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**We Cannot Disagree Forever! Reality Polarization and Citizens'
Post-Pandemic Fiscal Adjustment Preferences**

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We cannot disagree forever! Reality polarization and citizens' post-pandemic fiscal adjustment preferences*

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Abstract

Recent studies have found considerable partisan polarization of both reality and policy preferences in the US. Furthermore, this polarization is so entrenched that it is immune to the provision of factual information. Has the COVID-19 pandemic, which affects citizens unequally, reproduced or disrupted these polarization patterns? To answer this question, we conducted a US-wide conjoint survey experiment with embedded information treatments. We focus on citizens' fiscal adjustment preferences given the (anticipated) lasting fiscal COVID-19 legacy. We find that both reality perceptions regarding the pandemic and policy preferences over post-COVID-19 fiscal adjustment are polarized along familiar partisan lines, but map less well onto traditional socio-economic cleavages. However, we find that the partisan policy polarization largely disappears when citizens are exposed to information on predicted COVID-19 deaths and income losses. This de-polarizing effect is due to synchronous movement by both Democrats and Republicans, which bridges the policy gap between them. Importantly, such information has the additional *reset effect* of shifting fiscal policy preferences towards a greater reliance on wealth and corporate taxes. We find further evidence that a US-wide consensus over a post-pandemic fiscal policy 'New Deal' is feasible and potentially lasting.

Keywords: COVID-19, fiscal adjustment, conjoint experiment, polarization, policy reset, partisanship

JEL codes: C90, D72, D83, H20, H30, Z13

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“[...] I don’t wear a mask like him [Joe Biden]. Every time you see him, he’s got a mask. He could be speaking 200 feet away and he shows up with the biggest mask I’ve ever seen” (Donald J. Trump, 2020 presidential debate, 29 September 2020)

“I think at some point it would require more [...] to get us back to the point where we at least recognize a common set of facts before we start arguing about what we should do about those facts” (Barack Obama, BBC interview, 16 November 2020)

1 INTRODUCTION

It is not surprising that differences in people’s policy preferences tend to map on to their respective party or political affiliations. People typically identify with a party because they agree with its policies.¹ But, in practice, the mapping can be more (or less) strong because people need not agree on all items of a party’s policy platform. When it is strong, politics becomes polarized along partisan lines because the differences in policy preferences in the population become reducible to a single dimension of party affiliation or support. Although there is some evidence that such partisan polarization has become stronger in recent years (e.g., [Campbell 2016](#)), a particularly notable, recent feature of this polarization in the US is its underpinning by a form of ‘reality polarization’. That is, people with different political affiliations also perceive reality in different ways. This has been observed with respect to policy issues like immigration, social mobility, economic inequality and taxation (see [Alesina et al. 2018a,b, 2020](#); [Kuziemko et al. 2015](#); [Stantcheva 2020](#)). Even more remarkable, this ‘reality polarization’ is such that the partisan polarization over policies appears to be immune to the provision of factual information on the policy issue ([Alesina et al. 2018b](#)). In this paper, we examine, with a conjoint survey experiment, whether the politics of COVID-19 appear to reproduce these patterns; that is, is the US similarly polarized over COVID-19? In particular, we address three questions:

¹ Of course, differences in policy preferences also map onto socio-economic cleavages (e.g., income, age).

- (i) Do differences in the perception of the reality of COVID-19 map onto partisan and/or other socio-economic cleavages? (‘reality polarization’)
- (ii) Are policy preferences with respect to the post-COVID-19 fiscal consolidation similarly polarized? (‘policy polarization’)
- (iii) Is any polarization in COVID-19 fiscal consolidation policy preferences unchanged by the provision of COVID-19 information? (‘information immunity’)

These questions are important because most of the electorate has no previous pandemic experience to draw upon when forming perceptions of reality and policy preferences with respect to COVID-19. The COVID-19 pandemic is, in this sense, a wholly new event and, hence, the natural mechanism of habit by which previous attitudes roll over into the present will not operate to the same degree. Thus, the pandemic enables us to test how entrenched the diagnosed polarization in US politics is. In other words, will COVID-19 re-set US politics (e.g. [Schwab 2020](#); [Rodrik and Stantcheva 2020](#)) or will it fit into what seems to have become the pattern of polarization as ‘business as usual’?

We focus the questions of policy preference polarization and information immunity on the specific issue of fiscal adjustment. This is, in part, because fiscal adjustment is likely to be an important and enduring issue in US politics. In the absence of pharmaceutical options, governments restricted individual freedoms to reduce the transmission of COVID-19, and responded to the economic knock-on effect of these restrictions by borrowing heavily to fund public programs of economic relief. The result has been a large increase in public debt that will create a fiscal COVID-19 legacy: the extra debt will generate either additional interest servicing and/or debt repayment costs.² The key policy question is how this future COVID-19 fiscal burden will be met. Which groups will bear the brunt of the burden? Will it be via

² Currently, with interest rates being almost zero, the rolling over of debt and the additional interest servicing payments might not look like a hefty burden on government budgets (as long as $r < g$). But there is no guarantee that interests will remain this low forever (they are at their lower bound) and, more importantly, COVID-19 has ushered deep recessions in almost all major economies. With the uncertainties surrounding the efficacy of pharmaceutical solutions, the return to growth could take even longer than anticipated. Thus, in the medium to long-run, the COVID-19 fiscal legacy will eventually kick in.

new taxes or spending cuts? If by spending cuts, where will they fall? We begin to answer these questions by examining the electorate’s policy preferences along these dimensions.

We also selected fiscal policy preferences because these preferences are characterized by disputes, especially around the issues of who should shoulder the bill (see, e.g., [Alesina and Drazen 1991](#); [Alesina et al. 2006, 2011](#); [Alesina and Ardagna 2013](#); [Fisman et al. 2020](#)) and in more ‘normal times’ fiscal policy preferences are similarly varied. For example, higher levels of redistribution are typically favored by Democratic, young, and less affluent voters (e.g., [Lupia et al. 2007](#)). Similarly, there is an ongoing debate regarding the desirability of higher wealth (and corporate) taxes (e.g., [Saez and Zucman 2019](#); [Fisman et al. 2020](#)). Thus, we have some relevant reference point for judging whether in this respect COVID-19 is at all unusual in provoking fiscal consolidation preferences that depart from these ‘normal’ patterns of fiscal policy polarization.

To answer our three research questions, we conducted a conjoint experiment with an information treatment component, embedded in a representative survey of more than 2,000 Americans. To our knowledge, we are the first to embed a further experiment in a conjoint survey experiment. Moreover, to our knowledge, this is the first conjoint survey experiment that studies fiscal adjustment preferences in the US – conjoint experiments are uniquely suited to elicit preferences on multi-faceted policy packages because they allow for the experimental manipulation of each policy element independently.

In particular, we first asked our subjects three factual questions about COVID-19 to test for ‘reality polarization’. Our subjects were then divided into three groups for the conjoint survey experiment. The control group, first of all, proceeded to the conjoint survey experiment after listening to a piece of instrumental music. The two information treatment groups received an information prompt before moving to the conjoint survey experiment. One information treatment group was exposed to the number of COVID-19 deaths in the next three months, as predicted by the Institute for Health Metrics and Evaluation (IHME). The other group received the IMF-predicted average income losses by the end of the year

2020. In the conjoint survey experiment, subjects chose between a sequence of four pairs of policy options, where each policy option has three dimensions (or attributes): (i) who should pay, (ii) what policy tool should be used to fiscally adjust, and (iii) how any spending cuts should be distributed across policy areas. Each of the three attributes was populated with one value that was randomly selected from a pool of such values. Since the subjects got to choose between pairs of fiscal adjustment packages – that is, randomly generated combinations of values on each of these attributes –, this effectively constitutes an experiment with a factorial vector of (conjoint) treatment conditions. The elicited preferences are represented by so-called average marginal component effects (AMCEs).³ We follow this terminology, but refer to these effects as the ‘main conjoint treatment effects’. This is to distinguish them from the information treatment effects which we track by comparing our control group with the two information treatment groups of subjects. It is by examining the policy preferences in the control group that we address the second question on policy polarization; and it is by comparing the preferences in the control group with those in the information treatment groups that we establish whether these patterns of polarization change in response to the provision of factual information (i.e., the ‘immunity’ question).

There are some pointers in the literature as to what we might expect to find regarding partisan polarization over these policy preferences, but they tend to pull in opposite directions since COVID-19 combines elements of a natural shock affecting life expectancy and an economic shock affecting income expectations. For example, there is evidence that people respond to localized natural disasters by acting more cooperatively (e.g., [Whitt and Wilson 2007](#); [Cassar et al. 2017](#)), and by strengthening their preferences for redistribution

³ In practice, as we set out in more detail in the next section, a conjoint survey experiment administers a vector of treatments to each subject, where each vector contains as many elements as attributes (three in our case) and each element is randomly populated by one of the pre-specified attribute values. Thus, the conjoint treatment effects that one can estimate by employing the AMCE-estimator (for its statistical properties, see [Hainmueller et al. 2014](#)) are the average treatment effects of each one of the marginal components of the vector of treatments (i.e., for each element/attribute value of the vector) on respondents’ preferences. Hence, the AMCE identifies the causal effect of changing the value of one element (known as the marginal component) in the treatment vector, *ceteris paribus*.

(Gualtieri et al. 2019). The latter may be due to disasters being perceived as bad luck (Capelen et al. 2013). At the same time, crisis-related attitudes have been shown to polarize easily in response to politicization of the crisis (Adida et al. 2020). This is what we have already observed in people’s responses to guidance on wearing masks and staying at home (e.g., Grossman et al. 2020; Wright et al. 2020). On the other hand, there is evidence that economic shocks are associated with more selfish responses (e.g., Margalit 2013; Colantone and Stanig 2018), and with moves to the right of the political spectrum (see Funke et al. 2016). Thus, our contribution is to address, in a methodologically novel way, an important open question over whether the politics of one aspect of COVID-19 is likely to reproduce the patterns of polarization that have recently been observed in US politics. In other words, could COVID-19 mark a re-set of politics away from these patterns? The question is relevant not least because the specific policy area that we consider, fiscal consolidation, often entails economic and, importantly, political conflict (Alesina and Drazen 1991; Alesina et al. 2006).

We find evidence of ‘business as usual’ in the form of partisan ‘reality polarization’ over COVID-19 and partisan fiscal policy preference polarization, particularly over ‘who pays’. However, importantly, we find no evidence in support of ‘information immunity’. Instead, the provision of both types of information *de-polarizes*: the partisan polarization of policy preference largely disappears with our information treatments. This is not the only way in which politics is re-set through information prompts in our experiment: there is a notable move away from disliking the reliance on tax increases as the main ‘policy tool’ and towards preferring such increases (levied upon companies and wealthier Americans) over a balance between tax increases and spending cuts. Moreover, our analyses suggest that this re-set is underpinned by pro-social values and economic (inequality-based) fundamentals that interact with COVID-19. In other words, the information-induced re-set maps back onto real outcomes, thus making the persistence of our effects even more likely. Finally, we show that of the different combinations of fiscal adjustment elements, it is the packages that rely on tax increases, combined with the wealthy and companies bearing most of the burden,

that attract most support among Americans; in fact, they may command majority support. We conclude that a post-pandemic ‘New Deal’ along those lines is politically feasible, albeit still uncertain.

The paper proceeds as follows. In the next section, we set out our design of the conjoint survey experiment. Section 3 presents and discusses the results, while Section 4 discusses the underlying mechanisms. We conclude the paper in Section 5.

2 EXPERIMENTAL DESIGN

Our survey experiment was undertaken online between 17 and 21 April 2020. We recruited 2,245 US subjects via Prolific Academic (full details below), who all received a payment based on the time it took to complete the survey.⁴ They began by answering three questions related to the reality of the COVID-19 pandemic:

- (i) How many people in the US would you estimate will die in total due to coronavirus?
- (ii) By what percentage would you estimate average income in the US will be lower in 2020 as compared to 2019?
- (iii) How serious do you think COVID-19 is compared to the seasonal flu?

They answered the first two questions by supplying a number, whilst they could answer the third question on a scale from 1 to 5. Subsequently, they were divided into three groups. The two information treatment groups received one of two information prompts, while the control group heard a piece of instrumental music. The two information treatments (T1 and T2) took the following form:

Information about COVID-19 deaths (T1): *The Washington-based Institute for Health Metrics and Evaluation (IHME) predicts that – with the current government guidance in place – about 68,841 people in the US will have died due*

⁴ More specifically, the payment they received was based on the average completion time of their survey subgroup. The overall average completion time was 34 minutes.

to the coronavirus by August 4. This means that the number of COVID-19 deaths per one million people would be 210.

Economic information (T2): *The International Monetary Fund (IMF) expects the US economy to shrink by 5.9% in 2020 compared with 2019. This estimated loss of 5.5% equates to a loss of around \$3848 per person in 2020 compared with 2019.*

2.1 Conjoint experiment

Conjoint survey experiments have long been used in sociology (Wallander 2009) and marketing research (Green et al. 2001) and are now also popular in other areas of the social sciences. Their advantage over other preference elicitation techniques lies in their ability to measure the impact of treatments with multiple dimensions (Hainmueller et al. 2014). Such survey experiments present respondents with a series of pairs of alternatives (policy packages) consisting of multiple (three in our case) attributes, where the attribute values are assigned randomly.⁵ For each pair, respondents are asked to provide some sort of score, with typical outcomes being the discrete choice of an alternative or the more fine-grained rating of the alternatives (Hainmueller et al. 2014, 5-7). Thus, conjoint analysis allows for the simultaneous identification and estimation of the effect of different attribute values on particular preference outcomes.

As government policies tend to be multi-dimensional, conjoint survey experiments are well-suited to measure people’s preferences over different options. By incorporating both the different attributes of a policy and the range of values on these attributes, the experiments allow for the identification of those values that drive up (or down) support for the overall policy (or policy package). Indeed, conjoint analysis has been used to study citizens’ policy

⁵ The order of the attributes is also typically randomized, but respondents always observe the full range of attributes, whilst each time, *only one element* from the full list of attribute values is *randomly assigned to each of the attributes*, with respondents observing two ‘packages’ of values at any one time. See Figure A3 in the Appendix for an example of a pair of packages from our survey.

preferences in areas such as climate agreements (Bechtel and Scheve 2013), financial bail-outs (Bechtel et al. 2017), and income tax (Ballard-Rosa et al. 2017).

In our conjoint survey experiment, we distinguish three attributes of post-pandemic fiscal adjustment: (i) who bears the financial burden (e.g., all in proportion to income; the wealthy proportionally more), (ii) the policy tool to fund the financial burden (in terms of taxation and spending cuts), and (iii) policy areas for any spending cuts (e.g., defense, health care, foreign aid).⁶ The attributes have three to eight values. Table 1 presents the three attributes and their values.

Table 1: Attributes and attribute values in the conjoint survey experiment

Attribute	Attribute values (treatment elements)
Who bears the financial burden?	<ul style="list-style-type: none"> – People contribute in proportion to their income – Older people bear proportionally more of the financial burden – Wealthier people bear proportionally more of the financial burden – Companies bear more of the financial burden
Policy tool to fund the future financial burden	<ul style="list-style-type: none"> – Mainly increasing taxes, but also some cuts in government spending – An equal balance of spending cuts and tax increases – Mainly cutting spending, but with some tax increases
Policy areas for any spending cuts	<ul style="list-style-type: none"> – Roads and public transport – Health care – Social security – Pensions – Defense – Foreign aid – Environmental policy – Research and development

⁶ Including three attributes is well within the limit of what can be included without losing response quality (Bansak et al. 2019).

We introduce our respondents to the post-pandemic fiscal adjustment debates with the following text:⁷

“Policies that mitigate the economic dislocation from the measures to reduce Covid-19 transmission – for instance, salary replacement and cheap loans – will initially increase the national debt. This will create future financial burdens: either the new debt will have to be repaid or interest payments will have to be made on this new debt. We would like you to assess below who should shoulder this future financial burden and what policy tool should be used to raise the funds to do this in the future. We combine the various options into financial packages below.”

Our conjoint survey experiment includes a range of commonly discussed fiscal adjustment options. First, we analyze whether the old, the wealthy, or companies should shoulder the burden as compared with everyone sharing the burden in proportion to their income. Second, we assess whether adjustment should mainly take the form of tax increases or spending cuts, compared with a balanced combination of the two. Third, we include a number of policy areas which may be affected by any spending cuts, including health, social security, defense and foreign aid. The first and third attribute relate to the redistributive character of citizen preferences in relation to fiscal policy. Thus, preferring that the wealthy shoulder the burden rather than the old is redistributive, as is a preference not to have any cuts fall on the ‘equality of opportunity’ areas of health and social security. The second attribute relates to preferences over the size of the state. Thus, a preference for tax increases as opposed to spending cuts is a preference for a larger role for the state as compared with the market. We focus on people’s redistributive and state/market preferences because policies are typically

⁷Our introductory statement was carefully phrased so as to avoid imposing on respondents the necessity of enacting a fiscal adjustment plan imminently. Moreover, the use of an 1-7 Likert scale to elicit respondents’ preferences allows for those who completely reject the need for any such fiscal adjustment plan (now, or in the future) to simply *assign the same lowest score* (1) to all presented packages. In fact, this is an additional reason that we prefer to use the Likert scale when presenting our main findings in Section 3.

distinguished in this way, and they map onto familiar political differences, with left-wing parties preferring more redistribution and a bigger role for the state than right-wing parties.

The values were chosen to reflect the alternatives discussed in actual policy debates across advanced economies, which facilitates respondents’ understanding of the options (cf. [Gallego and Marx 2017](#), 1035). For instance, in April 2020, when we fielded our survey, the role of multinational companies in post-pandemic fiscal adjustment was already discussed (e.g., [Turner 2020](#)), and broader reform of fiscal policy was called for. Indeed, even the editors of the [Financial Times](#) (2020) argued that “[p]olicies until recently considered eccentric, such as basic income and wealth taxes, will have to be in the mix.” Our values on ‘policy areas for any spending cuts’ resemble those included in questions about federal budget spending in the American National Election Study (ANES), and they represent areas that were singled out during the pandemic – some for reasons of fiscal protection (health, environmental policy), others as areas for economic stimulus (roads and public transport), and again others in the context of potential cuts (e.g., defense, foreign aid).

In the survey, we presented each respondent with four pairs of alternatives with randomly assigned attribute values. We also randomized the attribute order (between subjects). After being shown a pair of policy packages, respondents were asked to perform two tasks, with the answers serving as outcome variables. First, for each package, respondents were asked to rate on a Likert scale from 1 to 7 how supportive they would be of the policy package being implemented. These data were used to construct a rating outcome variable.⁸ Second, still observing the same pair of packages, respondents were asked to choose the one package they would prefer (forced choice). This resulted in a (forced) choice outcome variable, which was coded as 1 for the preferred packages and 0 for the rejected ones. Figure [A3](#) in the Appendix

⁸ The distribution of this variable is presented in Figure [A4](#) in the Appendix. In the analyses, the 1-7 Likert scale was re-coded to range from 0 (fully oppose) to 1 (fully support) for ease of interpretation of the results. We use the Likert-based outcome variable as our main DV in the analyses presented in Section [3](#) because it contains finer information and allows for ties. We replicate all our main results using the binary outcome variable in the Appendix.

displays an example pair of packages, along with the two outcome variables of the conjoint experiment.

To identify the causal effects of the fiscal adjustment attribute values on the likelihood that they are preferred, we leverage the difference in attributes between distinct policy package preferences. We estimate average marginal component effects (AMCEs) using the following OLS regression:

$$Y_{ijk} = a_0 + \gamma_k + \delta \mathbf{T}_{ij} + v_{ijk} \quad (1)$$

where $\mathbf{T}_{ij}$ is a treatment vector (containing three randomly assigned values, one for each of the policy’s attributes) that indicates whether (or not) a policy package has a particular attribute value, and Y_{ijk} is the outcome variable (Likert scale and binary). We cluster the standard errors by respondent i . We also use quota-based and (manually targeted) entropy balancing weights in our preferred specifications to further ensure that our estimates are representative of the U.S. general (adult) population. In additional specifications we also use U.S. state fixed effects as a robustness check (we report those results in Table E2 in the Appendix). In our experiment, respondents were asked to choose between $j = 2$ alternative policy packages in each of their $k = 4$ choice tasks. In total, our experiment had 18,116 policy profiles being randomly generated.

2.2 Information treatment

The COVID-19 death estimates were taken from the Institute for Health Metrics and Evaluation (IHME 2020a).⁹ The income loss estimates for 2020 came from the IMF (2020). Both sets of estimates were published in the week preceding our field work.

To identify the causal effects of the information treatments on respondents’ preferences over the different attributes of the fiscal adjustment packages, we ran the following regression:

⁹We now know that the numbers were hugely underestimated for the US.

$$Y_{ijk} = a_0 + \gamma_k + \delta_1 \mathbf{I}_1 \mathbf{T}_{ij} + \delta_2 \mathbf{I}_2 \mathbf{T}_{ij} + v_{ijk} \quad (2)$$

where I_1 and I_2 are binary indicating whether subjects were assigned to one of the two treatment groups or the control group, T_{ij} is the treatment vector indicating whether a policy package has a particular attribute element, and Y_{ijk} is the outcome; δ 's capture the causal estimates of our treatment effects. Again, we made similar decisions as above regarding clustering, the use of weights and fixed effects in our preferred specifications.

