Labour	−0.047	−0.016	−0.103	−0.036	0.106
	(0.115)	(0.112)	(0.121)	(0.120)	(0.105)
Positive * Other	0.010	0.102	−0.004	0.060	0.051
	(0.088)	(0.085)	(0.092)	(0.091)	(0.071)
Negative * Other	−0.067	0.107	−0.026	0.045	0.253***
	(0.089)	(0.086)	(0.092)	(0.092)	(0.071)
Positive (Salience) * Other	0.102	0.045	−0.050	0.089	0.064
	(0.104)	(0.101)	(0.108)	(0.108)	(0.095)
Num.Obs.	4517	4741	4577	4338	6352
RMSE	0.95	0.94	1.00	0.97	0.93

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table 11: Experiment and age interactions

	Welfare	NHS	Pensions	Index	Trust
Positive	0.041	0.045	−0.277**	−0.088	0.244***
	(0.086)	(0.079)	(0.085)	(0.089)	(0.070)
Negative	−0.051	−0.014	−0.172*	−0.140	0.003
	(0.084)	(0.078)	(0.083)	(0.087)	(0.068)
Positive (Salience)	0.028	−0.037	−0.214*	−0.058	0.057
	(0.099)	(0.092)	(0.099)	(0.103)	(0.092)
Aged 35-60	−0.098	−0.006	0.100	−0.014	0.123*
	(0.072)	(0.067)	(0.071)	(0.075)	(0.059)
Aged 60+	−0.081	0.005	0.247**	0.069	0.479***
	(0.076)	(0.071)	(0.075)	(0.079)	(0.062)
Experiment FE	−0.036+	−0.019	0.099***	0.016	−0.110***
	(0.019)	(0.018)	(0.019)	(0.020)	(0.016)
Positive * Aged 35-60	−0.079	0.018	0.316**	0.105	0.041
	(0.104)	(0.096)	(0.103)	(0.108)	(0.085)
Negative * Aged 35-60	0.091	0.035	0.131	0.149	−0.169*
	(0.103)	(0.096)	(0.102)	(0.107)	(0.084)
Positive (Salience) * Aged 35-60	−0.032	0.040	0.224+	0.033	0.122
	(0.121)	(0.113)	(0.120)	(0.125)	(0.112)
Positive * Aged 60+	0.037	0.016	0.383***	0.205+	0.011
	(0.110)	(0.102)	(0.108)	(0.113)	(0.089)
Negative * Aged 60+	0.096	−0.018	0.196+	0.155	−0.332***
	(0.109)	(0.102)	(0.107)	(0.112)	(0.088)
expgrpPositive (Salience):agecat60+	0.069	0.230+	0.265*	0.203	0.043
	(0.126)	(0.118)	(0.125)	(0.130)	(0.116)
Num.Obs.	4517	4741	4577	4338	6352
RMSE	1.00	0.96	0.99	1.00	0.98

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table 12: Experiment and age interaction

	Welfare	NHS	Pensions	Index	Trust
Positive	−0.049	0.021	−0.449***	−0.244+	0.280**
	(0.129)	(0.120)	(0.127)	(0.133)	(0.104)
Negative	−0.081	0.029	−0.275*	−0.197	0.167
	(0.127)	(0.118)	(0.125)	(0.131)	(0.103)
Positive (Salience)	−0.005	−0.199	−0.299*	−0.208	0.089
	(0.151)	(0.141)	(0.149)	(0.156)	(0.140)
Age	−0.001	0.000	0.006***	0.002	0.011***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Experiment FE	−0.037+	−0.019	0.103***	0.016	−0.108***
	(0.019)	(0.018)	(0.019)	(0.019)	(0.016)
Positive * Age	0.001	0.001	0.009***	0.005*	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Negative * Age	0.002	−0.001	0.004+	0.003	−0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Positive (Salience) * Age	0.001	0.005+	0.006*	0.005	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Num.Obs.	4517	4741	4577	4338	6352
RMSE	1.00	0.96	0.99	1.00	0.98

