

Does information reduce partisan policy polarization? A survey experiment on post-pandemic fiscal policy preferences*

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Abstract

Does information about the world have the potential to reduce partisan policy polarization? To answer this question, we embed information treatments in a conjoint survey experiment on post-pandemic fiscal adjustment in the United States. We find that there is both partisan reality polarization and partisan policy polarization; yet, the policy differences disappear with exposure to information about COVID-19 deaths and income losses. Moreover, a cross-party reset in preferences towards tax-based fiscal adjustment occurs under the influence of information, from opposition to endorsement. These effects of information on preferences do not appear to be driven by Bayesian updating; instead, our analysis suggests a psychological mechanism whereby information triggers change in people's affective state which, in turn, has only a short-term effect on preferences. Hence, we conclude that information reduces polarization only in a specific and limited way. The latter amplifies the importance of 'windows of opportunity' for policy reform.

Keywords: anxiety, bad news, conjoint experiment, COVID-19, fiscal preferences, information, polarization, policy reset, partisanship

JEL codes: C90, D72, D83, D91, H20, H30, Z13

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I venture the judgment, however, that currently in the Western world, and especially in the United States, differences about economic policy among disinterested citizens derive predominantly from different predictions about the economic consequences of taking action – differences that in principle can be eliminated by the progress of positive economics – rather than from fundamental differences in basic values, differences about which men can ultimately only fight (Friedman 1953, 5).

1 Introduction

It is well known that differences in people's policy preferences tend to map onto their respective party or political affiliations (e.g., [Gerber et al. 2010](#)). People typically identify with a party because they agree with its policies and so this tendency towards partisan policy polarization is hardly surprising.¹ In this paper, we use a conjoint survey experiment to test whether the provision of potentially relevant information about the world affects the degree of this partisan policy polarization.

This test is important for two reasons. First, as exemplified by the Friedman quote above, there is a common presumption that many policy differences arise from differences in people's perceptions about the way the world, in relevant respects, works or is. If this is the case, then it is reasonable to expect – as Friedman did – that the provision of policy-relevant information with respect to aspects of the world over which there are current differences in opinion will shrink partisan policy differences. There is evidence that people with different policy preferences over issues like immigration, social mobility, economic inequality, and taxation do indeed perceive aspects of the relevant reality in different ways (see [Alesina et al. 2018a,b, 2020](#); [Kuziemko et al. 2015](#); [Stantcheva 2020](#)). In this sense, a partisan policy polarization is connected to a polarized partisan perception of reality. There is less evidence, however, on how the provision of policy relevant information affects these partisan policy differences. If anything, Friedman's optimism seems misplaced: the policy differences identified by [Alesina et al. \(2018b\)](#) seem immune to the provision of relevant information.

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

Our first contribution is, therefore, to supply a further test of Friedman’s optimism in this respect. Contrary to [Alesina et al. \(2018b\)](#), we find that information does dissolve partisan policy polarization.

Second, there are a range of survey and field experiments that, while not directly addressing partisan policy differences, still also find that the provision of information about the world does affect preferences and behavior (e.g., [Dupas 2011](#); [Lieberman and Luttmer 2015](#); [Corbacho et al. 2016](#)). Two questions arise about this evidence and the specific and similar result we find on the effect of information on policy polarization: (i) What is the mechanism linking information provision to the change in policy preferences? (ii) Is such an effect long-lasting?

Our survey experiment addresses both questions. Friedman’s optimism would follow naturally from a Bayesian view of how new information affects behavior. If people hold divergent relevant opinions about the world, then the provision of reliable information on these disputed points could narrow any related policy divergence and this attenuation should, *ceteris paribus*, prove long-lasting. Yet, we find no evidence to support the Bayesian mechanism, and the effect on policy polarization that we detect does not prove long-lasting. Instead, our results are broadly consistent with findings in psychology where information influences anxiety and mood, with changes in these affective states having short-term effects on people’s policy preferences.

More specifically, we examine, with a panel design across two waves of a conjoint survey experiment (conducted in April and October 2020), whether post-pandemic fiscal policy preferences are affected by the provision of reliable information on the then-expected economic and health costs of COVID-19 in 2020. The conjoint part of the experiment asks respondents to consider, in the standard manner of these experiments, random combinations of fiscal policy attributes. We include three attributes in each fiscal policy package that respondents are asked to compare. (i) Who should bear the financial burden of post-pandemic fiscal adjustments? (ii) Which policy tool should be used to fund the financial burden? (iii)

Where should any spending cuts fall? Each attribute has a number of possible values, with these values being randomly assigned to the policy packages that respondents compare.