2.3 Survey data and sampling

Our subjects were representative in terms of age, gender, region and work status in the US.¹⁰ We recruited participants via Prolific Academic, a web-based panel with over 40,000 active participants (and well over 100,000 participants in total), primarily in the US and the UK. Our quota-based sample was recruited between 17 and 21 April 2020. This was at the end of the week in which the US was predicted to hit peak deaths (IHME 2020b). To generate samples, we used the US Current Population Survey (US Census Bureau 2018). We created a total of 170 subgroups weighted based on age, gender, region and work status. By 21 April, 2,245 respondents had participated. This was close to our target of 2,500.¹¹ Further details on the survey are included in Section A of the Appendix.

3 RESULTS

We first present our findings on reality polarization and then turn to questions of policy preference polarization and the possible information immunity of these polarities.

¹⁰ The survey was pre-registered via the EGAP Pre-Analysis Registry (20200416AB) and registered as a minimal risk study by the King's Research Ethics Committee under REC ref. MRSP-19/20-18237.

¹¹ There were a few subgroups for which we did not completely fill our quotas; see Tables A1 and A2 in the Appendix. This was due to not enough participants over the age of 60 being registered on Prolific. In the main analyses, we use weights to ensure representativeness on these four dimensions. We also used entropy balancing to ensure representativeness in other dimensions (see Section A for more detail).

3.1 Reality polarization in COVID-19 perceptions

Table 2 reports mean values by subgroups for the three reality questions. We compare perceptions across Trump and Clinton voters, Democrats and Republicans as well as across income and age categories. We added the latter two for comparison because they reflect, respectively, a traditional class-based source of political difference and an intergenerational one, with the latter having become salient during the pandemic as there is evidence that the young are more likely to suffer economic losses from COVID-19, whereas the old are more likely to experience the health risks of COVID-19. Overall, we find strong evidence of reality polarization across the political variables and less evidence across income and age groups.

In particular, the Democrats and Clinton voters have estimates of COVID deaths that are, on average, double those of, respectively, Republicans and Trump voters; and these differences are statistically significant. There are similarly significant differences in their perception of the seriousness of COVID-19 compared with seasonal flu: Trump and Republican voters are more inclined than Clinton and Democratic voters to think that COVID-19 is no more serious than seasonal flu. The high and low income earners do not exhibit any difference in this perception of the relative seriousness of COVID-19. Yet, they share a difference in their estimates of COVID-19 deaths that is significant; although the difference is smaller in magnitude than in the case of the political variables.

Unlike the political variables, there are significant differences in estimated income losses between high and low income earners. This may be because low income recipients have actually suffered proportionately larger personal income losses than high income ones and they have projected their own experience on to estimates for national average loss. In this sense, the difference in perception may be associated with people’s differential point of view. It also points to the possibility of people on lower incomes experiencing higher levels of economic anxiety and fear – a feature that is consistent with the unequal economic impact of the pandemic across income groups. For this reason, and the fact that the difference in

COVID-19 death estimates is smaller, we conclude that reality is marginally more polarized for the political variables than for income groups.

This difference is clearer when we compare perceptions across age. The COVID-19 death estimates are weakly significantly different between the old and the young, but there are no other significant differences in the perception of COVID-19 reality. Taken together, the findings suggest that reality polarization is more likely to ensue when politics become unhinged from traditional socio-economic cleavages.¹² Result 1 then follows:

RESULT 1 (in support of ‘reality polarization’): Reality polarization exists for the political variables. This reality polarization is, respectively, marginally and clearly stronger for the political variables than for the income and age variables.

¹² We note the interesting implication that, if these differences survived a change to political differences that mapped more closely either onto the traditional source of party allegiance in class or the putative new inter-generational source of politics, this would mean ‘reality polarization’ diminished. Or to put this round the other way, it is possible that it is the decline of class-based politics, and the failure of a generation-based politics to emerge, that has contributed to ‘reality polarization’ in the US.

Table 2: Polarization of Reality

	Party			2016 Vote		
	Rep	Dem	T-statistic	Trump	Clinton	T-statistic
Death estimate	151,487 (320,982)	352,614 (688,099)	6.48*** (1,141)	156,169 (322,365)	342,193 (655,376)	5.57*** (954)
Income loss estimate	25.63% (19.08%)	26.62% (19.12%)	0.90 (1,615)	26.82% (19.89%)	26.51% (18.95%)	-0.27 (1,330)
Seriousness	2.473 (0.714)	2.894 (0.347)	11.31*** (1,705)	2.430 (0.744)	2.905 (0.341)	12.00*** (1,400)
Observations	475	1,246	-	444	973	-
	Income			Age		
	High	Low	T-statistic	Below 25	Above 65	T-statistic
Death estimate	243,301 (493,341)	424,842 (728,111)	3.02*** (472)	430,868 (863,532)	281,486 (623,611)	1.95* (352)
Income loss estimate	24.14% (16.78%)	32.73% (21.48%)	5.56*** (667)	24.39% (20.24%)	25.76% (15.92%)	-0.90 (541)
Seriousness	2.696 (0.598)	2.762 (0.531)	1.48 (707)	2.792 (0.534)	2.756 (0.525)	0.80 (573)
Observations	389	327	-	662	686	-

Notes: The table reports the mean values for each subgroup. Seriousness ranges from 0-4 with higher values indicating more concern and seriousness. High income respondents are those with an annual household income above \$100,000, low income respondents are those with an annual household income below \$30,000. Asterisks indicate significant differences in mean values between two groups from a Wald test of significance. *** p<0.01, ** p<0.05, * p<0.1.

3.2 Conjoint analysis: Policy polarization in the control group

A conjoint is essentially a fully randomized, factorial experiment involving a large number of factors and factor levels. Analyzing conjoint experiments is not as straightforward as other types of experiments because the number of potential combinations of attribute values (in our case, the unique policy profiles) is very large compared to the number of observations. Nonetheless, as our design relied on randomization of values for attributes, we can estimate treatment effects because the variation in one attribute is orthogonal to that of other attributes. Here we follow [Hainmueller et al. \(2014\)](#) by identifying the AMCEs. In our case, this means the average change in the probability of a fiscal adjustment policy being preferred by US subjects when we compare an attribute value to the reference value (e.g., pension cuts versus health care cuts), assuming the other attributes take their average values (i.e., the policies have identical attributes except for the marginal one where the value changes).¹³

To test for the existence of partisan policy polarization, we analyze whether the estimated AMCEs are different for Republicans and Democrats. Figure 1 gives the AMCE by party political affiliation (see also Appendix Table E4). Two significant differences are revealed with the first attribute of ‘who pays?’. Democrats are more likely to prefer a policy when ‘companies’ and the ‘wealthy’ shoulder the burden (as compared with sharing being proportionate to income) than are Republicans – the two coefficients for Democrats are not only statistically different from the respective ones for Republicans (see Table 3, column 10 for the relevant t-tests) but they are also statistically different from zero. As regards the second attribute of ‘policy tool’, there is a significant difference between Republicans, who prefer a package of tools weighted towards spending cuts (as compared with a balance of tax increases and spending cuts) and Democrats, who do not (i.e., they dislike this weighting to spending cuts). Again the coefficient for Democrats is not only statistically different from

¹³ Unless otherwise noted, all regression models employ Likert scale re-coded to range from 0 to 1 as the dependent variable. Regressions with the (binary) forced choice outcome are presented in Section F in the Appendix.

that for Republicans but also from zero. Figure 2 presents similar results, but this time for Trump and Clinton voters (see also Appendix Table E5). Again a significant difference in preferred policy tool is revealed (see Table 4, column 10 for the relevant t-tests). Finally, with respect to where spending cuts should fall (the third attribute), Democrats have a significantly stronger preference than Republicans for these cuts to fall on defense and pensions.

In brief, the heterogeneity (sub-group) analysis based on party affiliation demonstrates stark partisan differences in fiscal adjustment preferences. Moreover, these differences are familiar ones: as expected, Democrats (and Clinton voters) are prefer those policy features that are more redistributive (e.g., targeting the wealthy and companies) and that attribute a bigger role to the state (e.g., aversion to spending cuts though, unsurprisingly, Democrats have a strong preference for defense spending cuts).

Nonetheless, these differences have not prevented some aggregate preferences from emerging. Opposition to health spending cuts and passing the burden on the elderly is a strong preference for all. Perhaps somewhat unexpectedly, but certainly in line with the fiscal policy discourse in the U.S. (see e.g. Stantcheva 2020; Jeffrey and Matakos 2020), both Republicans (Trump voters) and Democrats (Clinton voters) agree in their opposition to tax financed fiscal adjustments –both coefficients are negative (significant at the 5% level) and statistically indistinguishable (see Figures 1 and 2 and Tables 3 and 4).

Figure 1: Conjoint treatment effects (AMCEs), by political party (no-info group)

Figure 2: Conjoint treatment effects (AMCEs), by 2016 vote (no-info group)

Notes: The plots show estimates of the effect of randomly assigned values of fiscal adjustment attributes on the probability of a policy package to be preferred. Each panel represents an attribute. The reference values on each attribute are the following, respectively: (1) people contribute in proportion to their income, (2) an equal balance of spending cuts and tax increases, and (3) health care. Tables E4 and E5 in the Appendix display the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

Table 3: Polarization and informational (de)polarization between Democrats and Republicans

	<i>Democrats estimates</i>			<i>Republicans estimates</i>			<i>Difference in estimates</i>			<i>t-statistic</i>		
	Control	T1	T2	Control	T1	T2	Control	T1	T2	Control	T1	T2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Companies to bear the burden	0.072	-0.000	-0.020	0.015	-0.014	0.012	0.057	0.014	-0.032	2.290**	0.370	-1.180
Older people to bear the burden	-0.138	0.023	-0.000	-0.153	0.029	0.075	0.015	-0.006	-0.075	0.536	-0.136	-1.854*
Wealthy to bear the burden	0.101	0.007	0.001	0.016	0.013	0.051	0.085	-0.006	-0.05	3.461***	-0.167	-1.305
Cut spending	-0.027	0.019	0.020	0.020	-0.024	-0.010	-0.047	0.043	0.03	-2.115***	1.360	1.026
Increase taxes	-0.025	0.019	0.028	-0.052	0.019	0.027	0.027	0	0.001	1.308	-0.023	0.006
Cut defense spending	0.234	-0.072	-0.050	0.046	-0.097	-0.026	0.188	0.025	-0.024	4.824***	0.390	-0.469
Cut environment spending	0.069	-0.027	-0.055	0.088	-0.080	-0.045	-0.019	0.053	-0.01	-0.471	0.957	-0.188
Cut foreign aid spending	0.179	-0.060	-0.029	0.157	-0.120	-0.042	0.022	0.06	0.013	0.543	1.006	0.236
Cut R&D spending	0.108	-0.017	-0.029	0.114	-0.096	-0.068	-0.006	0.079	0.039	-0.173	1.496	0.828
Cut pensions spending	0.068	-0.049	-0.019	-0.004	-0.018	-0.057	0.072	-0.031	0.038	1.990**	-0.543	0.736
Cut social security spending	0.007	-0.022	-0.040	-0.063	-0.061	-0.018	0.07	0.039	-0.022	1.720*	0.676	0.286
Cut public transport spending	0.129	-0.049	-0.046	0.084	-0.080	-0.016	0.045	0.031	-0.03	1.244	0.576	-0.596

Notes: The first six columns show coefficients. Columns (7), (8) and (9) show differences in estimates between Democrats and Republicans. Columns (10), (11) and (12) display the t-statistics. Asterisks indicate significant differences in coefficients between two groups from a Wald test of significance. Number of observations: Democrats N=1248 and Republicans N=479. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Polarization and informational (de)polarization between Clinton and Trump voters (2016)

	<i>Clinton voters estimates</i>			<i>Trump voters estimates</i>			<i>Difference in estimates</i>			<i>t-statistic</i>		
	Control	T1	T2	Control	T1	T2	Control	T1	T2	Control	T1	T2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Companies to bear the burden	0.082	-0.009	-0.030	0.024	-0.012	-0.011	0.058	0.003	-0.019	2.059**	0.084	-0.505
Older people to bear the burden	-0.145	0.040	0.017	-0.154	0.036	0.017	0.009	0.004	0	0.262	0.113	0.006
Wealthy to bear the burden	0.102	0.015	0.006	0.019	0.032	0.022	0.083	-0.017	-0.016	2.887***	-0.444	-0.404
Cut spending	-0.018	0.001	0.012	0.035	-0.028	-0.009	-0.053	0.029	0.021	-2.138**	0.913	0.707
Increase taxes	-0.025	0.026	0.017	-0.034	0.000	0.012	0.009	-0.025	0.005	0.382	0.829	0.160
Cut defense spending	0.221	-0.051	-0.031	0.053	-0.065	-0.013	0.168	0.286	-0.018	3.744***	0.209	-0.342
Cut environment spending	0.039	0.005	-0.021	0.102	-0.102	-0.057	-0.063	0.141	0.036	-1.357	1.926	0.622
Cut foreign aid spending	0.152	-0.026	-0.028	0.163	-0.100	-0.006	-0.011	0.074	-0.022	-0.249	1.262	-0.379
Cut R&D spending	0.104	-0.011	-0.005	0.124	-0.074	-0.091	-0.02	0.063	0.086	-0.506	1.184	1.814
Cut pensions spending	0.050	-0.043	-0.029	0.012	-0.026	-0.054	0.038	-0.017	0.025	0.916	-0.296	0.484
Cut social security spending	-0.038	0.005	-0.017	-0.050	-0.036	-0.041	0.012	-0.002	0.024	0.267	0.899	0.406
Cut public transport spending	0.106	-0.030	-0.027	0.073	-0.034	-0.002	0.033	0.14	-0.029	0.791	0.077	-0.507

Notes: The first six columns show coefficients. Columns (7), (8) and (9) show differences in estimates between Clinton and Trump voters. Columns (10), (11) and (12) display the t-statistics. Asterisks indicate significant differences in coefficients between two groups from a Wald test of significance. Number of observations: Clinton voters N=976 and Trump voters N=450. *** p<0.01, ** p<0.05, * p<0.1.

Figure 3: Average marginal component effects at the baseline (no-info group)

Notes: The plot shows estimates of the effect of randomly assigned values of fiscal adjustment attributes on the probability of a policy package to be preferred. Each panel represents an attribute. The reference values on each attribute are the following, respectively: (1) people contribute in proportion to their income, (2) an equal balance of spending cuts and tax increases, and (3) health care. Table E1 displays the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

This ‘agreed’ bi-partisan opposition to fiscal adjustments that mainly rely on tax increases (and putting the burden on the elderly) is so pronounced that is visible when we consider aggregate preferences at the baseline (no-info group) in Figure 3 (see also Table E1), which also include independents and non-affiliated voters.¹⁴ This documented aversion to tax increases, in particular, most likely reflect the widespread policy consensus in the US over the last decades, with tax increases being portrayed as key obstacles to growth and employment

¹⁴ As a robustness check, we also implemented entropy balancing to fully balance our sample in terms of partisan affiliation. We do not observe any major differences in the aggregate preferences when we use entropy-adjusted weights. Also note that at the baseline, fiscal adjustment plans having companies and the wealthy carry most of the burden receive more support, but those aggregate preferences are solely driven by Democrat (and Clinton) voters. Section A provides more detail to this method. Figure A5 and Table E3 report regression results with partisanship-adjusted weights.

(see Tables G1 and G2, for similar t-tests for fiscal preference questions in the 2016 American National Election Study (ANES) by partisanship, income and age).

Finally, moving to the other socio-economic cleavages, Figures C1 and C2 in the Appendix reproduce the analyses above for different age and income groups. We do not observe any significant polarization of fiscal preferences across income groups; as far as age is concerned, we see that older people (above 65 years) are less likely to support policies when these put the burden on them (all groups dislike this option) and when social security is cut. This limited policy polarization echoes Result 1 for these groups (limited reality polarization across income and age groups).¹⁵ Result 2 is, therefore, the following:

RESULT 2 (in support of ‘partisan policy polarization’): There is partisan polarization over the COVID-19 fiscal adjustment preferences with respect to a) who pays (i.e. over whether the wealthy and companies should shoulder the burden), b) the policy tool (i.e. over whether greater reliance should be placed on spending cuts) and c) where cuts should fall (i.e. whether defense should be cut). This COVID-19 polarization reproduces the normal patterns of partisan fiscal policy preference polarization. The fiscal policy adjustment preferences are notably less polarized across income and age groups.

¹⁵ Figures D1 and D2 show that when we divide up our sample in groups whose income is affected differently by the pandemic, and groups with different levels of tolerance for inequality, there are, again, very few differences between the groups.

3.3 Information immunity: Information treatments versus control

To test for the influence of information provision on policy polarization, we compare the (polarized) preferences in the control group with the same preferences in our information treatment groups. The most striking result is that many of the specific partisan policy polarizations disappear; that is, information appears to have a de-polarizing effect on partisan differences and, in this respect, COVID-19 politics marks a departure from ‘business-as-usual’.

We reproduce the control group analysis of AMCEs for the information treatments groups. To explore whether the policy preference polarization (among Democrat/Clinton voters and Republican/Trump voters) that we found in the control group is still present (column 10 in Tables 3 and 4), we analyze the significant differences between the AMCEs (on various attributes, both on who pays and also what fiscal tools to employ) when information is provided (Treatments 1 and 2). In other words, we conduct a pairwise comparison of those coefficients (between Democrats and Republicans) for each information treatment in order to examine what polarities survive. As we can observe, none of those partisan differences are statistically different in the two treatment groups (see columns 11 and 12 in Tables 3 and 4). In other words, the de-polarization is universal, and it is mostly due to movements by Democrat/Clinton voters in the direction of Republican/Trump voters, with one notable exception which we address below.

Figures 4 and 5, combined with Tables 3 and 4 (in previous subsection 3.2), further examine whether there are any differences in the de-polarizing effect of information between Democrats and Republicans and between Trump and Clinton voters, respectively (for the underlying regression results, see Tables E4 and E5). The significant differences between the control and the information treatments typically arise in both information treatments. Democrats move away from preferring that companies and the wealthy should shoulder the burden relative to everyone paying in proportion to their income. There is no significant movement in preferences among Republicans with respect to companies or the wealthy,

which is consistent with the de-polarization finding above. It is relevant, however, that both Democrats and Republicans shift their preferences with respect to whether older people should shoulder the burden. With the COVID-19 deaths prompt, both partisan groups move towards supporting policies where older people pay more. The same is the case for Republicans with the income loss information, but Democrats move marginally in the opposite direction in this case. Both Democrats and (particularly) Republicans also notably shift towards tax increases as the policy tool after the income loss information. Finally, while both Democrats and Republicans become less likely to want to cut most categories of spending, the turnaround is most marked in the case of Democrats over defense spending.

Figures C3 and C4 in the Appendix plot the information treatment AMCEs by the other possible income and age cleavages that we considered in the control. When looking at income groups, we see that the pro-social preferences of wealthier Americans (those with income > \$100k)¹⁶ that we found in the baseline (i.e., support for placing the burden on companies and themselves) is reversed, *but only* when the group receives information about COVID-19 related economic losses (differences are significant; $p < 0.001$ ¹⁷). In other words, selfish considerations of rich come to the fore and overshadow any pro-social attitudes they might have had: now, they are alone in opposing placing the burden on rich Americans and companies. Nothing else was polarized in the baseline and this remains the case after receiving information.

A similar pattern appears when we examine fiscal preferences across the different age groups. There was no significant polarization to begin with other than middle-aged and older people (> 50 years) being more likely to reject policies that impose a burden on themselves (though all age groups opposed this option) and this now disappears after the information treatments (differences are significant at $p=0.005$ under deaths information and

¹⁶ Those earning above \$100k in the US accounted for about 10% of the US population in 2019.

¹⁷ See Column 4 in Table E6 for corresponding coefficients; N=389.

at $p=0.002$ under income losses information).¹⁸ In other words, relatively older people are now equally – if not more – likely to support placing the burden on themselves compared to relatively younger segments of the population. In sum, for both income and age, we do not observe significant information treatment effects on preference de-polarization (because there was little polarization to begin with), with the exception of a (pro-social) de-polarizing movement by the older and a polarizing one by the wealthier. Therefore, Result 3 reads as follows:

RESULT 3 (against ‘information immunity’): Partisan policy polarization in the control disappears in both information treatments. There is no attribute over which partisan polarization remains. This de-polarization arises because information has a more significant effect on Democrat and Clinton voters than Republicans and Trump voters. Not unsurprisingly with less policy polarization among age and income groups, the information provision has negligible effects on the polarization within these groups.

¹⁸ See Column 4 and 5 in Table E7 for corresponding coefficients; $N=867$.

Figure 4: Information treatment effects, by party

Figure 5: Information treatment effects, by presidential vote

Notes: The plot shows estimates of the effect of randomly assigned values of fiscal adjustment attributes on the probability of a policy package to be preferred. Each panel represents an attribute. The reference values on each attribute are the following, respectively: (1) people contribute in proportion to their income, (2) an equal balance of spending cuts and tax increases, and (3) health care. Tables E4 and E5 display the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

It is apparent from Figures 4 and 5 (and Tables E4 and E5) that the information provision not only had a partisan de-polarising effect, but also shifted preferences with respect to some attributes in the same direction for both Republicans and Democrats (and the same is true for Trump and Clinton voters). To bring out these aggregate effects of information provision on policy preferences, we compare the revealed aggregate policy preferences in the control with those in the information treatments (see Figure 6 and Table E1).