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

A.5.4 Alternative baseline

An even stricter test is to set an alternative baseline - instead of comparing those treated to the placebo, compare other experimental groups to those treated with the positive vignette. Those results are shown in figure 9. The results show a very large effect of being treated with the negative vignette (relative to the positive); the difference is approximately 90% the difference between Labour and Conservative voters. In other words, the treatment on redistribution trust is almost as large as the difference between the voters of the primary left- and right-wing parties. Still, There is an approximately null effect and, at any rate, a substantively trivial effect on redistribution preferences.
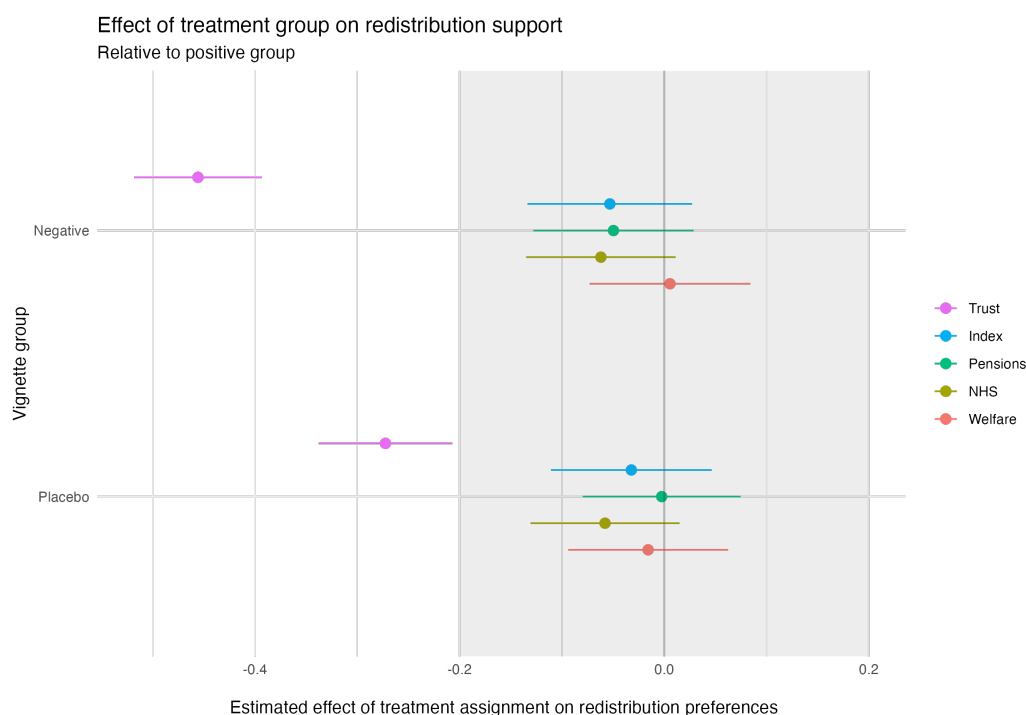
Figure 9: Primary model with 'positive' as the baseline category

A.6   Research ethics

The research was approved by the Social Sciences and Humanities Interdivisional Research Ethics Committee at [University Name] (Ref: SSH/DPIRC1A21026).

The survey participants were drawn from an established survey provider, YouGov, through which they provide consent to participate and are paid. There are no direct incentives from the researcher. For more information on YouGov's payment strategy, please see . By signing up to YouGov and accepting the invite to the survey, respondents are aware they are taking part in a survey.

There is minor deception in providing respondents with a false vignette. This was to update respondents' political trust. Whilst this could be done without deception (for

Table 13: Experiment and age interaction (trade-off split))

	Welfare (trade-off)	NHS (trade-off)	Pensions (trade-off)	Index (trade-off)	Welfare (no trade-off)	NHS (no trade-off)	Pensions (no trade-off)	Index (no trade-off)
Positive	-0.074	-0.151	-0.008	-0.145	-0.079	0.160	-0.384*	-0.221
	(0.197)	(0.187)	(0.189)	(0.200)	(0.194)	(0.171)	(0.189)	(0.194)
Negative	0.120	0.128	-0.048	0.016	-0.064	0.344*	-0.276	-0.120
	(0.196)	(0.185)	(0.189)	(0.200)	(0.195)	(0.171)	(0.188)	(0.195)
Age	0.003	0.000	0.016***	0.008**	-0.001	0.003	0.007**	0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)
Positive * Age	0.001	0.004	-0.001	0.003	0.002	-0.001	0.008*	0.005
	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
Negative * Age	-0.003	-0.003	0.000	-0.002	0.002	-0.005+	0.004	0.002
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)
Num.Obs.	1679	1739	1703	1607	1642	1729	1668	1566
RMSE	1.01	0.98	0.98	0.99	1.02	0.93	0.99	0.99