There are two treatment arms where respondents receive reliable information on, respectively, the then expected health and economic costs of COVID-19. For this purpose, we use in one treatment the number of COVID-19 deaths in the next three months, as predicted by the Institute for Health Metrics and Evaluation (IHME), and in the other the IMF-predicted average income losses by the end of the year 2020. At the point in the survey when the respondents in the treatment arms receive their information, those in the control group hear a piece of instrumental music.

Our test for an information provision effect on policy preferences comes from a comparison of the policy preferences in the treatment arms with those in the control in the April wave. Our test for whether any information effects are enduring comes (a) from a within-subject comparison of the policy preferences of those in the treatment arms in April with their preferences in October, and (b) a between-subject comparison of the policy preferences in October of those in the treatment arms in April and those in the control in April.

We focus on post-pandemic fiscal policy preferences for several reasons. Post-pandemic fiscal policy was a developing policy issue at the time. COVID-19 was contributing to significant growth in public debt and the extra debt would generate either additional interest servicing and/or debt repayment costs. A key fiscal policy question was, therefore, naturally emerging at the time: How was this future COVID-19 fiscal burden to be met? Which groups should bear the brunt of the burden? Will it be via new taxes or spending cuts? If by spending cuts, where will they fall? The fiscal policy adjustment focus of survey experiment was not only timely for this reason, but it also touched on a policy domain where there is known to be partisan polarization (e.g., [Alesina and Drazen 1991](#); [Alesina et al. 2006, 2011](#); [Alesina and Ardagna 2013](#); [Fisman et al. 2020](#)). Finally, the expected health and economic costs of COVID-19 at the time might plausibly affect the expression of fiscal policy preferences. For instance, two people who held divergent views on the relative

magnitudes of the health and economic costs of COVID-19 might, as a result, plausibly hold different policy preferences. This is because the health costs of COVID-19 were typically concentrated on the old, while the economic costs fell largely on those of working age. Thus, the differences in beliefs about the relative magnitude of the two types of costs would translate into different views regarding which groups in society were shouldering the burden of COVID-19; and this, in turn, might plausibly affect people's preferences for which groups should shoulder the burden of the post-pandemic fiscal adjustment. In these circumstances, the provision of reliable information on the respective likely economic and health costs might, *ceteris paribus*, be expected to narrow the policy differences.

We use a conjoint experiment because it is uniquely suited to elicit policy preferences and it has been shown to reduce social desirability bias in comparison to a partially randomized design (Horiuchi et al. 2022). The conjoint design can accommodate multi-faceted policy packages, allowing for the experimental manipulation of each policy attribute or dimension. In particular, and unlike conventional survey experiments, conjoint experiments allow us to estimate the effects of randomly assigned treatments with multiple attributes *simultaneously and independently* (Bansak et al. 2021b); conventional experiments, by contrast, are typically limited to analyzing the average effects of one or two randomly assigned treatments.² This *ceteris paribus* feature of conjoint experiments is particularly important when dimensions are co-dependent or co-determined. Moreover, the conjoint design has an additional desirable feature: it allows us to identify experimentally the winning policy (in electoral terms) by varying one policy element at a time, holding all else at their means. To our knowledge, we are the first to embed an information treatment into a conjoint experiment.

We begin the survey experiment with questions on COVID-19 to test for 'reality polarization'. We find evidence of such polarization among our respondents: Republicans (and Trump voters in 2016) both believe COVID-19 will result in significantly fewer death and

² We provide more details and explanation of the use and interpretation of conjoint experiments in Subsection 2.2.

are more likely to believe that COVID-19 is more serious than the seasonal flu, as compared with Democrats (and Clinton voters in 2016). In our control group in April, we also find evidence of partisan policy polarization: Democrats prefer for companies and the wealthy to shoulder the burden, but Republicans do not; furthermore, Republicans prefer spending cuts to any tax increase, while Democrats do not; finally, Democrats have a significantly stronger preference than Republicans for the cuts to fall on defense and pensions. Thus, the conditions are met for a test of the information effects: there is partisan reality polarization and partisan policy polarization in the control.

These policy differences disappear in our information treatments and a further interesting effect is observable: both Democrats and Republicans move to a clear preference for tax increases in the information treatments. The latter is difficult to reconcile with a Bayesian updating mechanism. Furthermore, there is no evidence that the different policy preferences in the information treatments can be related to whether respondents over- or under-estimated the number of deaths or average income losses relative to the information they were given. We explore an alternative possible psychological mechanism whereby COVID-19 death and income loss information plays into the anxiety of those who are initially most anxious about COVID-19, with the heightened sense of anxiety causing the change in policy preference. There is some evidence consistent with this mechanism. In this context, it is perhaps not so surprising that the within-subject and between-subject tests of these information effects coming from the second wave suggest that they are not enduring. The latter also echoes (to some degree) the findings by [Barrera et al. \(2020\)](#), who show that the policy conclusions citizens draw are largely invariable in the face of factual information.