With respect to the first attribute of ‘who pays’, there was a clear aggregate preference for the companies and the wealthy to pay, relative to people paying in proportion to their income, and this disappears in both information treatments. This is not surprising given what we know above about how Democratic preferences in these areas moved closer to those of Republicans. With respect to the second attribute of ‘policy tool’, there is a notable information effect in the aggregate as individuals shift from disliking the predominant use of ‘tax increases’ to actually preferring this policy tool. Finally, with respect to the third attribute of ‘where spending cuts should fall’, the information reverses the clear preferences that existed in the control group for almost all categories of expenditure to be cut. What is also notable is that, in the control group, individuals in the aggregate had a stronger preference for cutting defense and foreign aid than R&D, and a stronger preference for cutting R&D than pensions, whilst pension cuts were themselves preferred over social security ones. In other words, there was a clear ordering in terms of where cuts should fall. By contrast, under the information treatments, there is no such hierarchy as all spending areas are equally preferred for cuts relative to health care. This is another testament to the de-polarizing effect that information provision had because the clear ordering for the control group was, for instance, in line with the strong preferences of Democrats for defense cuts.

The wholesale switch, however, from disliking a predominant reliance on tax increases to actually preferring such a reliance cannot be accounted for by de-polarization. The switch is all the more remarkable because fiscal policy preferences in ‘normal times’ tend to be what we found in the control group: both Democrats and Republicans expressed a clear aversion

Table 5: Informational reversals within Democrats

	<i>Estimates</i>			<i>Differences in estimates</i>		<i>p-value</i>	
	Control	T1	T2	Control-T1	Control-T2	Control-T1	Control-T2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Companies to bear the burden	0.072	-0.000	-0.020	0.072	0.092	0.026**	0.004***
Older people to bear the burden	-0.138	0.023	-0.000	-0.161	-0.138	<0.001***	<0.001***
Wealthy to bear the burden	0.101	0.007	0.001	0.094	0.1	0.004***	0.002***
Cut spending	-0.027	0.019	0.020	-0.046	-0.047	0.070*	0.058*
Increase taxes	-0.025	0.019	0.028	-0.044	-0.053	0.078*	0.029**
Cut defense spending	0.234	-0.072	-0.050	0.306	0.284	<0.001***	<0.001***
Cut environment spending	0.069	-0.027	-0.055	0.096	0.124	0.031**	0.006***
Cut foreign aid spending	0.179	-0.060	-0.029	0.239	0.208	<0.001***	<0.001***
Cut R&D spending	0.108	-0.017	-0.029	0.125	0.137	0.005***	0.002***
Cut pensions spending	0.068	-0.049	-0.019	0.117	0.087	0.015**	0.06*
Cut social security spending	0.007	-0.022	-0.040	0.029	0.047	0.527	0.289
Cut public transport spending	0.129	-0.049	-0.046	0.178	0.175	<0.001***	<0.001***

Notes: The first three columns show coefficients. Columns (4) and (5) show differences in estimates. Asterisks indicate significant differences in coefficients between two groups from a Wald test of significance. N=1248. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Informational reversals within Republicans

	<i>Estimates</i>			<i>Differences in estimates</i>		<i>p-value</i>	
	Control	T1	T2	Control-T1	Control-T2	Control-T1	Control-T2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Companies to bear the burden	0.015	-0.014	0.012	0.029	0.003	0.523	0.962
Older people to bear the burden	-0.153	0.029	0.075	-0.182	-0.228	<0.001***	<0.001***
Wealthy to bear the burden	0.016	0.013	0.051	0.003	-0.035	0.959	0.448
Cut spending	0.020	-0.024	-0.010	0.044	0.03	0.300	0.458
Increase taxes	-0.052	0.019	0.027	-0.071	-0.079	0.067*	0.040**
Cut defense spending	0.046	-0.097	-0.026	0.143	0.072	0.068*	0.302
Cut environment spending	0.088	-0.080	-0.045	0.168	0.133	0.025**	0.075*
Cut foreign aid spending	0.157	-0.120	-0.042	0.277	0.199	0.001***	0.009***
Cut R&D spending	0.114	-0.096	-0.068	0.21	0.182	0.001***	0.002***
Cut pensions spending	-0.004	-0.018	-0.057	0.014	0.053	0.836	0.417
Cut social security spending	-0.063	-0.061	-0.018	-0.002	-0.045	0.981	0.569
Cut public transport spending	0.084	-0.080	-0.016	0.164	0.1	0.015**	0.122

Notes: The first three columns show coefficients. Columns (4) and (5) show differences in estimates. Asterisks indicate significant differences in coefficients between two groups from a Wald test of significance. N=479. *** p<0.01, ** p<0.05, * p<0.1.

to tax hikes (both coefficients were negative and significantly different from zero); this is now reversed as both groups support/do not oppose tax increases (the difference of coefficients between both treatments and the control for both groups is statistically significant; see Tables 5 and 6, line 5 in columns 6 and 7).¹⁹ In this sense, information provision not only

¹⁹ Also see Tables B1 and B2 for Clinton and Trump voters, respectively.

de-polarizes, it also re-sets the normal presumption against tax increases that has been a long-standing feature of US politics. This cross-partisan re-set in favor of taxation-based fiscal adjustment is strong enough that it even appears in the aggregate results (see Figure 6); in fact, it is among the very few clear fiscal policy preferences that emerge in the aggregate post-information treatment. This is our fourth result:

RESULT 4 (Consensus on post-pandemic fiscal policy): COVID-19-related information provision shifts preference from opposition to the predominant use of tax increases towards support for such use in fiscal consolidation.

This result deserves further comment. The fact that information de-polarizes partisan policy differences is perhaps not so surprising since we began by finding partisan ‘reality polarization’ and information provision could plausibly address this possible source of partisan policy polarization even if, in this respect, it represents a re-set of politics from the ‘information immunity’ that has recently been observed in US politics. However, the re-set of politics with respect to the preference for relying on tax increases is somewhat surprising. But why should information provision also re-set politics with respect to the use of tax increases and will such a re-set prove enduring?

We turn, albeit more speculatively, to these questions in the next section where we attempt to delve a bit deeper into the mechanism that might explain this re-set that we found. There (Section 4), we also draw some implications for the fiscal policy debate that is bound to ensue in the aftermath of the COVID-19 pandemic and the fiscal legacy it leaves behind.

4 DISCUSSION

Taken at face value, Result 4 suggests that COVID-19 information has the potential to tilt US politics in the direction of an European equilibrium (Benabou and Tirole 2006; Alesina

Figure 6: Aggregate information treatment effects on the conjoint experiment (AMCEs)

Notes: The plot shows estimates of the effect of randomly assigned values of fiscal adjustment attributes on the probability of a policy package to be preferred. Each panel represents an attribute. The reference values on each attribute are the following, respectively: (1) people contribute in proportion to their income, (2) an equal balance of spending cuts and tax increases, and (3) health care. Table E1 displays the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

and Angeletos 2005) or a kind of fiscal ‘New Deal’. Our subjects are sensitized separately to the health and economic effects of COVID-19 in our information treatments. This is what causes the re-set of their preferences in our experiment as compared with the control. There is some evidence consistent with this effect from the prominence of COVID-19 in the 2020 Presidential campaign and the election of Joe Biden. But even if Result 4 was in play in the short run due to the prominence of COVID-19 in that campaign, a natural question arises: will such a preference re-set prove enduring. And how likely is that it maps into fiscal policy outcomes?

One part of the answer to these questions depends, given the logic of Result 4, on whether people are likely, in practice, to continue to be sensitized to the COVID-19 health and eco-

nomic information over the medium term. Once the new vaccines and other pharmaceutical treatments lower the death rate, the health effects of COVID-19 will depend on memory. For this reason, people are likely to become less sensitized to the health effects of COVID-19 over time. However, we have argued the economic effects are likely to prove more enduring because there is a large fiscal legacy: US government debt has grown by at least 20% of GDP through COVID, setting a fiscal adjustment agenda for the medium term. Since the economic information is enough by itself in our experiment to cause a re-set in policy preferences, this gives a reason to suppose the re-set may also endure.

The other part of our answer to whether the re-set will endure comes from considering what might underpin these information effects. In particular, is this some ‘shock’ effect from COVID-19 information or has the information triggered an engagement of people’s fundamental values? In the one case, the ‘shock’ effect will likely disappear as people adapt to the fact of COVID-19: people are known to adapt to such shocks and so its ‘shock-value’ will decline. On the other hand, people’s fundamental values are enduring and if they have been mobilized by COVID-19 information, then the effects are likely to be more enduring.

4.1 Mechanisms

Our first test of whether fundamental values have been triggered by the COVID-19 comes from considering whether those who put a high value on life were the ones that particularly adjusted to the preferences for raising taxes in our information treatments. We elicit the value of a statistical life (VSL) saved through COVID-19 behavioral interventions in our survey in a manner that is designed to capture not only selfish preferences but also pro-social preferences. In particular, our subjects make a sequence of [Holt and Laury \(2002\)](#) like binary choices between outcomes that contain a pair of deaths per 1m and average household income losses in the US. The switch point in this sequence allows us to infer the boundary values for that person’s COVID VSL. However, unlike the usual way of eliciting VSL that focuses on how the subject’s *personal* chances of death change through their choice of option,

our [Holt and Laury \(2002\)](#) questions focus on how each option affects the chances of death in society as a whole. A selfish person will infer from this that their personal chances have changed with society’s and answer accordingly, but a pro-social person will in addition take account of how not only their, but also other people’s chances of death have also changed with each option. We find that the elicited VSLs are much higher than those generated by the usual *personal* chances question; and we interpret this difference as a signaling the influence of pro-social preferences in their elicited VSLs.²⁰ In particular, we infer that those who have a high VSL have significant pro-social preferences. Thus, when we ask do those who put a high VSL adjust more to the information prompt, we are asking: is there evidence that the COVID-19 information prompt engaged with people’s pro-sociality and so could explain why this prompt shifted their preferences towards tax increases?

Figure 7 presents conjoint control and information treatment effects for the sub-sample of those subjects who revealed the highest valuations of life (above \$45.5 million), (see also Table E10 in the Appendix). We compared the estimates for this subgroup (N=986) with those for the rest of the pool of subjects (N=1259). It is clear that for those high VSL individuals, the effect of both treatments is positive. In other words, they are more likely to support fiscal packages that rely mostly on increasing taxes; a sizable 5 percentage point difference compared to the control.

Our second test of whether fundamental values have been engaged by the information prompt relates to redistributive preferences. It is well known that COVID-19 has exacerbated inequality (see e.g. [Adams-Prassl et al. 2020](#)) and the resulting labor market shock might have challenged (some of) Americans’ long-held beliefs about inequality and fairness ([Jeffrey and Matakos 2020](#)). Thus, an additional possible mechanism through which the COVID-19

²⁰ There may be other reasons for the difference between the usual and our estimates of COVID-19 value of statistical life (VSL). For example, it is well known that people’s VSL is sensitive to whether the event in question is hypothetical or real and present. The average COVID-related VSL estimate was \$36 million –about three times higher than the value that the Environmental Protection Agency attaches to life– and the highest possible VSL was above \$45.5 million (see [Hargreaves Heap et al. 2020](#)). We estimated the VSL using the usual personal-odds focused question and found that VSLs were also high, probably for the reason of immediacy above. The uplift from the pro-social inclusive form of the question was about 50%.

Figure 7: Information effects on value of life and fiscal consensus.

Notes: The plot shows estimates of the effect of randomly assigned values of fiscal adjustment attributes on the probability of a policy package to be preferred. Table E10 displays the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

information might have shifted tax preferences is through the way COVID-19 information engages people’s redistributive preferences. This might be particularly marked among those with strong redistributive preferences and/or among those who have been particularly adversely affected economically by the pandemic. In Figures D1 and D2 in the Appendix (Section D) we reproduce an analysis, similar to that of Figure 7, for those who have strong redistributive preferences and who have been most economically affected by the pandemic. Subjects who are more concerned about inequality and whose earnings have been most affected the most by the pandemic are more likely to support a fiscal adjustment policy that mainly relies on tax increases (primarily levied on wealthy Americans) after receiving the information treatments.

Thus, we find evidence that is consistent with the COVID-19 information prompts engaging people’s fundamental values. Taken together with the likely long-lived nature of the economic effects of COVID-19, this leads us cautiously to expect that the fiscal preference re-set may also endure. With this in mind, we next turn our attention to the substantive interpretation of our results in terms of actual policy outcomes in an election and hence the political feasibility of this re-set.

4.2 Policy implications

While the AMCEs are useful for the identification of causal effects on preferences over multi-dimensional policies – thus helping us detect significant de-polarization (and the possibility of a fiscal policy re-set), they come with an important caveat. Their substantive interpretation is not straightforward for political contests. To put it differently, they do not tell us how particular policy packages might fare in an actual ballot. In particular, there may be shift in preferences towards tax increases, but does this mean a policy of tax increases would actually win an election? Or, in other words, is the post-pandemic fiscal ‘New Deal’ politically feasible?

The next set of exercises helps address this question by providing a key substantive interpretation of our findings. First, in Figure 8, we condition a tax-based fiscal adjustment policy on the ‘who shall pay?’ question (see also Table E11). If Americans seem to support increases in taxes (after receiving information), who do they wish to see carrying the burden? The answer is clear: a tax levied on companies and the wealthy is preferred by 5 to 7 percentage points compared to the baseline (everyone carries the burden proportionally). Moreover, this effect is relatively stronger after people receive the economic loss information.

Would this translate into policy outcomes, given the complexity of fiscal policy –with its multiple dimensions? We take up this task next by identifying whether there is any tax-based fiscal policy package that can garner sufficient support. This effectively amounts to the computation of the marginal mean support of such a tax-based plan: that is, we estimate its

Figure 8: Aggregate preference for tax burden allocation (“Who should pay?”)

Notes: The plot shows estimates of the effect of randomly assigned values of fiscal adjustment attributes on the probability of a policy package to be preferred. Table E11 displays the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

popularity against the baseline (equal mix of taxes and spending cuts), varying one element at a time and evaluating all other attributes on their mean support. The purpose of this exercise is to identify whether there is a clear policy winner (preferred by a majority) among possible fiscal adjustment packages.

Figure 9 presents estimates of popular support over a series of plans (see also Tables H1, H2 and H3 in the Appendix). In the first panel of Figure 9, we display those estimates that are based on tax increases (the only clear preference we identified post-information treatment) against the baseline (an equal mix of taxes and spending cuts) while varying the identity of those who should carry the tax burden. In the second and third panel, we show estimates of policy packages based on spending cuts and an equal mix of taxes and spending cuts, respectively.

Figure 9: Support for fiscal adjustment policy packages (marginal means)

Notes: Three panels demonstrate policy combinations with increasing taxes (packages V to P1), spending cuts (packages L2 to F3) and a balance between the two (packages B4 to V4). Each bar demonstrates linear estimates for a particular policy package as presented in Appendix Tables H1, H2 and H3 –these also include the estimates for the baseline (no-info) group which are not depicted here. Each dashed line depicts mean estimates from groups of policy combinations when policy areas for cuts are averaged across.

These sets of columns in each panel (purple, green and black dashed lines) refer to the two information treatments (combined). We also report packages with 'no information treatment' condition in Tables [H1](#), [H2](#) and [H3](#). Considering all possible fiscal consolidation packages, the most favorable ones appear to be those that combine increasing taxes (post-treatment) and putting the burden either on wealthy Americans or on companies, with an *average 20 percentage points more support* compared to the baseline. These are the policy combinations with the highest average support (dashed black and purple lines, in the top panel of Figure [9](#)). In other words, compared with the status quo of an equal mix of fiscal policy tools with the burden spread across everyone, a policy of tax increases with the burden falling on the wealthy or companies is preferred by a margin of (at least) 60 to 40%.

To see how much of this 20 percentage point margin is due to tax increases and how much is due to the burden falling on companies or the wealthy, we now compare this policy with a status quo where again there is a balance of tax and increases and spending cuts, but this time the burden falls on either the wealthy or companies. This amounts to comparing (primarily) tax-based fiscal adjustment plans with *both* other options, whilst always conditioning on either richer Americans or companies carrying more of the fiscal burden. Put differently, is this re-set really centered around increasing taxes?

Again in Figure [9](#), we compare the averages of those policy packages that put the burden on companies and richer Americans (purple and black dashed lines, respectively) across the three panels. It is obvious that tax-based plans (*ceteris paribus*) muster *at least a 10 percentage point more support* compared to the other two options (averaged over all other remaining attribute values). Hence, tax-based plans would carry a 55-45% majority of the American public, *ceteris paribus*.^{[21](#)}

In sum, our findings suggest not only that information on COVID-19 produces a policy preference re-set, this re-set creates a politically feasible outcome with tax increases, in the

²¹In fact, support for tax-based fiscal adjustment plans is so strong that manages to stay (marginally) on the positive when compared to the other two options (see the green dashed lines), even if the elderly are to carry the burden –the least liked option by all.

sense that such a policy package with tax increases could command an electoral majority, and this fiscal ‘new Deal’ could be long lasting.

5 CONCLUSIONS

In this study, we find that citizens’ responses to COVID-19 in many respects fit with earlier patterns in US politics. There is reality polarization and there is partisan fiscal policy preference polarization along familiar lines in our control group. Furthermore, there is one conspicuous area of partisan agreement; the one that most clearly distinguishes the US from social-democratic politics in Europe: the size of the state. Neither Democrats nor Republicans in our control group have an appetite for fiscal adjustment policies that predominantly rely on tax increases.

However, compared with pre-COVID-19 politics, there is one key difference that is potentially highly significant: the provision of COVID-19 information has a dramatic effect on policy preferences. First, the partisan fiscal policy polarization disappears. Second, the distinguishing consensus against tax increases is reversed: both Democrats and Republicans now swing behind reliance on tax increases. This is an important result in its own right: the pre-COVID-19 characteristic of ‘information immunity’ disappears in our experiment. Importantly, this could signal a turn towards a more consensual and social-democratic type of politics in the US as information on the COVID-19 and its health and economic implications accumulates.

In our experiment, COVID-19 information had this protean effect on US politics at the height of the first wave of the pandemic in April 2020. As a result, a wholly new question arises. Will the effect persist as COVID-19 information accumulates? In other words, will this consensual and social-democratic turn prove enduring?

Although our experiment cannot directly answer this question, it does contain some indirect pointers. In particular, we find evidence that these information induced changes are

underpinned by key values and beliefs as well as socio-economic fundamentals. Key values and beliefs are typically more robust and so once engaged are likely to prove enduring in their influence. Likewise, socio-economic fundamentals only move slowly in the medium term. But these are just pointers. Another pointer, of course, is the recent election of Joe Biden. What our study suggests in this respect is that Biden's COVID-19 campaign messages played into what, with these campaign COVID-19 triggers, was likely to be a new and more sympathetic political audience at the height of the first wave of the pandemic. This plausibly contributed to his success in the election and so is one indication that these COVID-19 information effects may have continued, at the very least as far as November 2020.

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A Materials and methods

To generate the sample, we used the US Current Population Survey. We created a total of 170 subgroups weighted based on age, gender, region and work status. The stratification tables can be found in Figure A1, assuming a total (targeted) sample size of 2,500 respondents in each country. Figure A2 reports the subgroups that we could not fill our quotas completely on Prolific and thus weighted accordingly in our analysis to ensure representativeness. Figure A3 shows an example pair from the conjoint experiment. Figure A4 demonstrates the distribution of the dependent variable.

To ensure the robustness of our aggregate results, we implemented (manually targeted) entropy-adjusted weights to create a fully balanced sample in terms of partisan affiliation. In specific, we used entropy balancing to re-weight our survey sample to known partisan affiliation characteristics of the US population (pre-2020). This method adjusts differences in the first, second, and third moment of the covariate distributions (i.e. covariate means, variances, and skewness) (for a detailed presentation of the methods, see [Hainmueller and Xu 2011](#)). We do not observe any major differences in the aggregate preferences when we adjust our sample to these characteristics. Figure A5 and Table E3 show regression results of aggregate preferences with partisanship-adjusted weights.

Figure A1: Stratification

		Work Status							
		Employed (also includes unemployed 65+)				Unemployed			
		Regions				Regions			
	Age	Northeast	Midwest	South	West	Northeast	Midwest	South	West
Female	18-24	14.11	20.39	31.58	20.17	10.98	10.49	23.89	14.10
	25-34	28.92	34.16	61.33	38.53	10.40	9.66	25.07	16.27
	35-44	25.44	32.29	55.80	36.64	9.14	9.48	22.33	15.41
	45-54	28.51	33.40	57.51	34.90	9.26	10.88	24.31	13.91
	55-64	24.98	28.73	45.27	27.63	15.91	16.95	37.60	21.58
	65+	52.43	60.08	106.15	62.46				
Male	16-24	13.10	19.64	33.75	21.07	12.00	11.59	23.25	14.48
	25-34	33.26	38.67	67.53	48.23	6.55	6.87	14.51	10.02
	35-44	28.69	35.94	64.57	45.16	4.56	5.09	10.29	7.07
	45-54	29.68	35.36	65.08	39.56	6.59	6.96	13.42	7.38
	55-64	24.84	30.99	49.21	31.59	11.89	12.37	23.93	14.87
	65+	42.59	49.76	86.33	52.67				

Figure A2: Subgroups not filled completely

United States		
Subgroup	Sample no.	Reached no.
Female/Northeast/65+	52	33
Male/Northeast/55- 64/e	25	12
Male/Northeast/65+	43	32
Male/Northeast/55- 64/u	12	8
Female/Midwest/65+	60	28
Male/ Midwest/65+	50	27
Female/South/65+	106	57
Male/South/55-64/e	49	40
Male/ South/65+	86	37
Male/South/55-64/u	24	22
Female/West/65+	62	34
Male/West/55-64/e	32	20
Male/ West/65+	53	34
Male/West/55-64/u	15	12

Note: Subgroups for respondents above the age of 65 do not include a work status variable. For those below the age of 65, e indicates “employed” and u indicates “unemployed”.