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

xx

instance, providing information on actual corruption or positive behaviour), deception was chosen because: i) it allowed full control over the information (i.e., an experiment), meaning *only* the negative/positive words were changed, and conditions could be added; ii) to closely follow previous work. Respondents were told at the end of the survey that this information was false, could opt to remove their data, and were given the contact details of the researchers and the ethics review board of the researchers' university. No one removed their data or used the contact details.

B Panel analyses

B.1 Descriptive statistics

Table 14 presents within-between variation for the three core variables. This shows that there's approximately similar variation between the two dependent variables (Std = 0.5 and 0.4). Importantly there is similar ratios of within-between variation: 91% of the variation in trust is between; for the dependent variables, it is 85% and 89%. As a point of reference, the 'satisfaction with democracy' measure has an equivalent between-variation ratio of 62%, suggesting that there is far greater within-variation for that measure.

This also indicates that there is greater within-variation for trust, relative to the between-variation. The standard deviation of the within variation is 64% of that of between variation for trust (1.26/1.96), but the equivalent numbers are 75% for both of the dependent variables. In other words, the within-variation is closer to the between-variation for the two dependent variables.

A note on interpretation of this table. The within variation is (average) deviation from an individual's average, which means the numbers do not necessarily have to be within the range. The maximum must subtract the global mean. This results in a maximum change of trust of 13.6-5.7 = 7.9. Whilst confusing, this is normal implementation of the *xtsum* function in Stata (here, an R implementation of it). See: https://www.stata.com/manuals/xtxtsum.pdf.

Table 14: Within and between variation for core variables

variable	variation	mean	std	min	max	obs
Trust	overall	5.7	2.15	0.00	10.00	N = 95475
	between		1.96	0.00	10.00	n = 19584
	within		1.26	-2.63	13.60	T-bar = 8.52
Social expenses	overall	2.2	0.77	1.00	3.00	N = 91493
	between		0.66	1.00	3.00	n = 19155
	within		0.50	0.35	4.06	T-bar = 8.34
Taxes	overall	1.38	0.65	1.00	3.00	N = 93562
	between		0.58	1.00	3.00	n = 19406
	within		0.44	-0.40	3.23	T-bar = 8.4

Table 15:   Descriptive statistics for numerical variables

	Unique (#)	Missing (%)	Mean	SD	Min	Median	Max
Trust	12	62	5.7	2.1	0.0	6.0	10.0
Social expenses	4	64	2.2	0.8	1.0	2.0	3.0
Taxation	4	63	2.6	0.7	1.0	3.0	3.0
Left-right	12	47	4.8	2.1	0.0	5.0	10.0
Gender	2	0	1.5	0.5	1.0	2.0	2.0
Income	11	47	5.5	2.9	1.0	5.0	10.0
Age	103	0	40.2	22.4	0.0	42.0	101.0
Wave	20	0	11.2	5.8	1.0	12.0	20.0

Table 16:   Descriptive statistics for categorical variables

		N	%
Work status	1	91184	35.9
	2	52246	20.6
	3	39878	15.7
	4	14655	5.8
Education	1	73770	29.1
	2	100457	39.6
	3	61142	24.1
Political interest	1	36524	14.4
	2	75438	29.7
	3	41732	16.4

B.2 Robustness tests and analyses

B.2.1 Full results tables

For reference, the full table for the results presented in Figure 3 follows. Wave fixed effects and variation components are omitted.