The paper proceeds as follows. In the next section, we describe the design of our conjoint survey experiment and information treatments. In Section 3, we present our results, including their policy implications. In Section 4, we discuss the possible underlying mechanisms, while in Section 5, we assess the endurance of these effects. We conclude in Section 6.

2 Experimental design

2.1 Control and treatment groups

Our main survey experiment was undertaken online between April 17 and 21, 2020. We recruited 2,245 US respondents via Prolific Academic (full details below), who received a payment based on the time it took to complete the survey.³ 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?

The respondents answered the first two questions by supplying a number, and 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: T1 provided information about *projected* COVID-19 deaths – the estimates that we used were retrieved by the Washington-based Institute for Health Metrics and Evaluation (IHME); T2 provided information regarding the expected output loss of the US economy in 2020, compared to 2019 – the figures were retrieved from the IMF.⁴ Thus, respondents in T1 and T2 received information that they might use, respectively, to update their answers to questions (i) and (ii), respectively. We provide more details on the information treatments and our instrument in Appendix A.

³ 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.

⁴ At the time of our survey, the IHME and IMF estimates constituted the most informed projections regarding the evolution of the pandemic's death and economic toll and all major media outlets, in the US and elsewhere, relied heavily on them.

2.2 The conjoint experiment

Our respondents then engaged with the conjoint part of the experiment. Conjoint analysis has been used to study citizens' policy 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). Their key advantage over other preference elicitation techniques lies in their ability to measure the impact of treatments with multiple dimensions (Hainmueller et al. 2014). Yet, conjoint experiments are not only useful for eliciting preferences over multi-dimensional policies: even when there are few policy elements (and preferences could be elicited using simple survey questions), the conjoint method is superior whenever preferences over policy dimensions are co-determined or co-dependent. In such cases, an ordinary 'elicitation' via simple (i.e., sequential) survey questions cannot identify the causal effect of each element. That is, the estimation of the effect of an individual policy attribute would need to assume a value for the remaining attributes since such values are not experimentally elicited in the survey process. Put simply, we cannot control for policy preferences on the remaining $n - 1$ dimensions when we ask respondents to reveal their preference over the n -th dimension. Conjoint survey experiments, by contrast, do include such controls and are, therefore, the ideal method for eliciting *ceteris paribus* preferences on individual policy elements when a policy has at least two dimensions.

We first introduced our respondents to the post-pandemic fiscal adjustment debate using the following text:⁵

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

⁵ Our 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 a 1-7 Likert scale to elicit respondents' preferences allows those individuals who completely reject the need for any fiscal adjustment plan (now or in the future) to simply *assign the same lowest score* of (1) to all packages that are presented. Indeed, this is another reason why we prefer to use the Likert scale when presenting our main findings in Section 3.

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. Table 1 presents our three attributes and the (three to eight) values they can take. First, we analyze whether older people, the wealthy, or companies should shoulder the burden as compared with everyone sharing the burden in proportion to their income. Second, we assess whether an 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 (broad) policy areas that may be affected by any spending cuts, including health, social security, defense, and foreign aid.⁶ The first and third attributes relate to the redistributive character of citizen preferences in relation to fiscal policy. For instance, a preference for the wealthy to 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, with a preference for tax increases as opposed to spending cuts representing a preference for a larger role for the state as compared with the market. We focus in this way on people’s redistributive and state/market preferences because policies are typically distinguished in this way. Moreover, 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 attribute values or policy elements were chosen to reflect the alternatives discussed in actual policy debates across advanced economies because this should facilitate respondents’ understanding of the options (Gallego and Marx 2017). For instance, in April 2020, when we

⁶ Including three attributes is well within the limit of what can be included without losing response quality (Bansak et al. 2021a).

Table 1: Policy attributes and attribute values in the conjoint experiment

Attribute	Attribute values (treatment elements)
Who bears the financial burden?	<ul style="list-style-type: none"> – People contribute in proportion to their income (R) – 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 (R) – Mainly cutting spending, but with some tax increases
Cuts on for any spending cuts	<ul style="list-style-type: none"> – Roads and public transport – Health care (R) – Social security – Pensions – Defense – Foreign aid – Environmental policy – Research and development

Note: The R indicates which attribute value serves as the reference in the analysis.

fielded our survey, the role of multinational companies in post-pandemic fiscal adjustment was discussed (e.g., [Turner 2020](#)). Likewise, broader reform of fiscal policy was being called for, with the editors of the [Financial Times \(2020\)](#) arguing that “[p]olicies until recently considered eccentric, such as basic income and wealth taxes, will have to be in the mix.” The elements on ‘policy areas for any spending cuts’ were chosen because they resemble those included in questions about federal budget spending in the American National Election Study (ANES), and 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 conjoint experiment, we presented each respondent with four pairs of alternatives, each with elements randomly assigned to each attribute. We also randomized the attribute order (between subjects).⁷ Respondents were asked to perform two tasks when presented with a pair of alternatives, and their answers are our outcome variables. First, for each package, they were asked to rate how much they would support the implementation of each policy package on a Likert scale from 1 to 7 (the rating outcome variable).⁸ Second, respondents were asked to choose the package they would prefer (choice outcome variable), coded as 1 for the preferred package and 0 for the rejected one. Appendix Figure A3 displays an example pair of packages, along with the two outcome variables.