Figure A3: An example pair from the conjoint

Policies that mitigate the economic dislocation from the measures to reduce Covid-19 transmission – for instance, salary replacement and cheap loans – will initially increase the national debt. This will create future financial burdens: either the new debt will have to be repaid or interest payments will have to be made on this new debt.

We would like you to assess below who should shoulder this future financial burden and what policy tool should be used to raise the funds to do this in the future. We combine the various options into financial packages below.

We now ask you to compare a pair of possible financial packages (A and B). We ask you to consider four such pairs.

First pair:

	Financial Package A	Financial Package B
Policy tool to fund the future financial burden	Mainly cutting spending, but with some tax increases	Mainly increasing taxes, but also some cuts in government spending
Policy areas for any spending cuts	Defence	Roads and public transport
Who bears the financial burden	People contribute in proportion to their income	Wealthier people bear proportionately more of the financial burden

On a scale from 1 to 7, where 1 indicates that you definitely do not want the financial package to be implemented, how would you rate the two packages?

definitely don't want 1 2 3 4 5 6 7 definitely want

Financial Package A:

Financial Package B:

If you had to choose between these two financial packages, which one would you prefer?

Financial Package A ☐

Financial Package B ☐

Figure A4: Distribution of the dependent variable

Note: Distribution of respondents' perceived rating of a policy package with 1-7 Likert scale. Red line depicts mean support level.

Figure A5: Aggregate results with partisanship-adjusted weights

Notes: The plot shows estimates of the effect of randomly assigned values of fiscal adjustment attributes on the probability of a policy package to be preferred. Each panel represents an attribute. The reference values on each attribute are the following, respectively: (1) people contribute in proportion to their income, (2) an equal balance of spending cuts and tax increases, and (3) health care. Table E3 displays the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

B Additional t-tests on informational reversals across control and treatment groups

Tables 5 and 6 showed informational reversals in fiscal policy preferences across control and treatment groups within Democrats and Republicans. Tables B1 and B2 show the same within Clinton and Trump voters, respectively.

Table B1: Informational reversals within Clinton voters (2016)

	<i>Estimates</i>			<i>Differences in Estimates</i>		<i>p-value</i>	
	Control	T1	T2	Control-T1	Control-T2	Control-T1	Control-T2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Companies to bear the burden	0.082	-0.009	-0.030	0.091	0.112	0.0138**	0.0018***
Older people to bear the burden	-0.145	0.040	0.017	-0.185	-0.162	<0.001***	<0.001***
Wealthy to bear the burden	0.102	0.015	0.006	0.087	0.096	0.018**	0.008***
Cut spending	-0.018	0.001	0.012	-0.019	-0.03	0.509	0.286
Increase taxes	-0.025	0.026	0.017	-0.051	-0.042	0.075*	0.116
Cut defense spending	0.221	-0.051	-0.031	0.272	0.252	<0.001***	<0.001***
Cut environment spending	0.039	0.005	-0.021	0.034	0.06	0.506	0.246
Cut foreign aid spending	0.152	-0.026	-0.028	0.178	0.18	0.001***	<0.001***
Cut R&D spending	0.104	-0.011	-0.005	0.115	0.109	0.020*	0.024**
Cut pensions spending	0.050	-0.043	-0.029	0.093	0.079	0.086*	0.135
Cut social security spending	-0.038	0.005	-0.017	-0.043	-0.021	0.409	0.687
Cut public transport spending	0.106	-0.030	-0.027	0.136	0.133	0.008***	0.007***

Notes: The first three columns show coefficients. Columns (4) and (5) show differences in estimates. Asterisks indicate significant differences in coefficients between two groups from a Wald test of significance. N=479. *** p<0.01, ** p<0.05, * p<0.1.

Table B2: Informational reversals within D. Trump 2016 voters

	<i>Estimates</i>			<i>Differences in Estimates</i>		<i>p-value</i>	
	Control	T1	T2	Control-T1	Control-T2	Control-T1	Control-T2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Companies to bear the burden	0.024	-0.012	-0.011	0.036	0.035	0.480	0.481
Older people to bear the burden	-0.154	0.036	0.017	-0.19	-0.171	0.003***	0.007***
Wealthy to bear the burden	0.019	0.032	0.022	-0.013	-0.003	0.819	0.966
Cut spending	0.035	-0.028	-0.009	0.063	0.044	0.183	0.343
Increase taxes	-0.034	0.000	0.012	-0.034	-0.046	0.403	0.259
Cut defense spending	0.053	-0.065	-0.013	0.118	0.066	0.175	0.396
Cut environment spending	0.102	-0.102	-0.057	0.204	0.159	0.019**	0.055*
Cut foreign aid spending	0.163	-0.100	-0.006	0.263	0.169	0.002***	0.038**
Cut R&D spending	0.124	-0.074	-0.091	0.198	0.215	0.008***	0.002***
Cut pensions spending	0.012	-0.026	-0.054	0.038	0.066	0.614	0.351
Cut social security spending	-0.050	-0.036	-0.041	-0.014	-0.009	0.867	0.913
Cut public transport spending	0.073	-0.034	-0.002	0.107	0.071	0.165	0.307

Notes: The first three columns show coefficients. Columns (4) and (5) show differences in estimates. Asterisks indicate significant differences in coefficients between two groups from a Wald test of significance. N=479. *** p<0.01, ** p<0.05, * p<0.1.

C Age and income and fiscal preferences

Figures C1 and C2 show conjoint treatment effects by concern for age and income groups.

Figures C3 and C4 demonstrates information treatment effects for the same groups.

Figure C1: Conjoint treatment (AMCE) effects, by income groups (control group)

Figure C2: Conjoint treatment (AMCE) effects, by age groups (control group).

Notes: The plot shows estimates of the effect of randomly assigned values of fiscal adjustment attributes on the probability of a policy package to be preferred. Each panel represents an attribute. The reference values on each attribute are the following, respectively: (1) people contribute in proportion to their income, (2) an equal balance of spending cuts and tax increases, and (3) health care. Tables E7 and Table E6 display the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

Figure C3: Information treatment effects, by income groups

Notes: The plot shows estimates of the effect of randomly assigned values of fiscal adjustment attributes on the probability of a policy package to be preferred. Each panel represents an attribute. The reference values on each attribute are the following, respectively: (1) people contribute in proportion to their income, (2) an equal balance of spending cuts and tax increases, and (3) health care. Table E6 displays the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

Figure C4: Information treatment effects, by age groups

Notes: The plot shows estimates of the effect of randomly assigned values of fiscal adjustment attributes on the probability of a policy package to be preferred. Each panel represents an attribute. The reference values on each attribute are the following, respectively: (1) people contribute in proportion to their income, (2) an equal balance of spending cuts and tax increases, and (3) health care. Table E7 displays the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

D Results by COVID-19-related income shocks and concern for inequality

Figures D1 and D2 show treatment and information treatment effects by concern for inequality and COVID-affected income groups, respectively.

Figure D1: Information treatment effects, by concern for inequality

Notes: The plot shows estimates of the effect of randomly assigned values of fiscal adjustment attributes on the probability of a policy package to be preferred. Each panel represents an attribute. The reference values on each attribute are the following, respectively: (1) people contribute in proportion to their income, (2) an equal balance of spending cuts and tax increases, and (3) health care. Table E8 displays the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

Figure D2: Information treatment effects, by COVID-19 affected incomes

Notes: The plot shows estimates of the effect of randomly assigned values of fiscal adjustment attributes on the probability of a policy package to be preferred. Each panel represents an attribute. The reference values on each attribute are the following, respectively: (1) people contribute in proportion to their income, (2) an equal balance of spending cuts and tax increases, and (3) health care. Table E9 displays the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

E Regression tables

- E.1 Aggregate results (Likert scale and binary outcome)
- E.2 Aggregate results with state fixed effects
- E.3 Aggregate results with partisanship-adjusted entropy weights
- E.4 Heterogeneous effects with party
- E.5 Heterogeneous effects with presidential vote
- E.6 Heterogeneous effects with income
- E.7 Heterogeneous effects with age
- E.8 Heterogeneous effects with inequality groups
- E.9 Heterogeneous effects with COVID-19 income groups
- E.10 Heterogeneous effects with value of life
- E.11 Heterogeneous effects with aggregate preferences for the allocation of tax burden (Conditioning on “Who should pay?”)

Table E1: Aggregate results

	Likert scale	Binary
(Intercept)	0.38*	0.43*
	[0.35; 0.41]	[0.38; 0.48]
Companies	0.06*	0.09*
	[0.04; 0.08]	[0.05; 0.13]
Companies x T1	−0.01	−0.01
	[−0.04; 0.02]	[−0.06; 0.05]
Companies x T2	−0.01	0.01
	[−0.04; 0.02]	[−0.04; 0.07]
Older people	−0.14*	−0.21*
	[−0.16; −0.11]	[−0.25; −0.18]
Older people x T1	0.02	0.00
	[−0.01; 0.06]	[−0.05; 0.05]
Older people x T2	0.01	0.02
	[−0.02; 0.04]	[−0.03; 0.07]
Wealthy	0.07*	0.12*
	[0.05; 0.10]	[0.09; 0.16]
Wealthy x T1	0.01	0.04
	[−0.02; 0.04]	[−0.02; 0.09]
Wealthy x T2	0.01	0.01
	[−0.02; 0.04]	[−0.04; 0.06]
Cutting spending	−0.01	−0.02
	[−0.03; 0.01]	[−0.05; 0.01]
Cutting spending x T1	0.01	0.02
	[−0.02; 0.04]	[−0.03; 0.06]
Cutting spending x T2	0.01	0.00
	[−0.02; 0.03]	[−0.04; 0.04]
Increasing taxes	−0.04*	−0.06*
	[−0.06; −0.02]	[−0.10; −0.03]
Increasing taxes x T1	0.03*	0.03
	[0.00; 0.05]	[−0.02; 0.07]
Increasing taxes x T2	0.03*	0.01
	[0.00; 0.05]	[−0.03; 0.05]
Defense	0.17*	0.21*
	[0.14; 0.20]	[0.15; 0.26]
Defense x T1	−0.07*	−0.02
	[−0.12; −0.03]	[−0.09; 0.06]
Defense x T2	−0.05*	−0.02
	[−0.09; −0.00]	[−0.09; 0.06]

Environment	0.07*	0.09*
	[0.04; 0.10]	[0.04; 0.14]
Environment x T1	-0.03	0.03
	[-0.08; 0.01]	[-0.04; 0.10]
Environment x T2	-0.05*	-0.05
	[-0.10; -0.01]	[-0.12; 0.02]
Foreign aid	0.17*	0.22*
	[0.14; 0.21]	[0.17; 0.28]
Foreign aid x T1	-0.07*	0.01
	[-0.12; -0.03]	[-0.07; 0.08]
Foreign aid x T2	-0.05*	-0.01
	[-0.09; -0.00]	[-0.08; 0.06]
Research development	0.10*	0.10*
	[0.07; 0.13]	[0.04; 0.15]
Research development x T1	-0.03	0.03
	[-0.07; 0.01]	[-0.04; 0.11]
Research development x T2	-0.04	0.03
	[-0.08; 0.00]	[-0.05; 0.10]
Pensions	0.05*	0.04
	[0.02; 0.08]	[-0.02; 0.09]
Pensions x T1	-0.03	0.03
	[-0.08; 0.01]	[-0.05; 0.10]
Pensions x T2	-0.03	-0.00
	[-0.07; 0.01]	[-0.07; 0.07]
Social security	-0.02	-0.05
	[-0.05; 0.01]	[-0.10; 0.00]
Social security x T1	-0.02	-0.01
	[-0.06; 0.03]	[-0.08; 0.07]
Social security x T2	-0.04	0.00
	[-0.08; 0.01]	[-0.07; 0.08]
Public transport	0.10*	0.13*
	[0.07; 0.13]	[0.08; 0.18]
Public transport x T1	-0.04	0.03
	[-0.08; 0.00]	[-0.04; 0.10]
Public transport x T2	-0.03	-0.00
	[-0.07; 0.01]	[-0.07; 0.07]
T1	0.01	-0.03
	[-0.03; 0.05]	[-0.10; 0.03]
T2	0.03	-0.00
	[-0.01; 0.07]	[-0.07; 0.06]
R ²	0.11	0.10
Adj. R ²	0.11	0.10
Num. obs.	18116	18100
RMSE	0.28	0.48
N Clusters	2245	2243

* Null hypothesis value outside the confidence interval.

Table E2: Aggregate results with state fixed effects

	Likert scale	Binary
Companies	0.06*	0.09*
	[0.04; 0.08]	[0.06; 0.13]
Companies x T1	-0.01	-0.01
	[-0.04; 0.02]	[-0.06; 0.05]
Companies x T2	-0.01	0.01
	[-0.04; 0.02]	[-0.04; 0.06]
Older people	-0.14*	-0.21*
	[-0.16; -0.11]	[-0.25; -0.18]
Older people x T1	0.02	0.00
	[-0.01; 0.06]	[-0.05; 0.05]
Older people x T2	0.01	0.02
	[-0.02; 0.04]	[-0.03; 0.07]
Wealthy	0.08*	0.13*
	[0.05; 0.10]	[0.09; 0.16]
Wealthy x T1	0.01	0.04
	[-0.02; 0.04]	[-0.02; 0.09]
Wealthy x T2	0.01	0.01
	[-0.02; 0.04]	[-0.04; 0.06]
Cutting spending	-0.01	-0.02
	[-0.03; 0.01]	[-0.05; 0.01]
Cutting spending x T1	0.01	0.02
	[-0.02; 0.04]	[-0.02; 0.06]
Cutting spending x T2	0.01	0.00
	[-0.02; 0.03]	[-0.04; 0.04]
Increasing taxes	-0.04*	-0.06*
	[-0.06; -0.02]	[-0.10; -0.03]
Increasing taxes x T1	0.03*	0.03
	[0.00; 0.05]	[-0.02; 0.07]
Increasing taxes x T2	0.03*	0.01
	[0.00; 0.05]	[-0.03; 0.05]
Defense	0.17*	0.21*
	[0.14; 0.20]	[0.15; 0.26]
Defense x T1	-0.07*	-0.02
	[-0.12; -0.03]	[-0.10; 0.06]
Defense x T2	-0.05*	-0.02
	[-0.09; -0.00]	[-0.09; 0.06]

Environment	0.07*	0.09*
	[0.04; 0.10]	[0.04; 0.14]
Environment x T1	-0.03	0.03
	[-0.07; 0.01]	[-0.04; 0.10]
Environment x T2	-0.05*	-0.05
	[-0.09; -0.01]	[-0.12; 0.02]
Foreign aid	0.17*	0.23*
	[0.14; 0.20]	[0.17; 0.28]
Foreign aid x T1	-0.07*	0.01
	[-0.12; -0.03]	[-0.07; 0.08]
Foreign aid x T2	-0.05*	-0.01
	[-0.09; -0.00]	[-0.08; 0.06]
Research development	0.10*	0.10*
	[0.08; 0.13]	[0.05; 0.15]
Research development x T1	-0.03	0.03
	[-0.07; 0.01]	[-0.04; 0.11]
Research development x T2	-0.04*	0.02
	[-0.08; -0.00]	[-0.05; 0.10]
Pensions	0.05*	0.04
	[0.02; 0.08]	[-0.02; 0.09]
Pensions x T1	-0.03	0.03
	[-0.08; 0.01]	[-0.05; 0.10]
Pensions x T2	-0.03	-0.00
	[-0.07; 0.01]	[-0.07; 0.07]
Social security	-0.02	-0.05
	[-0.05; 0.01]	[-0.10; 0.00]
Social security x T1	-0.02	-0.01
	[-0.06; 0.03]	[-0.08; 0.07]
Social security x T2	-0.04	0.00
	[-0.08; 0.00]	[-0.07; 0.08]
Public transport	0.11*	0.13*
	[0.08; 0.13]	[0.08; 0.18]
Public transport x T1	-0.04	0.03
	[-0.08; 0.00]	[-0.04; 0.10]
Public transport x T2	-0.03	-0.00
	[-0.07; 0.01]	[-0.07; 0.07]
T1	0.01	-0.03
	[-0.03; 0.05]	[-0.10; 0.04]
T2	0.03	-0.00
	[-0.01; 0.07]	[-0.07; 0.06]
R ²	0.12	0.10
Adj. R ²	0.12	0.10
Num. obs.	18116	18100
RMSE	0.28	0.48
N Clusters	2245	2243

* Null hypothesis value outside the confidence interval.

Table E3: Aggregate results with partisanship-adjusted entropy weights

	Quota weight	Partisanship adjusted weight
(Intercept)	0.38*	0.40*
	[0.35; 0.41]	[0.36; 0.43]
Companies	0.06*	0.04*
	[0.04; 0.08]	[0.02; 0.07]
Companies x T1	-0.01	-0.01
	[-0.04; 0.02]	[-0.04; 0.03]
Companies x T2	-0.01	-0.00
	[-0.04; 0.02]	[-0.04; 0.03]
Older people	-0.14*	-0.14*
	[-0.16; -0.11]	[-0.17; -0.12]
Older people x T1	0.02	0.03
	[-0.01; 0.06]	[-0.01; 0.06]
Older people x T2	0.01	0.03
	[-0.02; 0.04]	[-0.01; 0.07]
Wealthy	0.07*	0.06*
	[0.05; 0.10]	[0.04; 0.08]
Wealthy x T1	0.01	0.01
	[-0.02; 0.04]	[-0.02; 0.05]
Wealthy x T2	0.01	0.02
	[-0.02; 0.04]	[-0.01; 0.06]
Cutting spending	-0.01	-0.00
	[-0.03; 0.01]	[-0.02; 0.02]
Cutting spending x T1	0.01	-0.00
	[-0.02; 0.04]	[-0.03; 0.03]
Cutting spending x T2	0.01	0.00
	[-0.02; 0.03]	[-0.02; 0.03]
Increasing taxes	-0.04*	-0.04*
	[-0.06; -0.02]	[-0.06; -0.02]
Increasing taxes x T1	0.03*	0.02
	[0.00; 0.05]	[-0.01; 0.05]
Increasing taxes x T2	0.03*	0.03*
	[0.00; 0.05]	[0.00; 0.06]
Defense	0.17*	0.14*
	[0.14; 0.20]	[0.10; 0.18]
Defense x T1	-0.07*	-0.08*
	[-0.12; -0.03]	[-0.14; -0.02]
Defense x T2	-0.05*	-0.04
	[-0.09; -0.00]	[-0.09; 0.01]

Environment	0.07*	0.07*
	[0.04; 0.10]	[0.04; 0.11]
Environment x T1	-0.03	-0.05
	[-0.08; 0.01]	[-0.10; 0.00]
Environment x T2	-0.05*	-0.05
	[-0.10; -0.01]	[-0.10; 0.00]
Foreign aid	0.17*	0.17*
	[0.14; 0.21]	[0.13; 0.21]
Foreign aid x T1	-0.07*	-0.09*
	[-0.12; -0.03]	[-0.15; -0.03]
Foreign aid x T2	-0.05*	-0.04
	[-0.09; -0.00]	[-0.09; 0.01]
Research development	0.10*	0.11*
	[0.07; 0.13]	[0.08; 0.14]
Research development x T1	-0.03	-0.05*
	[-0.07; 0.01]	[-0.10; -0.00]
Research development x T2	-0.04	-0.05*
	[-0.08; 0.00]	[-0.09; -0.00]
Pensions	0.05*	0.03
	[0.02; 0.08]	[-0.00; 0.07]
Pensions x T1	-0.03	-0.03
	[-0.08; 0.01]	[-0.08; 0.02]
Pensions x T2	-0.03	-0.04
	[-0.07; 0.01]	[-0.09; 0.01]
Social security	-0.02	-0.03
	[-0.05; 0.01]	[-0.07; 0.01]
Social security x T1	-0.02	-0.04
	[-0.06; 0.03]	[-0.09; 0.02]
Social security x T2	-0.04	-0.03
	[-0.08; 0.01]	[-0.08; 0.02]
Public transport	0.10*	0.10*
	[0.07; 0.13]	[0.07; 0.14]
Public transport x T1	-0.04	-0.06*
	[-0.08; 0.00]	[-0.11; -0.01]
Public transport x T2	-0.03	-0.03
	[-0.07; 0.01]	[-0.08; 0.02]
T1	0.01	0.02
	[-0.03; 0.05]	[-0.03; 0.07]
T2	0.03	0.01
	[-0.01; 0.07]	[-0.04; 0.06]
R ²	0.11	0.10
Adj. R ²	0.11	0.10
Num. obs.	18116	18116
RMSE	0.28	0.29
N Clusters	2245	2245

* Null hypothesis value outside the confidence interval.