	Social expenses	High taxes
(Intercept)	2.701***	3.116***
	(0.045)	(0.040)
Trust (between)	0.009**	−0.011***
	(0.003)	(0.003)
Trust (within)	0.002	0.000
	(0.002)	(0.001)
Age (within)	0.001	0.001
	(0.002)	(0.001)
Age (between)	0.001*	0.000
	(0.001)	(0.001)
Income (within)	−0.003	−0.002
	(0.002)	(0.002)
Income (Between)	−0.003	−0.025***
	(0.003)	(0.003)
Education: Med (within)	0.008	0.108***
	(0.018)	(0.015)
Education: Med (between)	0.033*	0.090***
	(0.015)	(0.013)
Education: University (within)	0.016	0.077***
	(0.025)	(0.021)
Education: University (between)	0.040*	0.019
	(0.018)	(0.016)
Student (within)	0.050**	−0.042**
	(0.018)	(0.016)
Student (between)	0.063*	−0.297***
	(0.027)	(0.023)
Retired (within)	0.009	0.025*
	(0.015)	(0.013)
Retired (between)	−0.034	−0.068**
	(0.025)	(0.022)
Unemployed (within)	0.031*	0.001
	(0.013)	(0.011)
Unemployed (Between)	0.104***	−0.185***
	(0.026)	(0.023)
Left-right (within)	−0.019***	−0.011***
	(0.002)	(0.002)
Left-right (between)	−0.138***	−0.071***
	(0.003)	(0.002)
Fairly interested (within)	0.019*	0.015+
	(0.009)	(0.008)
Fairly interested (between)	0.079***	0.124***
	(0.017)	(0.015)
Very interested (within)	0.021+	0.018+
	(0.012)	(0.010)
Very interested (between)	0.031+	0.039*
	(0.018)	(0.016)
Gender	0.056***	−0.022*
	(0.011)	(0.010)
Num.Obs. ^xxv	65 020	65 020

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

B.2.2   Predicted values

Figure 10 plots the predicted values across the response scale for the primary analyses presented and for the within- and between-respondent coefficients.
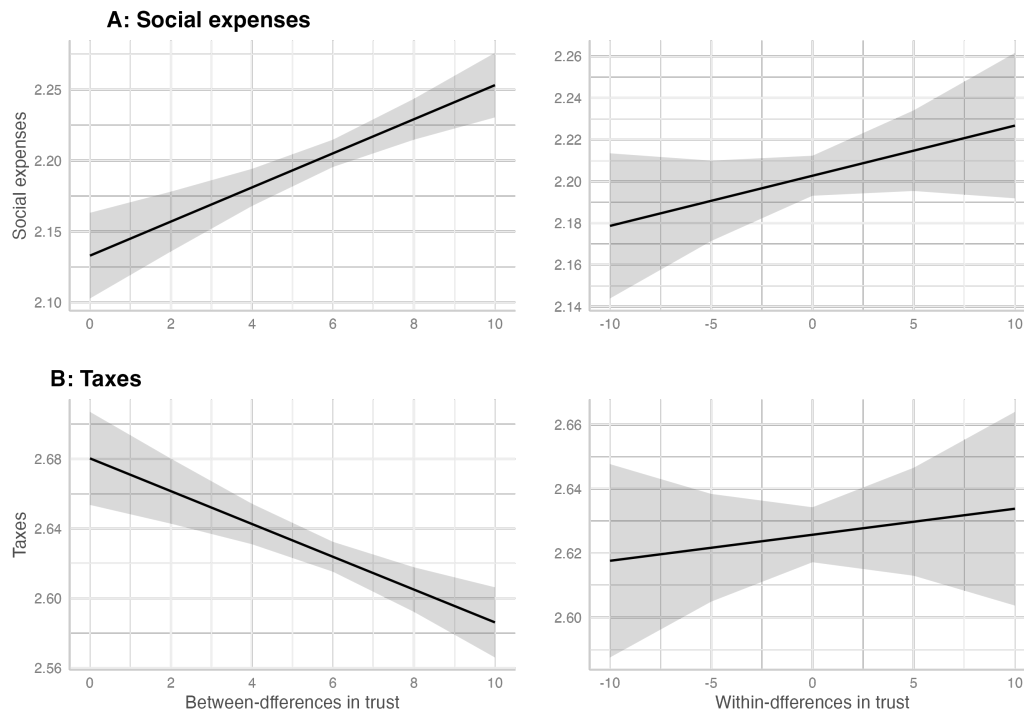

Figure 10: Predicted values across the response scale

B.2.3 Cross-sectional models

The basic pooled, cross-sectional models are presented in table 17. These are pooled across years and standard errors are clustered by individual and household. Note that the intention here is to pool the within- and between-effects as would happen in cross-sectional models. The model controls for age, education, work status, political interest and gender. The results of control variables are not shown.