Table E4: Heterogeneous effects with party

	Republican	Democrat	Independent
(Intercept)	0.43*	0.36*	0.37*
	[0.38; 0.49]	[0.32; 0.40]	[0.30; 0.44]
Companies	0.02	0.07*	0.07*
	[−0.03; 0.06]	[0.04; 0.10]	[0.01; 0.13]
Companies x T1	−0.01	−0.00	−0.03
	[−0.08; 0.05]	[−0.04; 0.04]	[−0.11; 0.06]
Companies x T2	0.01	−0.02	−0.01
	[−0.05; 0.08]	[−0.06; 0.02]	[−0.09; 0.06]
Older people	−0.15*	−0.14*	−0.10*
	[−0.20; −0.11]	[−0.17; −0.11]	[−0.16; −0.05]
Older people x T1	0.03	0.02	0.03
	[−0.04; 0.10]	[−0.02; 0.06]	[−0.05; 0.10]
Older people x T2	0.08*	−0.00	−0.01
	[0.00; 0.15]	[−0.04; 0.04]	[−0.09; 0.07]
Wealthy	0.02	0.10*	0.08*
	[−0.02; 0.06]	[0.07; 0.13]	[0.03; 0.14]
Wealthy x T1	0.01	0.01	0.02
	[−0.05; 0.07]	[−0.03; 0.05]	[−0.06; 0.09]
Wealthy x T2	0.05	0.00	0.00
	[−0.01; 0.12]	[−0.04; 0.04]	[−0.07; 0.08]
Cutting spending	0.02	−0.03*	−0.01
	[−0.02; 0.06]	[−0.05; −0.00]	[−0.06; 0.04]
Cutting spending x T1	−0.02	0.02	0.04
	[−0.08; 0.03]	[−0.01; 0.05]	[−0.02; 0.11]
Cutting spending x T2	−0.01	0.02	−0.00
	[−0.06; 0.04]	[−0.01; 0.05]	[−0.07; 0.06]
Increasing taxes	−0.05*	−0.03*	−0.06*
	[−0.09; −0.02]	[−0.05; −0.00]	[−0.11; −0.02]
Increasing taxes x T1	0.02	0.02	0.07*
	[−0.03; 0.07]	[−0.01; 0.05]	[0.00; 0.13]
Increasing taxes x T2	0.03	0.03	0.03
	[−0.02; 0.08]	[−0.00; 0.06]	[−0.04; 0.10]
Defense	0.05	0.23*	0.12*
	[−0.02; 0.11]	[0.19; 0.28]	[0.03; 0.21]
Defense x T1	−0.10	−0.07*	−0.01
	[−0.20; 0.01]	[−0.13; −0.01]	[−0.12; 0.10]
Defense x T2	−0.03	−0.05	−0.01
	[−0.11; 0.06]	[−0.11; 0.00]	[−0.12; 0.11]

Environment	0.09*	0.07*	0.05
	[0.02; 0.16]	[0.03; 0.11]	[−0.03; 0.14]
Environment x T1	−0.08	−0.03	0.01
	[−0.18; 0.01]	[−0.08; 0.03]	[−0.10; 0.12]
Environment x T2	−0.05	−0.06	−0.02
	[−0.14; 0.05]	[−0.11; 0.00]	[−0.13; 0.10]
Foreign aid	0.16*	0.18*	0.17*
	[0.09; 0.23]	[0.14; 0.22]	[0.09; 0.25]
Foreign aid x T1	−0.12*	−0.06	−0.02
	[−0.22; −0.02]	[−0.12; 0.00]	[−0.13; 0.08]
Foreign aid x T2	−0.04	−0.03	−0.06
	[−0.14; 0.05]	[−0.09; 0.03]	[−0.16; 0.05]
Research development	0.11*	0.11*	0.07
	[0.06; 0.17]	[0.07; 0.15]	[−0.01; 0.14]
Research development x T1	−0.10*	−0.02	0.04
	[−0.18; −0.01]	[−0.08; 0.04]	[−0.06; 0.13]
Research development x T2	−0.07	−0.03	−0.01
	[−0.14; 0.01]	[−0.08; 0.02]	[−0.11; 0.09]
Pensions	−0.00	0.07*	0.04
	[−0.06; 0.05]	[0.03; 0.11]	[−0.03; 0.11]
Pensions x T1	−0.02	−0.05	0.00
	[−0.11; 0.08]	[−0.11; 0.01]	[−0.09; 0.10]
Pensions x T2	−0.06	−0.02	0.00
	[−0.14; 0.03]	[−0.08; 0.04]	[−0.10; 0.11]
Social security	−0.06	0.01	−0.05
	[−0.13; 0.01]	[−0.03; 0.05]	[−0.12; 0.02]
Social security x T1	−0.06	−0.02	0.09
	[−0.16; 0.04]	[−0.08; 0.04]	[−0.02; 0.19]
Social security x T2	−0.02	−0.04	−0.04
	[−0.12; 0.08]	[−0.10; 0.02]	[−0.14; 0.05]
Public transport	0.08*	0.13*	0.05
	[0.03; 0.14]	[0.09; 0.17]	[−0.01; 0.12]
Public transport x T1	−0.08	−0.05	0.06
	[−0.17; 0.01]	[−0.11; 0.01]	[−0.03; 0.15]
Public transport x T2	−0.02	−0.05	0.02
	[−0.10; 0.07]	[−0.10; 0.01]	[−0.08; 0.12]
T1	0.04	0.02	−0.08
	[−0.05; 0.12]	[−0.04; 0.07]	[−0.17; 0.02]
T2	−0.01	0.03	0.04
	[−0.09; 0.07]	[−0.02; 0.09]	[−0.06; 0.14]
R ²	0.09	0.15	0.10
Adj. R ²	0.08	0.15	0.09
Num. obs.	3912	10015	3197
RMSE	0.30	0.27	0.28
N Clusters	479	1248	398

* Null hypothesis value outside the confidence interval.

Table E5: Heterogeneous effects with presidential vote

	D.Trump	H.Clinton	Did not vote
(Intercept)	0.41*	0.37*	0.38*
	[0.34; 0.48]	[0.33; 0.42]	[0.32; 0.43]
Companies	0.02	0.08*	0.05*
	[−0.02; 0.07]	[0.05; 0.11]	[0.01; 0.09]
Companies x T1	−0.01	−0.01	−0.03
	[−0.08; 0.06]	[−0.06; 0.04]	[−0.09; 0.03]
Companies x T2	−0.01	−0.03	0.03
	[−0.08; 0.06]	[−0.07; 0.01]	[−0.03; 0.08]
Older people	−0.15*	−0.15*	−0.12*
	[−0.21; −0.10]	[−0.18; −0.11]	[−0.16; −0.08]
Older people x T1	0.04	0.04	−0.01
	[−0.04; 0.11]	[−0.01; 0.09]	[−0.07; 0.05]
Older people x T2	0.02	0.02	0.00
	[−0.06; 0.10]	[−0.03; 0.06]	[−0.05; 0.06]
Wealthy	0.02	0.10*	0.08*
	[−0.03; 0.07]	[0.07; 0.14]	[0.03; 0.12]
Wealthy x T1	0.03	0.02	−0.01
	[−0.04; 0.10]	[−0.03; 0.06]	[−0.07; 0.06]
Wealthy x T2	0.02	0.01	0.02
	[−0.05; 0.09]	[−0.04; 0.05]	[−0.04; 0.08]
Cutting spending	0.04	−0.02	−0.03
	[−0.01; 0.08]	[−0.04; 0.01]	[−0.07; 0.00]
Cutting spending x T1	−0.03	0.00	0.03
	[−0.09; 0.03]	[−0.04; 0.04]	[−0.02; 0.07]
Cutting spending x T2	−0.01	0.01	0.01
	[−0.07; 0.05]	[−0.02; 0.05]	[−0.04; 0.05]
Increasing taxes	−0.03	−0.03*	−0.05*
	[−0.07; 0.00]	[−0.05; −0.00]	[−0.09; −0.02]
Increasing taxes x T1	0.00	0.03	0.05
	[−0.05; 0.05]	[−0.01; 0.06]	[−0.00; 0.09]
Increasing taxes x T2	0.01	0.02	0.02
	[−0.04; 0.07]	[−0.02; 0.05]	[−0.02; 0.07]
Defense	0.05	0.22*	0.18*
	[−0.02; 0.13]	[0.17; 0.27]	[0.12; 0.25]
Defense x T1	−0.07	−0.05	−0.09*
	[−0.18; 0.05]	[−0.12; 0.02]	[−0.18; −0.00]
Defense x T2	−0.01	−0.03	−0.10*
	[−0.11; 0.08]	[−0.10; 0.03]	[−0.18; −0.01]

Environment	0.10*	0.04	0.10*
	[0.02; 0.18]	[−0.01; 0.09]	[0.04; 0.16]
Environment x T1	−0.10	0.01	−0.02
	[−0.21; 0.00]	[−0.06; 0.07]	[−0.10; 0.06]
Environment x T2	−0.06	−0.02	−0.11*
	[−0.16; 0.04]	[−0.09; 0.05]	[−0.19; −0.04]
Foreign aid	0.16*	0.15*	0.24*
	[0.09; 0.24]	[0.10; 0.20]	[0.18; 0.30]
Foreign aid x T1	−0.10	−0.03	−0.14*
	[−0.21; 0.01]	[−0.10; 0.04]	[−0.22; −0.06]
Foreign aid x T2	−0.01	−0.03	−0.17*
	[−0.10; 0.09]	[−0.09; 0.04]	[−0.25; −0.09]
Research development	0.12*	0.10*	0.11*
	[0.06; 0.19]	[0.06; 0.15]	[0.06; 0.17]
Research development x T1	−0.07	−0.01	−0.06
	[−0.17; 0.02]	[−0.08; 0.05]	[−0.14; 0.02]
Research development x T2	−0.09*	−0.01	−0.08*
	[−0.18; −0.00]	[−0.07; 0.06]	[−0.15; −0.00]
Pensions	0.01	0.05*	0.08*
	[−0.05; 0.08]	[0.00; 0.10]	[0.02; 0.15]
Pensions x T1	−0.03	−0.04	−0.07
	[−0.13; 0.08]	[−0.11; 0.02]	[−0.15; 0.02]
Pensions x T2	−0.05	−0.03	−0.04
	[−0.14; 0.03]	[−0.09; 0.03]	[−0.12; 0.04]
Social security	−0.05	−0.04	0.02
	[−0.13; 0.03]	[−0.08; 0.01]	[−0.04; 0.08]
Social security x T1	−0.04	0.01	−0.02
	[−0.14; 0.07]	[−0.06; 0.07]	[−0.10; 0.06]
Social security x T2	−0.04	−0.02	−0.05
	[−0.14; 0.06]	[−0.08; 0.05]	[−0.13; 0.03]
Public transport	0.07*	0.11*	0.14*
	[0.00; 0.14]	[0.06; 0.15]	[0.07; 0.20]
Public transport x T1	−0.03	−0.03	−0.05
	[−0.13; 0.06]	[−0.09; 0.03]	[−0.13; 0.03]
Public transport x T2	−0.00	−0.03	−0.08*
	[−0.10; 0.09]	[−0.09; 0.03]	[−0.16; −0.00]
T1	0.02	−0.00	0.01
	[−0.08; 0.12]	[−0.06; 0.06]	[−0.06; 0.09]
T2	0.02	0.02	0.06
	[−0.08; 0.11]	[−0.04; 0.08]	[−0.01; 0.14]
R ²	0.10	0.16	0.12
Adj. R ²	0.09	0.15	0.11
Num. obs.	3695	7856	4848
RMSE	0.31	0.27	0.27
N Clusters	450	976	604

* Null hypothesis value outside the confidence interval.

Table E6: Heterogeneous effects with income

	Under 20k	20-60k	60-100k	100k+
(Intercept)	0.40*	0.39*	0.38*	0.35*
	[0.32; 0.48]	[0.34; 0.43]	[0.32; 0.44]	[0.28; 0.41]
Companies	0.04	0.06*	0.04	0.08*
	[−0.02; 0.10]	[0.02; 0.09]	[−0.01; 0.09]	[0.02; 0.13]
Companies x T1	0.03	−0.02	0.00	0.01
	[−0.05; 0.12]	[−0.07; 0.03]	[−0.06; 0.06]	[−0.07; 0.09]
Companies x T2	0.01	0.01	0.00	−0.09*
	[−0.06; 0.08]	[−0.04; 0.06]	[−0.06; 0.07]	[−0.16; −0.01]
Older people	−0.15*	−0.15*	−0.12*	−0.12*
	[−0.20; −0.09]	[−0.19; −0.12]	[−0.16; −0.08]	[−0.17; −0.06]
Older people x T1	0.03	0.02	0.00	0.05
	[−0.05; 0.11]	[−0.03; 0.07]	[−0.06; 0.06]	[−0.02; 0.13]
Older people x T2	−0.01	0.06*	−0.03	−0.02
	[−0.08; 0.06]	[0.01; 0.11]	[−0.09; 0.03]	[−0.10; 0.06]
Wealthy	0.08*	0.06*	0.06*	0.12*
	[0.02; 0.13]	[0.03; 0.10]	[0.02; 0.11]	[0.06; 0.17]
Wealthy x T1	0.09*	−0.02	0.02	0.00
	[0.00; 0.17]	[−0.07; 0.03]	[−0.04; 0.07]	[−0.06; 0.07]
Wealthy x T2	−0.01	0.03	0.03	−0.09*
	[−0.08; 0.06]	[−0.02; 0.08]	[−0.03; 0.09]	[−0.16; −0.01]
Cutting spending	−0.01	−0.01	−0.03	0.00
	[−0.05; 0.04]	[−0.04; 0.02]	[−0.07; 0.01]	[−0.04; 0.04]
Cutting spending x T1	0.01	−0.01	0.04	0.02
	[−0.05; 0.08]	[−0.05; 0.03]	[−0.02; 0.09]	[−0.04; 0.07]
Cutting spending x T2	−0.01	0.01	0.00	0.02
	[−0.07; 0.05]	[−0.03; 0.05]	[−0.05; 0.06]	[−0.03; 0.08]
Increasing taxes	−0.05*	−0.03*	−0.06*	−0.04*
	[−0.10; −0.00]	[−0.06; −0.00]	[−0.10; −0.02]	[−0.08; −0.01]
Increasing taxes x T1	0.06	0.01	0.06*	0.01
	[−0.02; 0.13]	[−0.03; 0.05]	[0.01; 0.11]	[−0.05; 0.07]
Increasing taxes x T2	0.02	0.02	0.04	0.04
	[−0.04; 0.09]	[−0.02; 0.06]	[−0.01; 0.09]	[−0.02; 0.09]
Defense	0.14*	0.17*	0.19*	0.20*
	[0.06; 0.22]	[0.11; 0.22]	[0.12; 0.26]	[0.12; 0.28]
Defense x T1	−0.09	−0.12*	−0.04	−0.05
	[−0.22; 0.04]	[−0.20; −0.04]	[−0.13; 0.06]	[−0.16; 0.06]
Defense x T2	−0.00	−0.05	−0.06	−0.08
	[−0.11; 0.10]	[−0.12; 0.02]	[−0.15; 0.03]	[−0.18; 0.02]

Environment	0.05	0.07*	0.04	0.13*
	[−0.03; 0.14]	[0.02; 0.12]	[−0.03; 0.11]	[0.06; 0.21]
Environment x T1	−0.05	−0.04	0.01	−0.10*
	[−0.18; 0.07]	[−0.11; 0.03]	[−0.08; 0.10]	[−0.21; −0.00]
Environment x T2	−0.05	−0.07*	−0.01	−0.07
	[−0.16; 0.06]	[−0.14; −0.00]	[−0.10; 0.08]	[−0.18; 0.03]
Foreign aid	0.14*	0.18*	0.16*	0.21*
	[0.05; 0.23]	[0.13; 0.24]	[0.09; 0.24]	[0.14; 0.28]
Foreign aid x T1	−0.12	−0.10*	−0.03	−0.09
	[−0.25; 0.02]	[−0.17; −0.02]	[−0.12; 0.06]	[−0.19; 0.02]
Foreign aid x T2	0.02	−0.07	−0.05	−0.07
	[−0.09; 0.13]	[−0.14; 0.00]	[−0.14; 0.04]	[−0.17; 0.03]
Research development	0.07	0.10*	0.11*	0.15*
	[−0.01; 0.14]	[0.06; 0.15]	[0.05; 0.18]	[0.09; 0.21]
Research development x T1	0.02	−0.04	−0.02	−0.11*
	[−0.11; 0.14]	[−0.11; 0.03]	[−0.10; 0.06]	[−0.21; −0.01]
Research development x T2	−0.02	−0.05	−0.05	−0.06
	[−0.12; 0.09]	[−0.11; 0.02]	[−0.13; 0.03]	[−0.15; 0.03]
Pensions	0.04	0.08*	0.03	0.01
	[−0.05; 0.13]	[0.03; 0.13]	[−0.03; 0.09]	[−0.06; 0.09]
Pensions x T1	−0.07	−0.07	0.03	−0.01
	[−0.21; 0.07]	[−0.14; 0.00]	[−0.06; 0.11]	[−0.10; 0.09]
Pensions x T2	0.00	−0.06	−0.01	−0.03
	[−0.11; 0.12]	[−0.13; 0.00]	[−0.09; 0.08]	[−0.13; 0.07]
Social security	0.01	−0.03	−0.02	−0.02
	[−0.07; 0.10]	[−0.07; 0.02]	[−0.08; 0.05]	[−0.10; 0.05]
Social security x T1	−0.10	−0.04	0.02	0.01
	[−0.24; 0.04]	[−0.11; 0.03]	[−0.07; 0.11]	[−0.09; 0.12]
Social security x T2	−0.05	−0.04	−0.06	−0.00
	[−0.16; 0.05]	[−0.11; 0.02]	[−0.15; 0.03]	[−0.11; 0.10]
Public transport	0.04	0.13*	0.11*	0.11*
	[−0.06; 0.13]	[0.09; 0.18]	[0.05; 0.17]	[0.04; 0.18]
Public transport x T1	0.05	−0.10*	−0.02	−0.05
	[−0.08; 0.18]	[−0.16; −0.03]	[−0.10; 0.06]	[−0.14; 0.04]
Public transport x T2	0.04	−0.06	−0.06	−0.02
	[−0.06; 0.15]	[−0.13; 0.00]	[−0.14; 0.02]	[−0.11; 0.07]
T1	0.01	0.05	−0.04	0.02
	[−0.12; 0.14]	[−0.01; 0.12]	[−0.12; 0.05]	[−0.07; 0.11]
T2	0.04	0.02	0.05	0.06
	[−0.06; 0.14]	[−0.05; 0.09]	[−0.03; 0.13]	[−0.03; 0.16]
R ²	0.14	0.11	0.12	0.13
Adj. R ²	0.12	0.11	0.11	0.12
Num. obs.	2736	6856	4448	3125
RMSE	0.29	0.29	0.28	0.27
N Clusters	332	853	554	389

* Null hypothesis value outside the confidence interval.

Table E7: Heterogeneous effects with age

	18-24	25-34	35-49	50-64	65+
(Intercept)	0.40* [0.33; 0.47]	0.32* [0.26; 0.39]	0.38* [0.32; 0.44]	0.38* [0.32; 0.44]	0.40* [0.32; 0.47]
Companies	0.07* [0.02; 0.13]	0.09* [0.04; 0.14]	0.07* [0.03; 0.11]	0.04 [-0.00; 0.08]	0.04 [-0.02; 0.10]
Companies x T1	-0.05 [-0.12; 0.03]	0.01 [-0.06; 0.08]	-0.01 [-0.07; 0.05]	-0.01 [-0.06; 0.05]	-0.03 [-0.12; 0.06]
Companies x T2	-0.01 [-0.09; 0.06]	-0.06 [-0.12; 0.01]	0.03 [-0.02; 0.08]	-0.02 [-0.08; 0.03]	-0.01 [-0.10; 0.08]
Older people	-0.08* [-0.13; -0.03]	-0.10* [-0.15; -0.05]	-0.12* [-0.16; -0.08]	-0.16* [-0.21; -0.12]	-0.18* [-0.24; -0.12]
Older people x T1	-0.03 [-0.10; 0.05]	0.05 [-0.03; 0.12]	0.02 [-0.04; 0.08]	0.04 [-0.02; 0.10]	0.01 [-0.07; 0.10]
Older people x T2	-0.05 [-0.13; 0.03]	-0.05 [-0.12; 0.02]	0.05 [-0.00; 0.11]	0.02 [-0.04; 0.08]	0.03 [-0.05; 0.12]
Wealthy	0.08* [0.03; 0.14]	0.11* [0.06; 0.17]	0.08* [0.04; 0.13]	0.06* [0.03; 0.10]	0.05 [-0.01; 0.11]
Wealthy x T1	0.01 [-0.06; 0.09]	0.03 [-0.04; 0.11]	0.01 [-0.05; 0.07]	0.01 [-0.04; 0.07]	-0.01 [-0.10; 0.08]
Wealthy x T2	0.00 [-0.08; 0.08]	-0.03 [-0.10; 0.05]	0.00 [-0.06; 0.06]	0.01 [-0.04; 0.07]	0.05 [-0.03; 0.13]
Cutting spending	-0.01 [-0.05; 0.03]	0.01 [-0.03; 0.05]	-0.01 [-0.05; 0.02]	0.01 [-0.03; 0.04]	-0.02 [-0.07; 0.02]
Cutting spending x T1	-0.00 [-0.06; 0.06]	-0.02 [-0.08; 0.04]	0.00 [-0.04; 0.05]	0.02 [-0.03; 0.07]	0.03 [-0.04; 0.10]
Cutting spending x T2	0.00 [-0.06; 0.06]	-0.01 [-0.07; 0.04]	-0.00 [-0.05; 0.04]	-0.03 [-0.08; 0.02]	0.05 [-0.01; 0.12]
Increasing taxes	-0.04* [-0.08; -0.00]	-0.03 [-0.07; 0.01]	-0.05* [-0.08; -0.01]	-0.03 [-0.06; 0.00]	-0.05* [-0.09; -0.00]
Increasing taxes x T1	-0.00 [-0.06; 0.06]	0.02 [-0.04; 0.07]	0.04 [-0.00; 0.09]	0.01 [-0.04; 0.06]	0.05 [-0.01; 0.12]
Increasing taxes x T2	0.00 [-0.06; 0.06]	0.02 [-0.03; 0.07]	0.05* [0.00; 0.10]	-0.01 [-0.06; 0.04]	0.05 [-0.01; 0.11]
Defense	0.16* [0.08; 0.23]	0.25* [0.18; 0.32]	0.18* [0.11; 0.25]	0.15* [0.09; 0.22]	0.13* [0.05; 0.22]
Defense x T1	-0.03 [-0.14; 0.08]	-0.09 [-0.18; 0.01]	-0.06 [-0.15; 0.04]	-0.10* [-0.18; -0.01]	-0.10 [-0.24; 0.05]
Defense x T2	-0.07 [-0.18; 0.04]	-0.10* [-0.19; -0.01]	-0.08 [-0.16; 0.01]	-0.01 [-0.09; 0.07]	0.00 [-0.11; 0.11]

Environment	0.05 [−0.02; 0.12]	0.15* [0.09; 0.22]	0.03 [−0.04; 0.10]	0.09* [0.03; 0.15]	0.04 [−0.04; 0.12]
Environment x T1	−0.01 [−0.12; 0.10]	−0.12* [−0.21; −0.03]	0.01 [−0.08; 0.09]	−0.06 [−0.14; 0.02]	0.02 [−0.10; 0.14]
Environment x T2	−0.08 [−0.19; 0.03]	−0.12* [−0.20; −0.03]	−0.04 [−0.12; 0.05]	−0.05 [−0.13; 0.04]	−0.02 [−0.14; 0.09]
Foreign aid	0.12* [0.05; 0.20]	0.21* [0.14; 0.27]	0.16* [0.09; 0.23]	0.21* [0.15; 0.27]	0.14* [0.05; 0.22]
Foreign aid x T1	−0.00 [−0.10; 0.10]	−0.08 [−0.18; 0.01]	−0.05 [−0.14; 0.04]	−0.13* [−0.22; −0.05]	−0.06 [−0.19; 0.08]
Foreign aid x T2	−0.04 [−0.14; 0.07]	−0.08 [−0.17; 0.01]	−0.06 [−0.15; 0.03]	−0.06 [−0.14; 0.03]	−0.01 [−0.13; 0.11]
Research development	0.06 [−0.01; 0.12]	0.11* [0.04; 0.17]	0.10* [0.04; 0.16]	0.09* [0.04; 0.15]	0.15* [0.08; 0.21]
Research development x T1	−0.04 [−0.15; 0.06]	−0.04 [−0.13; 0.06]	−0.03 [−0.11; 0.05]	−0.03 [−0.11; 0.05]	−0.01 [−0.13; 0.11]
Research development x T2	−0.03 [−0.13; 0.08]	−0.02 [−0.11; 0.07]	−0.06 [−0.14; 0.02]	−0.01 [−0.08; 0.07]	−0.08 [−0.17; 0.02]
Pensions	0.09* [0.02; 0.16]	0.13* [0.06; 0.20]	0.05 [−0.01; 0.12]	−0.00 [−0.06; 0.06]	−0.01 [−0.09; 0.07]
Pensions x T1	−0.06 [−0.16; 0.05]	−0.03 [−0.13; 0.06]	−0.03 [−0.11; 0.06]	−0.06 [−0.14; 0.03]	0.01 [−0.11; 0.14]
Pensions x T2	0.01 [−0.09; 0.12]	−0.08 [−0.17; 0.00]	−0.06 [−0.14; 0.02]	0.02 [−0.06; 0.10]	−0.02 [−0.12; 0.09]
Social security	0.03 [−0.05; 0.10]	0.08* [0.02; 0.15]	−0.02 [−0.08; 0.04]	−0.04 [−0.09; 0.02]	−0.08* [−0.16; −0.01]
Social security x T1	0.08 [−0.04; 0.19]	−0.05 [−0.15; 0.05]	0.02 [−0.06; 0.11]	−0.07 [−0.15; 0.01]	−0.04 [−0.17; 0.08]
Social security x T2	−0.00 [−0.11; 0.10]	−0.07 [−0.16; 0.02]	−0.01 [−0.09; 0.08]	−0.03 [−0.11; 0.05]	−0.08 [−0.19; 0.03]
Public transport	0.11* [0.04; 0.18]	0.16* [0.10; 0.23]	0.07* [0.00; 0.13]	0.08* [0.02; 0.14]	0.12* [0.04; 0.19]
Public transport x T1	−0.08 [−0.19; 0.02]	−0.06 [−0.16; 0.03]	0.00 [−0.08; 0.08]	−0.06 [−0.14; 0.02]	0.01 [−0.11; 0.12]
Public transport x T2	−0.06 [−0.17; 0.04]	−0.06 [−0.15; 0.03]	−0.03 [−0.11; 0.05]	−0.01 [−0.09; 0.07]	−0.03 [−0.14; 0.08]
T1	0.02 [−0.08; 0.11]	0.03 [−0.07; 0.12]	0.00 [−0.08; 0.09]	0.03 [−0.05; 0.11]	−0.03 [−0.14; 0.09]
T2	0.06 [−0.04; 0.16]	0.09* [0.01; 0.18]	0.01 [−0.06; 0.09]	0.03 [−0.05; 0.11]	0.01 [−0.09; 0.12]
R ²	0.11	0.13	0.10	0.13	0.16
Adj. R ²	0.10	0.12	0.09	0.13	0.15
Num. obs.	2376	3712	5005	4712	2303
N Clusters	295	462	620	581	286

* Null hypothesis value outside the confidence interval.

Table E8: Heterogeneous effects with concern for inequality

	Low	Medium	High
(Intercept)	0.39*	0.41*	0.34*
	[0.33; 0.46]	[0.35; 0.46]	[0.30; 0.39]
Companies	0.02	0.09*	0.07*
	[−0.03; 0.07]	[0.05; 0.12]	[0.04; 0.10]
Companies x T1	−0.03	−0.03	0.01
	[−0.09; 0.04]	[−0.09; 0.02]	[−0.03; 0.06]
Companies x T2	−0.01	−0.03	−0.01
	[−0.07; 0.06]	[−0.08; 0.02]	[−0.05; 0.03]
Older people	−0.14*	−0.12*	−0.13*
	[−0.20; −0.09]	[−0.15; −0.08]	[−0.16; −0.10]
Older people x T1	0.02	0.02	0.01
	[−0.05; 0.09]	[−0.03; 0.08]	[−0.03; 0.06]
Older people x T2	0.00	0.02	−0.01
	[−0.07; 0.08]	[−0.03; 0.07]	[−0.05; 0.03]
Wealthy	0.03	0.07*	0.11*
	[−0.01; 0.08]	[0.04; 0.11]	[0.08; 0.14]
Wealthy x T1	0.01	0.03	−0.00
	[−0.05; 0.08]	[−0.02; 0.09]	[−0.04; 0.04]
Wealthy x T2	0.00	0.00	0.00
	[−0.06; 0.06]	[−0.05; 0.05]	[−0.04; 0.05]
Cutting spending	0.01	−0.02	−0.02
	[−0.03; 0.05]	[−0.05; 0.01]	[−0.04; 0.01]
Cutting spending x T1	0.00	0.02	0.01
	[−0.05; 0.06]	[−0.03; 0.06]	[−0.02; 0.05]
Cutting spending x T2	0.01	−0.01	0.02
	[−0.04; 0.06]	[−0.05; 0.04]	[−0.02; 0.05]
Increasing taxes	−0.05*	−0.04*	−0.04*
	[−0.09; −0.01]	[−0.07; −0.01]	[−0.07; −0.02]
Increasing taxes x T1	0.02	0.01	0.04*
	[−0.03; 0.07]	[−0.04; 0.05]	[0.00; 0.08]
Increasing taxes x T2	0.01	0.01	0.04*
	[−0.04; 0.06]	[−0.03; 0.05]	[0.01; 0.08]
Defense	0.09*	0.13*	0.24*
	[0.02; 0.16]	[0.08; 0.18]	[0.19; 0.29]
Defense x T1	−0.04	−0.02	−0.10*
	[−0.14; 0.06]	[−0.10; 0.06]	[−0.17; −0.03]
Defense x T2	−0.01	−0.04	−0.07*
	[−0.10; 0.08]	[−0.11; 0.04]	[−0.13; −0.01]

Environment	0.11*	0.05	0.08*
	[0.04; 0.18]	[−0.01; 0.11]	[0.03; 0.12]
Environment x T1	−0.04	0.01	−0.06*
	[−0.13; 0.05]	[−0.07; 0.09]	[−0.13; −0.00]
Environment x T2	−0.07	−0.04	−0.07*
	[−0.16; 0.02]	[−0.12; 0.04]	[−0.13; −0.01]
Foreign aid	0.22*	0.15*	0.18*
	[0.15; 0.29]	[0.10; 0.21]	[0.13; 0.22]
Foreign aid x T1	−0.11*	−0.04	−0.07*
	[−0.20; −0.02]	[−0.12; 0.04]	[−0.14; −0.00]
Foreign aid x T2	−0.06	−0.06	−0.04
	[−0.16; 0.03]	[−0.13; 0.01]	[−0.11; 0.02]
Research development	0.11*	0.09*	0.10*
	[0.05; 0.18]	[0.04; 0.14]	[0.06; 0.14]
Research development x T1	−0.07	−0.00	−0.02
	[−0.15; 0.02]	[−0.07; 0.07]	[−0.08; 0.05]
Research development x T2	−0.01	−0.06	−0.03
	[−0.10; 0.07]	[−0.13; 0.01]	[−0.09; 0.03]
Pensions	0.04	0.08*	0.04
	[−0.03; 0.11]	[0.02; 0.13]	[−0.01; 0.08]
Pensions x T1	−0.06	0.00	−0.04
	[−0.15; 0.04]	[−0.07; 0.08]	[−0.10; 0.03]
Pensions x T2	−0.01	−0.07*	−0.01
	[−0.11; 0.08]	[−0.14; −0.00]	[−0.07; 0.05]
Social security	−0.01	−0.01	−0.00
	[−0.08; 0.06]	[−0.06; 0.05]	[−0.04; 0.04]
Social security x T1	−0.02	0.03	−0.03
	[−0.12; 0.07]	[−0.05; 0.11]	[−0.10; 0.03]
Social security x T2	−0.05	−0.03	−0.03
	[−0.14; 0.04]	[−0.10; 0.05]	[−0.09; 0.03]
Public transport	0.11*	0.09*	0.12*
	[0.04; 0.17]	[0.03; 0.14]	[0.07; 0.16]
Public transport x T1	−0.06	−0.01	−0.05
	[−0.15; 0.02]	[−0.08; 0.06]	[−0.11; 0.02]
Public transport x T2	−0.02	−0.02	−0.05
	[−0.11; 0.06]	[−0.09; 0.05]	[−0.11; 0.01]
T1	0.03	−0.03	0.02
	[−0.06; 0.12]	[−0.10; 0.05]	[−0.04; 0.08]
T2	−0.01	0.04	0.05
	[−0.09; 0.08]	[−0.03; 0.11]	[−0.01; 0.11]
R ²	0.09	0.10	0.15
Adj. R ²	0.09	0.09	0.14
Num. obs.	4045	5120	8344
RMSE	0.29	0.26	0.28
N Clusters	500	636	1033

* Null hypothesis value outside the confidence interval.

Table E9: Heterogeneous effects with post-COVID-19 earnings

	Less than usual	About the same or more
(Intercept)	0.37*	0.38*
	[0.32; 0.42]	[0.35; 0.42]
Companies	0.05*	0.07*
	[0.01; 0.08]	[0.04; 0.10]
Companies x T1	-0.00	-0.02
	[-0.05; 0.05]	[-0.06; 0.02]
Companies x T2	0.02	-0.03
	[-0.02; 0.07]	[-0.07; 0.01]
Older people	-0.14*	-0.13*
	[-0.17; -0.10]	[-0.16; -0.10]
Older people x T1	0.02	0.03
	[-0.02; 0.07]	[-0.01; 0.07]
Older people x T2	0.05	-0.01
	[-0.00; 0.09]	[-0.05; 0.02]
Wealthy	0.06*	0.09*
	[0.02; 0.09]	[0.06; 0.12]
Wealthy x T1	0.04	0.00
	[-0.01; 0.08]	[-0.04; 0.04]
Wealthy x T2	0.06*	-0.03
	[0.02; 0.11]	[-0.07; 0.01]
Cutting spending	-0.00	-0.01
	[-0.03; 0.03]	[-0.04; 0.01]
Cutting spending x T1	-0.01	0.02
	[-0.05; 0.03]	[-0.02; 0.05]
Cutting spending x T2	-0.02	0.02
	[-0.06; 0.02]	[-0.01; 0.05]
Increasing taxes	-0.05*	-0.04*
	[-0.07; -0.02]	[-0.06; -0.01]
Increasing taxes x T1	0.02	0.03
	[-0.02; 0.06]	[-0.01; 0.06]
Increasing taxes x T2	0.03	0.02
	[-0.01; 0.07]	[-0.01; 0.05]
Defense	0.21*	0.15*
	[0.15; 0.26]	[0.11; 0.19]
Defense x T1	-0.10*	-0.05
	[-0.17; -0.03]	[-0.11; 0.01]
Defense x T2	-0.08*	-0.04
	[-0.15; -0.00]	[-0.09; 0.02]

Environment	0.10*	0.06*
	[0.05; 0.14]	[0.02; 0.10]
Environment x T1	-0.06	-0.02
	[-0.13; 0.00]	[-0.07; 0.04]
Environment x T2	-0.09*	-0.04
	[-0.16; -0.02]	[-0.10; 0.01]
Foreign aid	0.20*	0.16*
	[0.15; 0.25]	[0.12; 0.20]
Foreign aid x T1	-0.14*	-0.03
	[-0.21; -0.07]	[-0.09; 0.03]
Foreign aid x T2	-0.10*	-0.03
	[-0.17; -0.03]	[-0.08; 0.03]
Research development	0.10*	0.10*
	[0.05; 0.14]	[0.07; 0.14]
Research development x T1	-0.04	-0.02
	[-0.11; 0.02]	[-0.08; 0.04]
Research development x T2	-0.04	-0.04
	[-0.10; 0.03]	[-0.09; 0.01]
Pensions	0.08*	0.03
	[0.03; 0.13]	[-0.01; 0.07]
Pensions x T1	-0.09*	0.02
	[-0.16; -0.03]	[-0.04; 0.07]
Pensions x T2	-0.05	-0.01
	[-0.12; 0.01]	[-0.07; 0.04]
Social security	-0.01	-0.01
	[-0.06; 0.04]	[-0.05; 0.03]
Social security x T1	-0.02	-0.02
	[-0.09; 0.05]	[-0.08; 0.04]
Social security x T2	-0.03	-0.04
	[-0.09; 0.04]	[-0.10; 0.01]
Public transport	0.12*	0.10*
	[0.07; 0.16]	[0.06; 0.13]
Public transport x T1	-0.08*	-0.01
	[-0.15; -0.02]	[-0.07; 0.04]
Public transport x T2	-0.05	-0.02
	[-0.12; 0.01]	[-0.08; 0.03]
T1	0.05	-0.01
	[-0.02; 0.11]	[-0.07; 0.04]
T2	0.03	0.04
	[-0.04; 0.09]	[-0.01; 0.09]
R ²	0.11	0.11
Adj. R ²	0.11	0.11
Num. obs.	7200	10548
RMSE	0.28	0.28
N Clusters	894	1307

* Null hypothesis value outside the confidence interval.

Table E10: Heterogeneous effects with top value of life

	Top value of life	Not top value of life
(Intercept)	0.34*	0.40*
	[0.31; 0.38]	[0.36; 0.44]
Companies	0.08*	0.05*
	[0.05; 0.11]	[0.02; 0.08]
Companies x T1	-0.00	-0.01
	[-0.04; 0.04]	[-0.06; 0.03]
Companies x T2	-0.03	-0.00
	[-0.07; 0.01]	[-0.04; 0.04]
Older people	-0.16*	-0.11*
	[-0.19; -0.13]	[-0.14; -0.08]
Older people x T1	0.05*	0.01
	[0.00; 0.09]	[-0.03; 0.05]
Older people x T2	0.02	-0.00
	[-0.03; 0.06]	[-0.04; 0.04]
Wealthy	0.09*	0.07*
	[0.06; 0.12]	[0.04; 0.10]
Wealthy x T1	0.04	-0.01
	[-0.00; 0.09]	[-0.05; 0.03]
Wealthy x T2	-0.00	0.01
	[-0.05; 0.04]	[-0.03; 0.05]
Cutting spending	-0.02	0.00
	[-0.05; 0.00]	[-0.02; 0.03]
Cutting spending x T1	0.03	-0.02
	[-0.00; 0.07]	[-0.05; 0.01]
Cutting spending x T2	0.02	-0.02
	[-0.01; 0.06]	[-0.05; 0.01]
Increasing taxes	-0.05*	-0.03*
	[-0.08; -0.02]	[-0.05; -0.01]
Increasing taxes x T1	0.04	0.01
	[-0.00; 0.08]	[-0.02; 0.04]
Increasing taxes x T2	0.06*	0.00
	[0.02; 0.09]	[-0.03; 0.03]
Defense	0.20*	0.15*
	[0.16; 0.25]	[0.10; 0.19]
Defense x T1	-0.08*	-0.05
	[-0.14; -0.01]	[-0.11; 0.01]
Defense x T2	-0.06	-0.04
	[-0.12; 0.01]	[-0.10; 0.01]

Environment	0.08*	0.07*
	[0.04; 0.13]	[0.03; 0.11]
Environment x T1	-0.06	-0.02
	[-0.12; 0.01]	[-0.08; 0.04]
Environment x T2	-0.06	-0.05
	[-0.12; 0.01]	[-0.11; 0.00]
Foreign aid	0.21*	0.15*
	[0.17; 0.26]	[0.11; 0.19]
Foreign aid x T1	-0.09*	-0.05
	[-0.16; -0.03]	[-0.11; 0.00]
Foreign aid x T2	-0.09*	-0.03
	[-0.15; -0.02]	[-0.08; 0.03]
Research development	0.11*	0.08*
	[0.07; 0.15]	[0.05; 0.12]
Research development x T1	-0.03	-0.03
	[-0.09; 0.03]	[-0.08; 0.03]
Research development x T2	-0.02	-0.04
	[-0.08; 0.04]	[-0.09; 0.01]
Pensions	0.06*	0.04
	[0.02; 0.10]	[-0.00; 0.08]
Pensions x T1	-0.04	-0.02
	[-0.10; 0.02]	[-0.08; 0.04]
Pensions x T2	-0.02	-0.03
	[-0.08; 0.05]	[-0.09; 0.02]
Social security	-0.01	-0.00
	[-0.06; 0.03]	[-0.04; 0.04]
Social security x T1	-0.01	-0.02
	[-0.07; 0.05]	[-0.08; 0.04]
Social security x T2	-0.04	-0.03
	[-0.10; 0.02]	[-0.09; 0.02]
Public transport	0.13*	0.08*
	[0.08; 0.17]	[0.04; 0.13]
Public transport x T1	-0.04	-0.04
	[-0.10; 0.02]	[-0.09; 0.02]
Public transport x T2	-0.04	-0.03
	[-0.10; 0.02]	[-0.08; 0.03]
T1	-0.01	0.03
	[-0.07; 0.04]	[-0.03; 0.08]
T2	0.02	0.04
	[-0.03; 0.08]	[-0.02; 0.09]
R ²	0.15	0.09
Adj. R ²	0.14	0.08
Num. obs.	7904	10212
RMSE	0.28	0.27
N Clusters	986	1259

* Null hypothesis value outside the confidence interval.

Table E11: Heterogeneous effects with aggregate preferences for the allocation of tax burden (Conditioning on “Who should pay?”)