Table 17: Pooled results from SHP data

	Dependent variable:	
	Social expenses	High taxes
	(1)	(2)
Trust	0.005**	−0.006***
	(0.002)	(0.002)
Controls	Y	Y
Observations	71,008	71,962
R^2	0.128	0.063
Adjusted R^2	0.128	0.062
Residual Std. Error	0.711 (df = 70995)	0.612 (df = 71949)

Note:	*p<0.1; **p<0.05; ***p<0.01

B.2.4 Fixed effects models

Table 18 presents fixed effects models, with fixed effects at the individual and household levels. This approximates the *within-effects only* model, such that the fixed effects 'control away' between variation. The model controls for age, education, work status, political interest and gender. The results of control variables are not shown.

Table 19 presents a comparison between different fixed effects models, with columns (1) and (3) including house and wave FEs alongside individual FEs (2 and 4). Again, these should be - and are - broadly consistent with the within-effects in the primary models.

B.2.5 Cross-lagged panel models

Figures 11 and 12 show the cross-lagged effect of trust on redistribution preferences at times T-1 and T-2 (i.e., a lag of up to two years). These both show no downstream effects of trust (indeed, in some cases, the effect is very small and negative).

Table 18: Fixed effects results from SHP data

	Dependent variable:	
	Social expenses	High taxes
	(1)	(2)
Trust	0.003	0.001
	(0.002)	(0.001)
Fixed effects ID	Y	Y
Fixed effects (House)	Y	Y
Observations	71,008	71,962
R^2	0.625	0.594
Adjusted R^2	0.498	0.458
Residual Std. Error	0.539 (df = 53096)	0.465 (df = 53858)

Note:	*p<0.1; **p<0.05; ***p<0.01

Table 19: Fixed effects results from SHP data (FE comparison)

	Dependent variable:			
	Social expenses		High taxes	
	(1)	(2)	(3)	(4)
Trust	0.001	0.003*	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.001)
Fixed effects (ID)	Y	Y	Y	Y
Fixed effects (House)	Y	N	Y	N
Fixed effects (wave)	Y	N	Y	N
Observations	71,008	71,008	71,962	71,962
R^2	0.627	0.613	0.597	0.581
Adjusted R^2	0.501	0.493	0.461	0.452

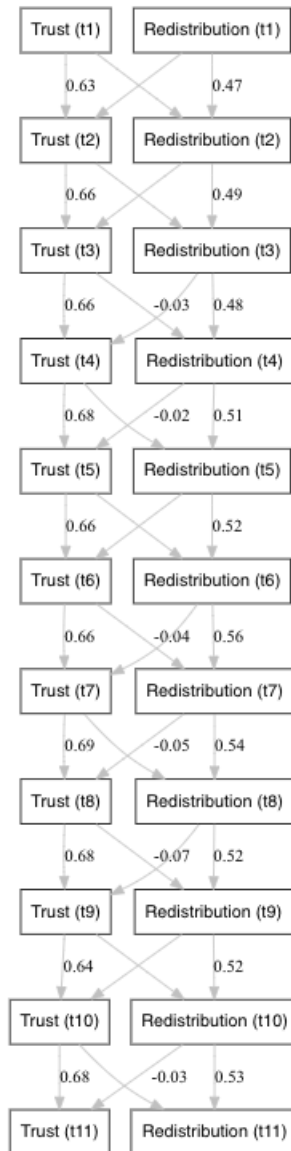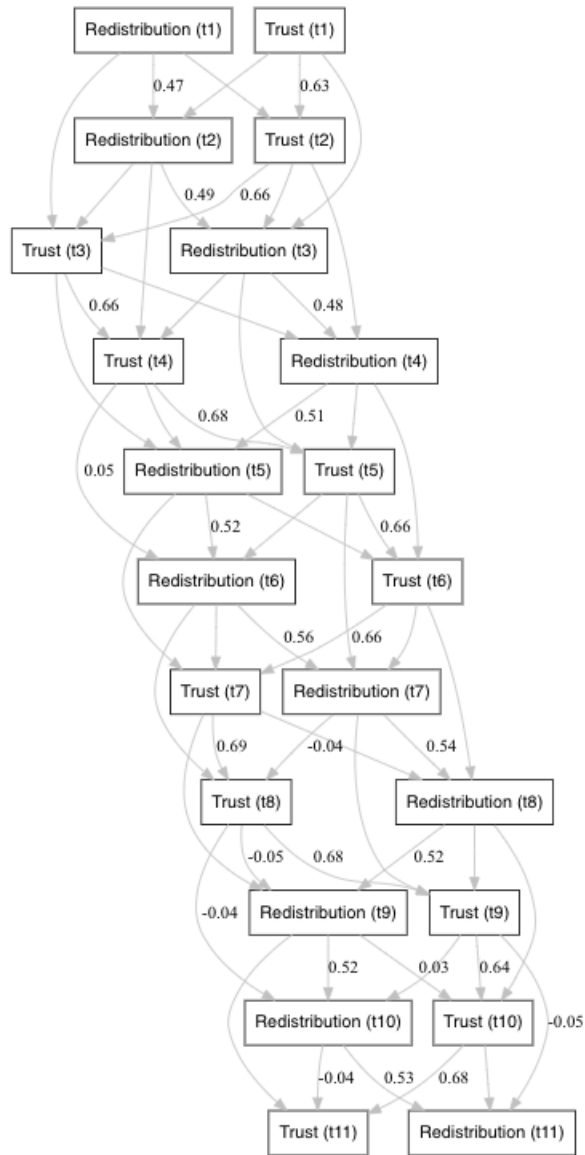| Note:	*p<0.1; **p<0.05; ***p<0.01

Figure 11: Cross-lagged panel models at 1 lag

Figure 12: Cross-lagged panel models at 2 lags

B.2.6 Variation on primary models

A range of alternative specifications are supplied here.

Table 20 presents a variation on the primary models that does not distinguish between the within and between effects of trust and, moreover, does not include a random effect for household, unlike the primary models. These show generally what we would expect: within- and between-effects similar to the primary models; namely, a reasonable positive/negative effect for the between results and a zero effect within. The models include the same controls as the main results.

Table 20: Comparison between growth models with pooled and disaggregated effects

	Dependent variable:			
	Social expenses		High taxes	
	(1)	(2)	(3)	(4)
Trust	0.007***		−0.002	
	(0.001)		(0.001)	
Trust (between)		0.012***		−0.010***
		(0.003)		(0.002)
Trust (within)		0.003		0.001
		(0.002)		(0.001)
Random effect (wave)	Y	Y	Y	Y
Random effect (house)	N	N	N	N
Observations	71,008	71,029	71,962	71,984
Log Likelihood	−67,900.090	−67,111.710	−58,613.200	−58,237.080
Akaike Inf. Crit.	135,836.200	134,281.400	117,262.400	116,532.200
Bayesian Inf. Crit.	136,001.300	134,547.400	117,427.700	116,798.500

Note: *p<0.1; **p<0.05; ***p<0.01

Table 21 presents the results from the primary models presented (columns (3) and (4)) alongside the models without the house random effects above. These show that the results are essentially identical. The household is still included to take into account the sampling strategy, though this does not affect the results (an ANOVA test indicates that including the household as a level is a better fit for the data).

Finally, Table 22 provides models with only trust (within and between) included (though still including the random effects for wave and house and fixed effects for wave). The results are consistent, with a larger between-effect but minimal and non-significant within-effect.

Table 21: Main model tables

	Dependent variable:			
	Social expenses		High taxes	
	(1)	(2)	(3)	(4)
Trust (between)	0.012***	0.012***	−0.010***	−0.009***
	(0.003)	(0.003)	(0.002)	(0.002)
Trust (within)	0.003	0.002	0.001	0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Random effect (wave)	Y	Y	Y	Y
Random effect (house)	N	Y	N	Y
Observations	71,029	71,029	71,984	71,984
Log Likelihood	−67,111.710	−66,934.310	−58,237.080	−58,027.940
Akaike Inf. Crit.	134,281.400	133,928.600	116,532.200	116,115.900
Bayesian Inf. Crit.	134,547.400	134,203.800	116,798.500	116,391.400