	Companies	Older people	Wealthy
(Intercept)	0.44*	0.28*	0.44*
	[0.39; 0.48]	[0.23; 0.32]	[0.39; 0.48]
Companies x T1	-0.04		
	[-0.10; 0.03]		
Companies x T2	0.02		
	[-0.04; 0.09]		
Older people x T1		0.04	
		[-0.03; 0.11]	
Older people x T2		0.04	
		[-0.02; 0.11]	
Wealthy x T1			0.02
			[-0.05; 0.09]
Wealthy x T2			0.04
			[-0.02; 0.11]
Cutting spending	-0.00	0.00	-0.06*
	[-0.04; 0.04]	[-0.03; 0.04]	[-0.09; -0.02]
Cutting spending x T1	-0.00	-0.01	0.05
	[-0.05; 0.05]	[-0.06; 0.04]	[-0.01; 0.10]
Cutting spending x T2	0.01	-0.01	0.04
	[-0.04; 0.06]	[-0.06; 0.04]	[-0.01; 0.10]
Increasing taxes	-0.08*	-0.02	-0.05*
	[-0.11; -0.04]	[-0.06; 0.01]	[-0.08; -0.01]
Increasing taxes x T1	0.07*	0.02	0.02
	[0.02; 0.13]	[-0.03; 0.06]	[-0.03; 0.07]
Increasing taxes x T2	0.05	0.03	0.05*
	[-0.00; 0.10]	[-0.02; 0.07]	[0.00; 0.11]
Defense	0.20*	0.09*	0.21*
	[0.13; 0.26]	[0.03; 0.14]	[0.15; 0.27]
Defense x T1	-0.06	-0.08	-0.04
	[-0.15; 0.03]	[-0.17; 0.01]	[-0.13; 0.05]
Defense x T2	-0.05	-0.04	-0.07
	[-0.13; 0.04]	[-0.12; 0.04]	[-0.16; 0.01]

Environment	0.06 [−0.00; 0.12]	0.02 [−0.03; 0.08]	0.13* [0.06; 0.19]
Environment X T1	0.02 [−0.06; 0.10]	0.01 [−0.08; 0.09]	−0.10* [−0.19; −0.01]
Environment x T2	−0.07 [−0.15; 0.02]	−0.04 [−0.12; 0.04]	−0.09 [−0.18; 0.00]
Foreign aid	0.17* [0.11; 0.24]	0.13* [0.07; 0.19]	0.22* [0.16; 0.28]
Foreign aid x T1	−0.03 [−0.12; 0.07]	−0.11* [−0.20; −0.03]	−0.08 [−0.16; 0.01]
Foreign aid x T2	−0.04 [−0.13; 0.05]	−0.06 [−0.14; 0.02]	−0.08 [−0.17; 0.00]
Research development	0.12* [0.06; 0.18]	0.04 [−0.01; 0.10]	0.14* [0.08; 0.19]
Research development x T1	−0.02 [−0.11; 0.06]	−0.03 [−0.11; 0.06]	−0.03 [−0.12; 0.05]
Research development X T2	−0.07 [−0.15; 0.01]	−0.04 [−0.12; 0.03]	−0.02 [−0.10; 0.06]
Pensions	0.04 [−0.02; 0.10]	0.02 [−0.04; 0.08]	0.09* [0.03; 0.15]
Pensions x T1	−0.00 [−0.09; 0.08]	−0.02 [−0.10; 0.07]	−0.07 [−0.16; 0.01]
Pensions x T2	−0.02 [−0.10; 0.07]	−0.02 [−0.09; 0.06]	−0.07 [−0.15; 0.01]
Social security	−0.02 [−0.07; 0.04]	−0.04 [−0.09; 0.02]	0.00 [−0.05; 0.06]
Social security x T1	−0.00 [−0.09; 0.08]	−0.00 [−0.08; 0.08]	−0.01 [−0.10; 0.07]
Social security x T2	−0.06 [−0.14; 0.03]	−0.05 [−0.13; 0.03]	−0.06 [−0.14; 0.03]
Public transport	0.14* [0.08; 0.19]	0.03 [−0.03; 0.08]	0.14* [0.08; 0.20]
Public transport x T1	−0.03 [−0.11; 0.06]	−0.02 [−0.10; 0.05]	−0.03 [−0.12; 0.05]
Public transport x T2	−0.06 [−0.14; 0.02]	−0.01 [−0.08; 0.07]	−0.05 [−0.13; 0.03]
R ²	0.06	0.02	0.06
Adj. R ²	0.06	0.02	0.06
Num. obs.	4506	4477	4516
RMSE	0.28	0.27	0.29
N Clusters	2022	2008	2038

* Null hypothesis value outside the confidence interval.

F Regression tables with binary outcome

F.1 Heterogeneous effects with party – binary outcome

F.2 Heterogeneous effects with presidential vote – binary outcome

F.3 Heterogeneous effects with income – binary outcome

F.4 Heterogeneous effects with age – binary outcome

Table F1: Heterogeneous effects with party - binary outcome

	Republican	Democrat	Independent
(Intercept)	0.58*	0.38*	0.36*
	[0.48; 0.68]	[0.32; 0.44]	[0.27; 0.46]
Companies	0.04	0.12*	0.07
	[−0.05; 0.12]	[0.07; 0.17]	[−0.01; 0.16]
Companies x T1	−0.05	−0.00	0.06
	[−0.16; 0.06]	[−0.07; 0.07]	[−0.06; 0.19]
Companies x T2	0.07	0.01	−0.01
	[−0.05; 0.19]	[−0.06; 0.07]	[−0.13; 0.11]
Older people	−0.22*	−0.21*	−0.20*
	[−0.30; −0.15]	[−0.26; −0.17]	[−0.28; −0.11]
Older people x T1	0.01	−0.00	0.03
	[−0.10; 0.12]	[−0.07; 0.07]	[−0.09; 0.16]
Older people x T2	0.11*	0.01	−0.02
	[0.00; 0.22]	[−0.06; 0.07]	[−0.14; 0.10]
Wealthy	−0.01	0.18*	0.10*
	[−0.09; 0.06]	[0.13; 0.24]	[0.01; 0.19]
Wealthy x T1	0.08	0.01	0.07
	[−0.03; 0.19]	[−0.06; 0.08]	[−0.05; 0.19]
Wealthy x T2	0.15*	−0.01	−0.05
	[0.04; 0.26]	[−0.08; 0.06]	[−0.18; 0.08]
Cutting spending	0.02	−0.05*	−0.00
	[−0.04; 0.08]	[−0.09; −0.00]	[−0.07; 0.07]
Cutting spending x T1	−0.01	0.02	0.02
	[−0.10; 0.07]	[−0.04; 0.08]	[−0.08; 0.12]
Cutting spending x T2	0.01	0.01	−0.03
	[−0.08; 0.10]	[−0.05; 0.07]	[−0.13; 0.08]
Increasing taxes	−0.09*	−0.05*	−0.02
	[−0.15; −0.02]	[−0.09; −0.01]	[−0.10; 0.05]
Increasing taxes x T1	0.03	0.03	−0.00
	[−0.06; 0.12]	[−0.03; 0.08]	[−0.11; 0.10]
Increasing taxes x T2	−0.01	0.02	−0.06
	[−0.10; 0.08]	[−0.04; 0.08]	[−0.16; 0.04]
Defense	−0.03	0.30*	0.22*
	[−0.14; 0.08]	[0.23; 0.37]	[0.09; 0.35]
Defense x T1	−0.02	−0.02	0.03
	[−0.19; 0.16]	[−0.12; 0.08]	[−0.15; 0.21]
Defense x T2	0.02	−0.01	−0.04
	[−0.14; 0.18]	[−0.11; 0.08]	[−0.22; 0.13]

Environment	0.03 [−0.08; 0.14]	0.10* [0.02; 0.17]	0.18* [0.07; 0.30]
Environment x T1	0.11 [−0.06; 0.27]	0.03 [−0.07; 0.12]	−0.03 [−0.19; 0.14]
Environment x T2	0.00 [−0.17; 0.17]	−0.03 [−0.13; 0.07]	−0.18* [−0.35; −0.02]
Foreign aid	0.16* [0.06; 0.27]	0.22* [0.15; 0.29]	0.32* [0.21; 0.43]
Foreign aid x T1	0.03 [−0.12; 0.19]	0.02 [−0.08; 0.12]	−0.04 [−0.19; 0.12]
Foreign aid x T2	−0.04 [−0.20; 0.12]	0.05 [−0.05; 0.15]	−0.18* [−0.34; −0.02]
Research development	0.02 [−0.09; 0.12]	0.11* [0.04; 0.17]	0.20* [0.07; 0.34]
Research development x T1	0.01 [−0.15; 0.17]	0.06 [−0.03; 0.16]	0.01 [−0.17; 0.18]
Research development x T2	0.03 [−0.12; 0.18]	0.08 [−0.02; 0.18]	−0.16 [−0.33; 0.02]
Pensions	−0.10 [−0.20; 0.01]	0.07 [−0.00; 0.14]	0.10 [−0.02; 0.22]
Pensions x T1	0.10 [−0.06; 0.26]	0.02 [−0.08; 0.12]	−0.01 [−0.17; 0.16]
Pensions x T2	0.02 [−0.13; 0.16]	0.02 [−0.08; 0.12]	−0.08 [−0.25; 0.09]
Social security	−0.19* [−0.30; −0.08]	0.01 [−0.06; 0.08]	−0.02 [−0.15; 0.10]
Social security x T1	−0.07 [−0.22; 0.08]	0.03 [−0.07; 0.12]	−0.02 [−0.19; 0.14]
Social security x T2	0.02 [−0.14; 0.18]	0.01 [−0.08; 0.11]	−0.12 [−0.28; 0.04]
Public transport	0.05 [−0.06; 0.16]	0.15* [0.09; 0.22]	0.15* [0.03; 0.26]
Public transport x T1	0.03 [−0.14; 0.19]	0.05 [−0.04; 0.14]	0.01 [−0.15; 0.17]
Public transport x T2	−0.01 [−0.16; 0.14]	0.03 [−0.07; 0.12]	−0.07 [−0.23; 0.09]
T1	−0.05 [−0.18; 0.09]	−0.03 [−0.12; 0.05]	−0.04 [−0.18; 0.10]
T2	−0.09 [−0.22; 0.04]	−0.02 [−0.11; 0.07]	0.17* [0.02; 0.31]
R ²	0.09	0.13	0.10
Adj. R ²	0.08	0.13	0.09
Num. obs.	3902	10014	3200
RMSE	0.50	0.47	0.48
N Clusters	478	1248	398

* Null hypothesis value outside the confidence interval.

Table F2: Heterogeneous effects with presidential vote - binary outcome

	D.Trump	H.Clinton	Did not vote
(Intercept)	0.54*	0.42*	0.37*
	[0.44; 0.64]	[0.35; 0.49]	[0.27; 0.46]
Companies	0.04	0.12*	0.08
	[−0.04; 0.12]	[0.06; 0.17]	[−0.00; 0.16]
Companies x T1	−0.04	0.01	−0.03
	[−0.15; 0.07]	[−0.07; 0.09]	[−0.14; 0.08]
Companies x T2	0.05	−0.01	0.06
	[−0.07; 0.16]	[−0.08; 0.07]	[−0.04; 0.17]
Older people	−0.23*	−0.23*	−0.17*
	[−0.31; −0.14]	[−0.28; −0.18]	[−0.25; −0.09]
Older people x T1	−0.01	0.02	−0.03
	[−0.12; 0.11]	[−0.05; 0.10]	[−0.13; 0.08]
Older people x T2	0.10	0.02	−0.01
	[−0.01; 0.22]	[−0.05; 0.09]	[−0.11; 0.09]
Wealthy	0.01	0.17*	0.14*
	[−0.08; 0.09]	[0.12; 0.23]	[0.06; 0.22]
Wealthy x T1	0.05	0.05	−0.00
	[−0.07; 0.16]	[−0.03; 0.13]	[−0.11; 0.11]
Wealthy x T2	0.09	−0.01	0.03
	[−0.03; 0.21]	[−0.08; 0.07]	[−0.07; 0.13]
Cutting spending	0.04	−0.04	−0.04
	[−0.02; 0.11]	[−0.09; 0.00]	[−0.09; 0.02]
Cutting spending x T1	−0.02	0.03	0.01
	[−0.11; 0.07]	[−0.04; 0.09]	[−0.08; 0.09]
Cutting spending x T2	−0.02	−0.00	0.00
	[−0.12; 0.07]	[−0.07; 0.06]	[−0.07; 0.08]
Increasing taxes	−0.05	−0.08*	−0.04
	[−0.12; 0.02]	[−0.13; −0.03]	[−0.10; 0.03]
Increasing taxes x T1	0.02	0.07*	−0.04
	[−0.07; 0.11]	[0.01; 0.13]	[−0.12; 0.04]
Increasing taxes x T2	−0.02	0.02	−0.03
	[−0.12; 0.07]	[−0.04; 0.09]	[−0.12; 0.06]
Defense	−0.06	0.29*	0.28*
	[−0.18; 0.06]	[0.21; 0.37]	[0.17; 0.38]
Defense x T1	0.03	0.01	−0.08
	[−0.16; 0.21]	[−0.10; 0.13]	[−0.23; 0.07]
Defense x T2	0.06	0.02	−0.16*
	[−0.10; 0.23]	[−0.09; 0.13]	[−0.30; −0.02]

Environment	0.11 [−0.01; 0.23]	0.07 [−0.01; 0.16]	0.09 [−0.01; 0.19]
Environment x T1	−0.00 [−0.18; 0.17]	0.03 [−0.08; 0.14]	0.10 [−0.04; 0.24]
Environment x T2	−0.06 [−0.23; 0.10]	−0.00 [−0.12; 0.11]	−0.13 [−0.27; 0.01]
Foreign aid	0.16* [0.05; 0.27]	0.22* [0.14; 0.30]	0.29* [0.19; 0.39]
Foreign aid x T1	0.03 [−0.14; 0.19]	0.01 [−0.10; 0.13]	−0.01 [−0.16; 0.13]
Foreign aid x T2	0.04 [−0.12; 0.20]	0.01 [−0.10; 0.13]	−0.14* [−0.28; −0.01]
Research development	0.07 [−0.04; 0.18]	0.12* [0.05; 0.20]	0.06 [−0.05; 0.17]
Research development x T1	−0.01 [−0.18; 0.16]	0.05 [−0.06; 0.16]	0.05 [−0.09; 0.19]
Research development x T2	−0.04 [−0.20; 0.11]	0.11 [−0.00; 0.21]	−0.03 [−0.17; 0.10]
Pensions	−0.08 [−0.19; 0.03]	0.03 [−0.05; 0.12]	0.14* [0.04; 0.24]
Pensions x T1	0.03 [−0.14; 0.20]	0.01 [−0.10; 0.13]	0.03 [−0.11; 0.17]
Pensions x T2	−0.00 [−0.16; 0.15]	0.04 [−0.08; 0.15]	−0.11 [−0.25; 0.02]
Social security	−0.18* [−0.29; −0.07]	−0.07 [−0.15; 0.01]	0.07 [−0.03; 0.17]
Social security x T1	−0.09 [−0.24; 0.07]	0.04 [−0.07; 0.15]	0.01 [−0.13; 0.16]
Social security x T2	−0.00 [−0.16; 0.16]	0.07 [−0.04; 0.18]	−0.10 [−0.24; 0.03]
Public transport	0.01 [−0.09; 0.12]	0.12* [0.05; 0.20]	0.23* [0.13; 0.34]
Public transport x T1	0.10 [−0.07; 0.26]	0.08 [−0.03; 0.18]	−0.05 [−0.19; 0.09]
Public transport x T2	0.03 [−0.11; 0.17]	0.06 [−0.04; 0.17]	−0.16* [−0.30; −0.03]
T1	−0.00 [−0.15; 0.14]	−0.07 [−0.17; 0.03]	0.02 [−0.11; 0.15]
T2	−0.04 [−0.18; 0.09]	−0.04 [−0.14; 0.06]	0.10 [−0.03; 0.22]
R ²	0.10	0.14	0.09
Adj. R ²	0.09	0.14	0.09
Num. obs.	3688	7854	4846
RMSE	0.50	0.47	0.47
N Clusters	449	976	604

* Null hypothesis value outside the confidence interval.

Table F3: Heterogeneous effects with income - binary outcome

	Under 20k	20-60k	60-100k	100k+
(Intercept)	0.49*	0.38*	0.44*	0.49*
	[0.35; 0.63]	[0.31; 0.46]	[0.34; 0.55]	[0.39; 0.59]
Companies	0.07	0.09*	0.09*	0.08
	[−0.03; 0.16]	[0.03; 0.15]	[0.01; 0.17]	[−0.01; 0.18]
Companies x T1	−0.04	−0.01	0.02	0.05
	[−0.19; 0.10]	[−0.09; 0.08]	[−0.09; 0.13]	[−0.08; 0.17]
Companies x T2	0.09	0.01	−0.01	0.01
	[−0.04; 0.22]	[−0.08; 0.09]	[−0.12; 0.09]	[−0.12; 0.13]
Older people	−0.27*	−0.20*	−0.17*	−0.23*
	[−0.37; −0.18]	[−0.26; −0.15]	[−0.25; −0.09]	[−0.31; −0.15]
Older people x T1	0.09	−0.03	−0.03	0.03
	[−0.04; 0.22]	[−0.12; 0.05]	[−0.13; 0.08]	[−0.09; 0.14]
Older people x T2	0.05	0.07	−0.06	0.02
	[−0.07; 0.18]	[−0.01; 0.15]	[−0.17; 0.05]	[−0.09; 0.14]
Wealthy	0.07	0.14*	0.12*	0.13*
	[−0.05; 0.18]	[0.08; 0.20]	[0.04; 0.20]	[0.04; 0.22]
Wealthy x T1	0.14	−0.03	0.08	0.04
	[−0.00; 0.29]	[−0.12; 0.05]	[−0.02; 0.19]	[−0.07; 0.16]
Wealthy x T2	0.10	−0.02	−0.00	−0.02
	[−0.04; 0.25]	[−0.10; 0.07]	[−0.11; 0.11]	[−0.15; 0.11]
Cutting spending	−0.00	−0.03	−0.03	−0.04
	[−0.08; 0.08]	[−0.08; 0.02]	[−0.10; 0.04]	[−0.10; 0.02]
Cutting spending x T1	−0.02	0.02	0.04	0.07
	[−0.13; 0.09]	[−0.05; 0.09]	[−0.05; 0.12]	[−0.03; 0.16]
Cutting spending x T2	−0.02	−0.01	−0.00	0.06
	[−0.12; 0.09]	[−0.08; 0.06]	[−0.09; 0.08]	[−0.04; 0.16]
Increasing taxes	−0.09*	−0.07*	−0.06	−0.07
	[−0.16; −0.01]	[−0.11; −0.02]	[−0.14; 0.01]	[−0.14; 0.01]
Increasing taxes x T1	0.02	0.06	0.03	−0.00
	[−0.09; 0.14]	[−0.01; 0.12]	[−0.06; 0.12]	[−0.11; 0.10]
Increasing taxes x T2	0.03	−0.00	0.03	0.02
	[−0.08; 0.13]	[−0.07; 0.07]	[−0.06; 0.13]	[−0.08; 0.13]
Defense	0.25*	0.26*	0.21*	0.11
	[0.12; 0.37]	[0.17; 0.34]	[0.07; 0.34]	[−0.02; 0.24]
Defense x T1	−0.06	−0.12	0.00	0.16
	[−0.26; 0.14]	[−0.25; 0.01]	[−0.17; 0.17]	[−0.02; 0.35]
Defense x T2	−0.10	−0.07	−0.01	0.12
	[−0.27; 0.07]	[−0.19; 0.05]	[−0.18; 0.16]	[−0.08; 0.31]

Environment	0.03	0.15*	0.02	0.10
	[−0.11; 0.16]	[0.07; 0.23]	[−0.10; 0.14]	[−0.02; 0.22]
Environment x T1	0.08	−0.03	0.10	0.02
	[−0.11; 0.26]	[−0.15; 0.09]	[−0.05; 0.25]	[−0.15; 0.20]
Environment x T2	−0.06	−0.14*	0.09	−0.02
	[−0.24; 0.13]	[−0.26; −0.03]	[−0.07; 0.25]	[−0.20; 0.15]
Foreign aid	0.20*	0.26*	0.22*	0.16*
	[0.07; 0.33]	[0.18; 0.34]	[0.10; 0.34]	[0.04; 0.29]
Foreign aid x T1	0.00	−0.01	0.02	0.06
	[−0.20; 0.20]	[−0.13; 0.11]	[−0.13; 0.17]	[−0.12; 0.23]
Foreign aid x T2	0.01	−0.09	0.09	0.07
	[−0.16; 0.18]	[−0.21; 0.03]	[−0.07; 0.25]	[−0.10; 0.24]
Research development	0.10	0.16*	0.08	0.04
	[−0.02; 0.23]	[0.07; 0.25]	[−0.03; 0.19]	[−0.09; 0.16]
Research development x T1	0.10	−0.02	0.01	0.11
	[−0.09; 0.28]	[−0.15; 0.11]	[−0.14; 0.15]	[−0.07; 0.29]
Research development x T2	−0.03	−0.08	0.10	0.11
	[−0.20; 0.14]	[−0.20; 0.05]	[−0.04; 0.24]	[−0.06; 0.29]
Pensions	0.04	0.12*	−0.02	−0.10
	[−0.10; 0.18]	[0.04; 0.20]	[−0.14; 0.10]	[−0.22; 0.02]
Pensions x T1	−0.03	−0.01	0.09	0.11
	[−0.22; 0.17]	[−0.13; 0.11]	[−0.06; 0.24]	[−0.06; 0.28]
Pensions x T2	0.02	−0.11	0.14	0.06
	[−0.17; 0.20]	[−0.23; 0.01]	[−0.01; 0.29]	[−0.11; 0.22]
Social security	−0.01	−0.02	−0.04	−0.17*
	[−0.14; 0.12]	[−0.10; 0.07]	[−0.15; 0.08]	[−0.30; −0.05]
Social security x T1	−0.00	−0.12	0.02	0.17
	[−0.20; 0.19]	[−0.24; 0.00]	[−0.13; 0.17]	[−0.01; 0.36]
Social security x T2	−0.04	−0.09	0.03	0.16
	[−0.22; 0.14]	[−0.21; 0.03]	[−0.12; 0.19]	[−0.02; 0.34]
Public transport	0.04	0.18*	0.14*	0.11
	[−0.09; 0.16]	[0.10; 0.25]	[0.03; 0.24]	[−0.01; 0.23]
Public transport x T1	0.17	−0.02	0.03	−0.02
	[−0.03; 0.37]	[−0.13; 0.10]	[−0.11; 0.16]	[−0.20; 0.16]
Public transport x T2	0.08	−0.05	−0.01	0.02
	[−0.09; 0.25]	[−0.16; 0.06]	[−0.16; 0.13]	[−0.15; 0.19]
T1	−0.09	0.04	−0.07	−0.11
	[−0.28; 0.10]	[−0.07; 0.15]	[−0.20; 0.06]	[−0.26; 0.04]
T2	−0.04	0.07	−0.03	−0.08
	[−0.22; 0.13]	[−0.03; 0.18]	[−0.17; 0.10]	[−0.23; 0.06]
R ²	0.13	0.10	0.10	0.12
Adj. R ²	0.12	0.10	0.10	0.11
Num. obs.	2734	6848	4448	3126
RMSE	0.48	0.49	0.48	0.47
N Clusters	332	852	554	389

* Null hypothesis value outside the confidence interval.