Note: *p<0.1; **p<0.05; ***p<0.01

Table 22: Bivariate models using the same specification as the main models

	Dependent variable:	
	Social expenses	High taxes
	(1)	(2)
Trust (between)	0.014***	−0.006***
	(0.002)	(0.002)
Trust (within)	0.001	−0.001
	(0.001)	(0.001)
Random effect (wave)	Y	Y
Random effect (house)	Y	Y
Observations	90,150	91,888
Log Likelihood	−87,789.340	−77,445.400
Akaike Inf. Crit.	175,596.700	154,908.800
Bayesian Inf. Crit.	175,681.400	154,993.700

Note: *p<0.1; **p<0.05; ***p<0.01

B.2.7 Change in wave completion

The models presented include respondents who complete two or more waves. However, this may mean that they are underestimating change; after all, those who completed waves over a longer period of time have more opportunity to change judgements. Here I include estimates for different subsamples of wave completion. Table 23 include respondents that have completed $>= 10$ waves. The results are similar to the presented results in the main claim - no within effects - but also show no between-effects for social expenses and a similar results for taxes. One reason for this is likely that the variables that generate the between differences also predict participation in long-run panel surveys, and thus those differences become less significant with more waves.

Table 23: Models with at least 10 waves completed

	Dependent variable:	
	Social expenses	High taxes
	(1)	(2)
Trust (between)	−0.002	−0.013***
	(0.005)	(0.005)
Trust (within)	0.003	−0.003
	(0.002)	(0.002)
Random effect (wave)	Y	Y
Random effect (house)	Y	Y
Observations	30,043	30,254
Log Likelihood	−25,453.990	−20,311.500
Akaike Inf. Crit.	50,967.990	40,683.000
Bayesian Inf. Crit.	51,217.300	40,932.520
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 24 presents the same models but for those who participated in $>= 5$ waves. This shows similar (but less significant) results to the main models, suggesting that the above proposition is plausible.

B.2.8 Ideology and income heterogeneity

Table 25 separates the sample by those who are left- (columns (1) and (2)) and those who are right-wing ((3) and (4)); these are defined by those who are 0-3 (left) or 7-10 (right) on the 0-10 left-right spectrum. This shows somewhat mixed results. For those who are right-wing, there is some support for the general theory: trust between and within increases support. However, it is *negative* for left-wing respondents. All models include the same control variables as in the main models, that is, education, work status, political interest, age, income, and left-right ideology.

Table 24: Models with at least 5 waves completed

	Dependent variable:	
	Social expenses	High taxes
	(1)	(2)
Trust (between)	0.007*	−0.014***
	(0.003)	(0.003)
Trust (within)	0.002	−0.001
	(0.002)	(0.002)
Random effect (wave)	Y	Y
Random effect (house)	Y	Y
Observations	57,139	57,774
Log Likelihood	−51,864.360	−43,615.950
Akaike Inf. Crit.	103,788.700	87,291.900
Bayesian Inf. Crit.	104,057.300	87,560.830
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 25: Separating by right and left-wing respondents

	Dependent variable:			
	Social expenses	High taxes	Social expenses	High taxes
	(1)	(2)	(3)	(4)
Trust (between)	−0.028***	−0.006*	0.034***	−0.016***
	(0.005)	(0.004)	(0.006)	(0.006)
Trust (within)	−0.006**	0.003	0.018***	−0.003
	(0.003)	(0.002)	(0.004)	(0.004)
Left-wing	Y	Y	N	N
Right-wing	N	N	Y	Y
Observations	15,806	15,941	11,358	11,453
Log Likelihood	−12,496.680	−8,693.936	−11,049.450	−11,017.960
Akaike Inf. Crit.	25,049.370	17,443.870	22,154.900	22,091.930
Bayesian Inf. Crit.	25,264.080	17,658.820	22,360.350	22,297.620
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 26 does likewise separating by high and low income respondents (these are, respectively, those in the top or bottom third of the income distribution). These show no evidence for the general theory.

Table 26: Separating by high and low income respondents

	Dependent variable:			
	Social expenses	High taxes	Social expenses	High taxes
	(1)	(2)	(3)	(4)
Trust (between)	0.006	−0.011***	0.007	−0.010**
	(0.005)	(0.004)	(0.005)	(0.004)
Trust (within)	0.003	0.001	−0.002	0.0001
	(0.003)	(0.002)	(0.004)	(0.003)
High income	Y	Y	N	N
Low income	N	N	Y	Y
Observations	26,886	27,007	17,726	18,146
Log Likelihood	−23,437.560	−20,357.600	−17,966.310	−16,390.930
Akaike Inf. Crit.	46,931.120	40,771.210	35,988.620	32,837.870
Bayesian Inf. Crit.	47,160.710	41,000.920	36,206.540	33,056.440

Note: *p<0.1; **p<0.05; ***p<0.01

I model these in an alternative way using interaction effects. Here, I interact the *within* effect of trust with the *between* effect of income and ideology. This asks: is the effect of changes in trust conditional on the average income/ideology of the respondent? These results are presented graphically in Figure 13. Results are consistent with the main analyses and the subsample analyses above.
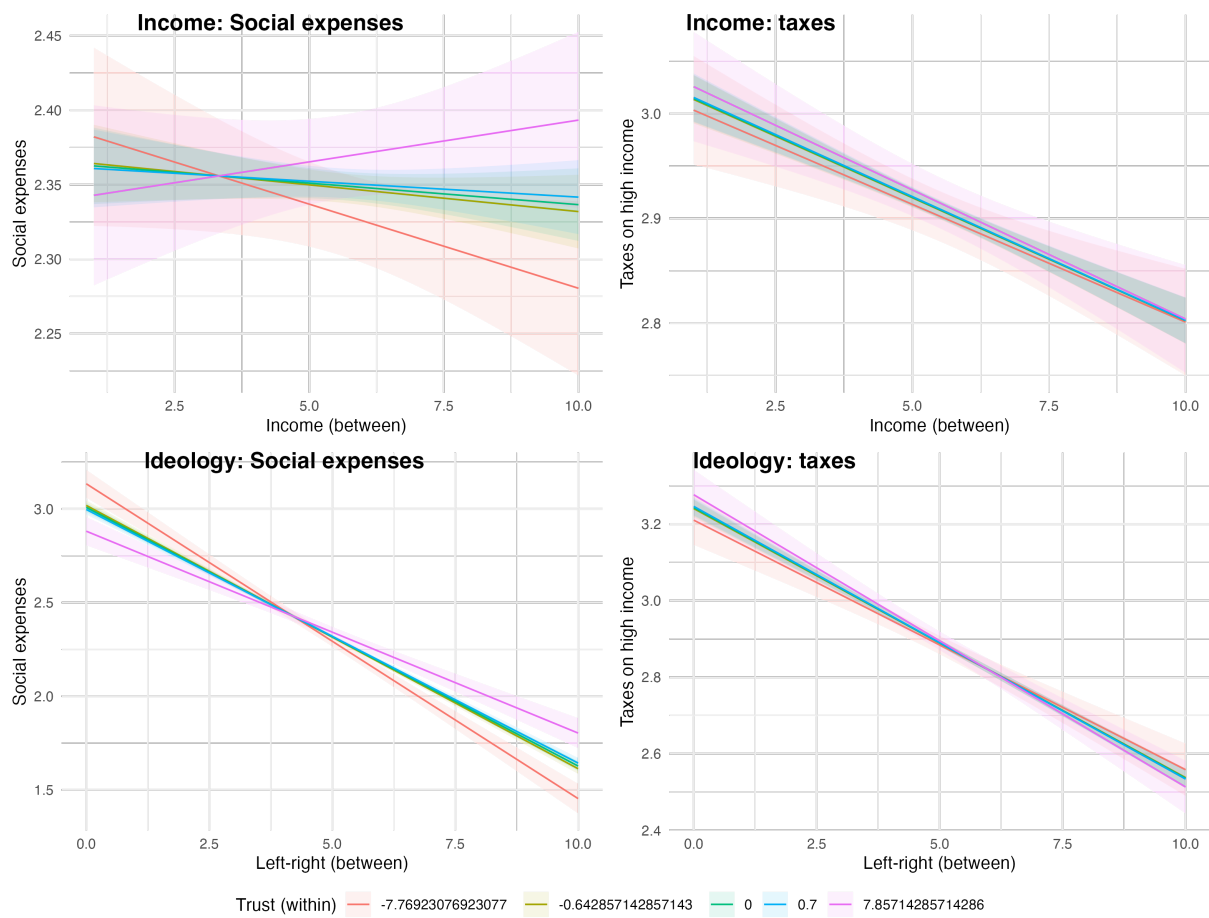
Figure 13: Continuous interactions