Table F4: Heterogeneous effects with age - binary outcome

	18-24	25-34	35-49	50-64	65+
(Intercept)	0.30* [0.18; 0.42]	0.35* [0.25; 0.44]	0.41* [0.32; 0.50]	0.48* [0.39; 0.57]	0.54* [0.41; 0.67]
Companies	0.22* [0.11; 0.33]	0.11* [0.03; 0.19]	0.11* [0.04; 0.18]	0.07* [0.01; 0.14]	0.03 [-0.07; 0.13]
Companies x T1	-0.13 [-0.27; 0.02]	0.08 [-0.03; 0.19]	-0.03 [-0.13; 0.07]	0.03 [-0.07; 0.13]	-0.05 [-0.18; 0.09]
Companies x T2	-0.12 [-0.27; 0.03]	-0.02 [-0.12; 0.09]	0.05 [-0.05; 0.14]	-0.02 [-0.12; 0.08]	0.10 [-0.03; 0.24]
Older people	-0.08 [-0.19; 0.03]	-0.17* [-0.25; -0.09]	-0.19* [-0.26; -0.12]	-0.25* [-0.31; -0.18]	-0.29* [-0.37; -0.21]
Older people x T1	-0.03 [-0.17; 0.12]	0.06 [-0.07; 0.18]	-0.00 [-0.10; 0.09]	0.01 [-0.09; 0.10]	-0.05 [-0.17; 0.07]
Older people x T2	-0.11 [-0.26; 0.04]	-0.05 [-0.16; 0.06]	0.04 [-0.05; 0.13]	0.08 [-0.01; 0.18]	0.08 [-0.05; 0.21]
Wealthy	0.20* [0.09; 0.32]	0.19* [0.11; 0.27]	0.14* [0.07; 0.21]	0.12* [0.05; 0.19]	0.02 [-0.08; 0.12]
Wealthy x T1	0.05 [-0.10; 0.19]	0.01 [-0.11; 0.13]	0.01 [-0.08; 0.11]	0.07 [-0.03; 0.16]	0.03 [-0.11; 0.17]
Wealthy x T2	-0.10 [-0.25; 0.06]	-0.06 [-0.17; 0.06]	-0.00 [-0.10; 0.09]	0.06 [-0.04; 0.16]	0.06 [-0.08; 0.21]
Cutting spending	-0.01 [-0.09; 0.07]	-0.04 [-0.10; 0.02]	-0.04 [-0.10; 0.01]	-0.01 [-0.07; 0.05]	0.00 [-0.08; 0.08]
Cutting spending x T1	-0.01 [-0.12; 0.10]	0.05 [-0.04; 0.15]	0.00 [-0.08; 0.08]	0.02 [-0.07; 0.10]	0.04 [-0.06; 0.15]
Cutting spending x T2	0.03 [-0.09; 0.15]	0.01 [-0.08; 0.10]	0.01 [-0.08; 0.09]	-0.02 [-0.10; 0.07]	-0.01 [-0.12; 0.11]
Increasing taxes	-0.06 [-0.14; 0.03]	-0.08* [-0.14; -0.02]	-0.04 [-0.11; 0.02]	-0.07* [-0.13; -0.01]	-0.07 [-0.15; 0.01]
Increasing taxes x T1	-0.02 [-0.14; 0.10]	0.04 [-0.04; 0.13]	0.00 [-0.09; 0.09]	0.03 [-0.04; 0.11]	0.08 [-0.04; 0.19]
Increasing taxes x T2	-0.02 [-0.14; 0.11]	0.03 [-0.05; 0.11]	0.00 [-0.08; 0.09]	0.01 [-0.07; 0.10]	0.03 [-0.09; 0.14]
Defense	0.23* [0.09; 0.38]	0.33* [0.23; 0.44]	0.22* [0.11; 0.33]	0.15* [0.04; 0.26]	0.13 [-0.01; 0.28]
Defense x T1	0.04 [-0.16; 0.24]	-0.08 [-0.24; 0.08]	0.04 [-0.11; 0.18]	-0.02 [-0.17; 0.12]	-0.06 [-0.30; 0.19]
Defense x T2	0.02 [-0.19; 0.23]	-0.14 [-0.29; 0.01]	-0.02 [-0.16; 0.12]	-0.04 [-0.19; 0.12]	0.11 [-0.08; 0.30]

Environment	0.03 [−0.12; 0.18]	0.19* [0.08; 0.30]	0.08 [−0.02; 0.18]	0.09 [−0.01; 0.20]	0.03 [−0.10; 0.17]
Environment x T1	0.10 [−0.09; 0.30]	−0.02 [−0.19; 0.14]	0.03 [−0.10; 0.17]	−0.00 [−0.14; 0.13]	0.10 [−0.11; 0.30]
Environment x T2	−0.02 [−0.24; 0.19]	−0.11 [−0.26; 0.05]	−0.08 [−0.22; 0.06]	−0.08 [−0.23; 0.06]	0.07 [−0.12; 0.26]
Foreign aid	0.19* [0.06; 0.31]	0.23* [0.12; 0.34]	0.28* [0.18; 0.37]	0.21* [0.11; 0.32]	0.19* [0.05; 0.33]
Foreign aid x T1	0.18* [0.00; 0.36]	0.10 [−0.05; 0.25]	−0.10 [−0.24; 0.04]	−0.04 [−0.18; 0.10]	0.04 [−0.17; 0.26]
Foreign aid x T2	0.08 [−0.10; 0.26]	0.09 [−0.06; 0.24]	−0.11 [−0.24; 0.02]	−0.08 [−0.24; 0.07]	0.06 [−0.14; 0.26]
Research development	0.05 [−0.08; 0.19]	0.06 [−0.04; 0.17]	0.09 [−0.00; 0.19]	0.10 [−0.01; 0.20]	0.13 [−0.01; 0.27]
Research development x T1	0.09 [−0.10; 0.28]	0.07 [−0.09; 0.24]	0.07 [−0.07; 0.20]	−0.02 [−0.15; 0.12]	0.01 [−0.21; 0.23]
Research development x T2	0.15 [−0.06; 0.35]	0.05 [−0.10; 0.20]	0.00 [−0.13; 0.14]	0.01 [−0.14; 0.15]	0.02 [−0.16; 0.21]
Pensions	0.21* [0.08; 0.35]	0.16* [0.05; 0.27]	0.02 [−0.08; 0.12]	−0.02 [−0.12; 0.09]	−0.11 [−0.25; 0.03]
Pensions x T1	−0.04 [−0.22; 0.14]	0.01 [−0.15; 0.17]	0.06 [−0.08; 0.20]	−0.03 [−0.17; 0.11]	0.15 [−0.07; 0.36]
Pensions x T2	0.01 [−0.20; 0.21]	−0.06 [−0.21; 0.09]	−0.01 [−0.15; 0.13]	−0.03 [−0.17; 0.11]	0.12 [−0.06; 0.30]
Social security	0.13 [−0.01; 0.27]	0.08 [−0.03; 0.19]	−0.02 [−0.12; 0.08]	−0.14* [−0.24; −0.04]	−0.17* [−0.31; −0.03]
Social security x T1	0.04 [−0.16; 0.23]	−0.05 [−0.20; 0.11]	0.05 [−0.10; 0.19]	−0.03 [−0.16; 0.10]	−0.04 [−0.24; 0.17]
Social security x T2	0.08 [−0.13; 0.28]	−0.02 [−0.17; 0.14]	−0.01 [−0.14; 0.13]	−0.03 [−0.17; 0.10]	0.04 [−0.15; 0.23]
Public transport	0.20* [0.07; 0.34]	0.20* [0.09; 0.31]	0.09 [−0.00; 0.19]	0.07 [−0.02; 0.16]	0.13* [0.01; 0.26]
Public transport x T1	−0.07 [−0.26; 0.12]	0.00 [−0.16; 0.17]	0.08 [−0.05; 0.21]	−0.00 [−0.13; 0.13]	0.14 [−0.07; 0.35]
Public transport x T2	−0.03 [−0.23; 0.17]	0.01 [−0.14; 0.17]	−0.01 [−0.13; 0.12]	−0.05 [−0.18; 0.08]	0.06 [−0.11; 0.23]
T1	0.01 [−0.15; 0.18]	−0.07 [−0.21; 0.07]	−0.02 [−0.14; 0.10]	−0.02 [−0.14; 0.09]	−0.06 [−0.26; 0.13]
T2	0.05 [−0.12; 0.23]	0.04 [−0.09; 0.18]	0.01 [−0.11; 0.13]	0.02 [−0.11; 0.14]	−0.12 [−0.29; 0.05]
R ²	0.10	0.11	0.10	0.12	0.14
Adj. R ²	0.09	0.10	0.09	0.12	0.13
Num. obs.	2376	3712	5000	4710	2294
N Clusters	295	462	619	581	285

* Null hypothesis value outside the confidence interval.

G Fiscal preferences expressed in the ANES 2016

Tables G1 and G2 include t-tests for several fiscal preference questions in the American National Election Study (ANES) 2016, by partisanship (party affiliation and 2016 presidential vote), income and age. The questions capture the support for (1) increasing income taxes on people making over one million dollars per year, (2) cuts in government spending to help people pay for health insurance when they can't pay for it all themselves, (3) cuts in federal spending on social security, (4) cuts in federal spending on science and technology, (5) cuts in federal spending on aid to the poor, (6) cuts in federal spending on protecting the environment, and (7) cuts in defense spending. The first six items run from 1 to 3, whilst the last item (defense cuts) runs from 1 to 7. The highest values represent most support for the relevant proposal.

Table G1: Fiscal preferences in the ANES – Partisanship

	Party			2016 vote		
	Dem	Rep	T-statistic	Clinton	Trump	T-statistic
(1) Millionaire tax	2.72 (0.60)	2.23 (0.84)	12.63*** (2,676)	2.78 (0.55)	2.23 (0.83)	15.84*** (2,460)
(2) Health care cuts	1.47 (0.70)	2.22 (0.81)	-18.51*** (2,661)	1.42 (0.67)	2.22 (0.80)	-21.50*** (2,441)
(3) Social security cuts	1.34 (0.52)	1.60 (0.66)	-8.47 (2,672)	1.37 (0.54)	1.54 (0.63)	-5.95*** (2,456)
(4) Science/technology cuts	1.42 (0.58)	1.62 (0.71)	-5.90*** (2,674)	1.35 (0.56)	1.62 (0.70)	-8.31*** (2,454)
(5) Cuts in aid to the poor	1.42 (0.59)	2.07 (0.74)	-18.45*** (2,672)	1.44 (0.60)	2.09 (0.73)	-19.82*** (2,449)
(6) Environmental cuts	1.35 (0.55)	1.97 (0.77)	-17.93*** (2,672)	1.28 (0.49)	2.02 (0.75)	-24.26*** (2,456)
(7) Defense cuts	3.94 (1.53)	2.73 (1.31)	15.34*** (2,405)	4.12 (1.50)	2.73 (1.42)	17.96*** (2,228)

Notes: Table reports the mean values for each subgroup. Sample weights are used. Asterisks indicate significant differences in mean values between two groups from a Wald test of significance (with degrees of freedom in parentheses). Standard deviations are below the means, in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table G2: Fiscal preferences in the ANES – Income and age

	Income			Age		
	Low	High	T-statistic	Below 25	Above 65	T-statistic
(1) Millionaire tax	2.50 (0.77)	2.50 (0.75)	-0.06 (2,078)	2.43 (0.64)	2.58 (0.78)	-2.42** (1,204)
(2) Health care cuts	1.72 (0.83)	1.85 (0.82)	-2.64*** (2,064)	1.79 (0.66)	1.77 (0.88)	0.36 (1,192)
(3) Social security cuts	1.38 (0.59)	1.62 (0.64)	-6.80*** (2,079)	1.67 (0.56)	1.34 (0.57)	6.18*** (1,203)
(4) Science/technology cuts	1.55 (0.68)	1.43 (0.62)	3.12*** (2,076)	1.52 (0.57)	1.45 (0.66)	1.25 (1,197)
(5) Cuts in aid to the poor	1.52 (0.68)	1.86 (0.74)	-8.06*** (2,070)	1.64 (0.59)	1.77 (0.78)	-2.37** (1,197)
(6) Environmental cuts	1.53 (0.70)	1.64 (0.72)	-2.80*** (2,076)	1.52 (0.56)	1.71 (0.84)	-3.42*** (1,200)
(7) Defense cuts	3.46 (1.73)	3.68 (1.51)	-2.06** (1,833)	3.87 (1.34)	3.06 (1.69)	5.37*** (1,066)

Notes: Table reports the mean values for each subgroup. Sample weights are used. Asterisks indicate significant differences in mean values between two groups from a Wald test of significance (with degrees of freedom in parentheses). Standard deviations are below the means, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

H Support for fiscal policy packages

Tables [H1](#), [H2](#) and [H3](#) show possible combinations of policies in the conjoint experiment. In these three tables, we compute linear estimates of fiscal plans while varying 'policy tool' across tables and varying 'who pays' and 'policy areas for spending cuts' across packages. Table [H1](#) demonstrates packages with increasing taxes, while varying who should pay and areas for policy cuts. These combinations with increasing taxes are divided into two: those preferred in the control group (packages from A to U) and those preferred in the two treatment groups combined (V to P1). Table [H2](#) demonstrates packages with spending cuts, while varying who should pay and areas for policy cuts. These combinations with spending cuts are divided into two: those preferred in the control group (packages from Q1 to K2) and those preferred in the two treatment groups combined (L2 to F3). Table [H3](#) demonstrates packages with a balance between increasing taxes and spending cuts, while varying who should pay and areas for policy cuts. These combinations with a balance are divided into two: those preferred in the control group (packages from G3 to A4) and those preferred in the two treatment groups combined (B4 to V4).

Table H1: Linear estimates of policy combinations in the conjoint experiment for packages with increasing taxes

Policy combination	Assignment group	Who should pay?	Area for policy cut	Estimate	SE
A	Control	Companies	Defense	0.187	0.022
B	Control	Companies	Environment	0.086	0.021
C	Control	Companies	Foreign aid	0.190	0.023
D	Control	Companies	R&D	0.120	0.020
E	Control	Companies	Pensions	0.062	0.021
F	Control	Companies	Social security	-0.001	0.021
G	Control	Companies	Public transport	0.121	0.021
H	Control	Older people	Defense	-0.006	0.021
I	Control	Older people	Environment	-0.108	0.021
J	Control	Older people	Foreign aid	-0.004	0.023
K	Control	Older people	R&D	-0.074	0.019
L	Control	Older people	Pensions	-0.131	0.021
M	Control	Older people	Social security	-0.195	0.022
N	Control	Older people	Public transport	-0.073	0.021
O	Control	Wealthy	Defense	0.204	0.022
P	Control	Wealthy	Environment	0.103	0.022
Q	Control	Wealthy	Foreign aid	0.206	0.023
R	Control	Wealthy	R&D	0.137	0.020
S	Control	Wealthy	Pensions	0.079	0.021
T	Control	Wealthy	Social security	0.016	0.022
U	Control	Wealthy	Public transport	0.138	0.021
V	T1, T2	Companies	Defense	0.282	0.030
W	T1, T2	Companies	Environment	0.181	0.029
X	T1, T2	Companies	Foreign aid	0.285	0.029
Y	T1, T2	Companies	R&D	0.215	0.029
Z	T1, T2	Companies	Pensions	0.157	0.029
A1	T1, T2	Companies	Social security	0.094	0.028
B1	T1, T2	Companies	Public transport	0.216	0.029
C1	T1, T2	Older people	Defense	0.089	0.029
D1	T1, T2	Older people	Environment	-0.013	0.028
E1	T1, T2	Older people	Foreign aid	0.091	0.028
F1	T1, T2	Older people	R&D	0.021	0.028
G1	T1, T2	Older people	Pensions	-0.036	0.028
H1	T1, T2	Older people	Social security	-0.100	0.028
I1	T1, T2	Older people	Public transport	0.022	0.028
J1	T1, T2	Wealthy	Defense	0.299	0.030
K1	T1, T2	Wealthy	Environment	0.198	0.029
L1	T1, T2	Wealthy	Foreign aid	0.302	0.029
M1	T1, T2	Wealthy	R&D	0.232	0.028
N1	T1, T2	Wealthy	Pensions	0.174	0.029
O1	T1, T2	Wealthy	Social security	0.111	0.028
P1	T1, T2	Wealthy	Public transport	0.233	0.029

Table H2: Linear estimates of policy combinations in the conjoint experiment for packages with spending cuts

Policy combination	Assignment group	Who should pay?	Area for policy cut	Estimate	SE
Q1	Control	Companies	Defense	0.259	0.022
R1	Control	Companies	Environment	0.157	0.021
S1	Control	Companies	Foreign aid	0.261	0.022
T1	Control	Companies	R&D	0.191	0.021
U1	Control	Companies	Pensions	0.134	0.021
V1	Control	Companies	Social security	0.070	0.021
W1	Control	Companies	Public transport	0.192	0.021
X1	Control	Older people	Defense	0.065	0.021
Y1	Control	Older people	Environment	-0.036	0.021
Z1	Control	Older people	Foreign aid	0.068	0.022
A2	Control	Older people	R&D	-0.002	0.020
B2	Control	Older people	Pensions	-0.060	0.021
C2	Control	Older people	Social security	-0.123	0.021
D2	Control	Older people	Public transport	-0.001	0.021
E2	Control	Wealthy	Defense	0.275	0.022
F2	Control	Wealthy	Environment	0.174	0.021
G2	Control	Wealthy	Foreign aid	0.278	0.022
H2	Control	Wealthy	R&D	0.208	0.020
I2	Control	Wealthy	Pensions	0.151	0.021
J2	Control	Wealthy	Social security	0.087	0.021
K2	Control	Wealthy	Public transport	0.209	0.021
L2	T1, T2	Companies	Defense	0.189	0.031
M2	T1, T2	Companies	Environment	0.088	0.031
N2	T1, T2	Companies	Foreign aid	0.192	0.031
O2	T1, T2	Companies	R&D	0.122	0.030
P2	T1, T2	Companies	Pensions	0.064	0.031
Q2	T1, T2	Companies	Social security	0.001	0.031
R2	T1, T2	Companies	Public transport	0.123	0.030
S2	T1, T2	Older people	Defense	0.005	0.031
T2	T1, T2	Older people	Environment	-0.106	0.030
U2	T1, T2	Older people	Foreign aid	0.002	0.031
V2	T1, T2	Older people	R&D	-0.072	0.029
W2	T1, T2	Older people	Pensions	-0.129	0.030
X2	T1, T2	Older people	Social security	-0.193	0.031
Y2	T1, T2	Older people	Public transport	-0.071	0.030
Z2	T1, T2	Wealthy	Defense	0.206	0.032
A3	T1, T2	Wealthy	Environment	0.105	0.031
B3	T1, T2	Wealthy	Foreign aid	0.208	0.032
C3	T1, T2	Wealthy	R&D	0.138	0.030
D3	T1, T2	Wealthy	Pensions	0.081	0.031
E3	T1, T2	Wealthy	Social security	0.014	0.032
F3	T1, T2	Wealthy	Public transport	0.140	0.031

Table H3: Linear estimates of policy combinations in the conjoint experiment for packages with an equal proportion of increasing taxes and spending cuts

Policy combination	Assignment group	Who should pay?	Area for policy cut	Estimate	SE
G3	Control	Companies	Defense	0.269	0.022
H3	Control	Companies	Environment	0.167	0.021
I3	Control	Companies	Foreign aid	0.271	0.021
J3	Control	Companies	R&D	0.201	0.021
K3	Control	Companies	Pensions	0.144	0.021
L3	Control	Companies	Social security	0.080	0.021
M3	Control	Companies	Public transport	0.202	0.021
N3	Control	Older people	Defense	0.075	0.022
O3	Control	Older people	Environment	-0.026	0.021
P3	Control	Older people	Foreign aid	0.078	0.021
Q3	Control	Older people	R&D	0.008	0.020
R3	Control	Older people	Pensions	-0.050	0.021
S3	Control	Older people	Social security	-0.113	0.021
T3	Control	Older people	Public transport	0.009	0.020
U3	Control	Wealthy	Defense	0.285	0.022
V3	Control	Wealthy	Environment	0.184	0.021
W3	Control	Wealthy	Foreign aid	0.288	0.021
X3	Control	Wealthy	R&D	0.218	0.020
Y3	Control	Wealthy	Pensions	0.161	0.021
Z3	Control	Wealthy	Social security	0.097	0.021
A4	Control	Wealthy	Public transport	0.219	0.021
B4	T1, T2	Companies	Defense	0.173	0.030
C4	T1, T2	Companies	Environment	0.072	0.029
D4	T1, T2	Companies	Foreign aid	0.176	0.031
E4	T1, T2	Companies	R&D	0.106	0.028
F4	T1, T2	Companies	Pensions	0.049	0.029
G4	T1, T2	Companies	Social security	-0.015	0.029
H4	T1, T2	Companies	Public transport	0.107	0.029
I4	T1, T2	Older people	Defense	-0.020	0.029
J4	T1, T2	Older people	Environment	-0.121	0.029
K4	T1, T2	Older people	Foreign aid	-0.017	0.030
L4	T1, T2	Older people	R&D	-0.087	0.027
M4	T1, T2	Older people	Pensions	-0.145	0.029
N4	T1, T2	Older people	Social security	-0.208	0.030
O4	T1, T2	Older people	Public transport	-0.086	0.029
P4	T1, T2	Wealthy	Defense	0.190	0.030
Q4	T1, T2	Wealthy	Environment	0.089	0.029
R4	T1, T2	Wealthy	Foreign aid	0.193	0.031
S4	T1, T2	Wealthy	R&D	0.123	0.028
T4	T1, T2	Wealthy	Pensions	0.066	0.029
U4	T1, T2	Wealthy	Social security	0.002	0.030
V4	T1, T2	Wealthy	Public transport	0.124	0.029