To identify the causal effects of the fiscal adjustment attribute values (policy elements) on the likelihood that they are preferred, we leveraged the difference in attributes between distinct policy package preferences. We estimated average marginal component effects (AM-CEs) 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 choice). As respondents were asked to express their preferences in relation to four pairs of policy packages, we clustered the standard errors by respondent i . We also used quota-based and (manually targeted) entropy balancing weights in our preferred specifications to ensure that our estimates are representative of the US general (adult) population. In additional specifications, we

⁷ The order of the attributes is also randomized, but respondents always observe the full range of attributes, while each time, *only one element* from the full list of attribute elements is *randomly assigned to each of the attributes*, with respondents observing two 'packages' of values at any one time.

⁸ The distribution of this variable is presented in Appendix Figure A4. 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 outcome variable 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 Appendix C.

used US state fixed effects as a robustness check (Appendix Table C2). In our experiment, respondents were asked to choose between $j = 2$ alternative policy packages in each of their $k = 4$ choice tasks. Our experiment randomly generated a total of 18,116 policy profiles.

Interpreting the average marginal component and marginal means effects

The average marginal component effects (AMCEs) represent subjects' elicited relative preference (as compared to the base category) for each of the individual attributes, while holding constant – that is, at their mean – their preferences on the other dimensions (a *ceteris paribus* argument). For example, a preference for placing the burden on the wealthy is expressed relative to distributing it proportionally among all members of society, *while holding constant* subjects' preferences on the fiscal policy instrument to be used (i.e., spending cuts, tax increases, or an equal mix). This is another reason to use a conjoint experiment: it allows us to elicit multi-dimensional preferences when dimensions are co-dependent and thus correlated. The implication is that an AMCE preference for, for instance, reliance on tax increases (as compared to a balanced mix of cuts and taxes) is not incompatible with a preference for allocating the burden proportionally among all as compared to placing it mostly on the rich. This is appropriate because taxes can be imposed on all in proportion to their income; this would still imply that the wealthier Americans pay more in absolute terms.

Similarly, the estimation of the marginal means (MMs) to gauge levels of policy support (Subsection 3.4) is an exercise in analyzing *conditional preferences*. It allows us to identify which combination of policy attributes can command majority support among the electorate, conditional on the policy having a certain attribute (e.g., relying on tax increases as the key fiscal adjustment tool), and with other attributes being evaluated at their respective means. This is another advantage of the conjoint elicitation as we would not have been able to calculate these estimates based on three standard, successive survey questions; that is, the latter would have made it impossible to *identify experimentally* the winning policy by varying one attribute at a time while holding other elements at their means.

2.3 Information treatments

Both the COVID-19 death estimates from the Institute for Health Metrics and Evaluation (IHME 2020a) and the IMF income loss estimates (IMF 2020) were published in the week preceding our fieldwork.⁹ 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:

$$Y_{ijk} = a_0 + \gamma_k + \delta I * T_{ij} + v_{ijk} \quad (2)$$

where I indicates 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. We made similar decisions as before regarding clustering, the use of weights, and fixed effects in our preferred specifications. To check that the randomization worked, we conducted two balance tests, which are presented in Tables A1 and A2. Table A1 reports the balance of the observable characteristics of respondents across treatment and control groups; Table A2 shows the balance of the conjoint policy elements across the groups. We did not detect any major imbalances in covariates or policy dimensions across treatments and control that were randomly shown to respondents.

2.4 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

⁹ We now know that the numbers were hugely underestimated for the US, which leads us to expect that the information effects would have been (still) stronger had the estimates been closer to the actual number.

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.

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 whether there is partisan reality polarization in our April sample on the first three perceptions of COVID-19 questions and then on whether there is partisan policy preference polarization in the control. Thereafter, we compare the information treatments with the control. We turn later in Section 5 to the within and between-subjects comparisons that are made available by the October wave to test for enduring effects of information provision on partisan policy depolarization.

3.1 Reality polarization in COVID-19 perceptions

Table 2 reports mean values for the three perceptions of reality questions. We compare perceptions across Trump and Clinton voters, and across Democrats and Republicans. The Democrats and Clinton voters have estimates of COVID-19 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 seri-

¹¹ There were 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).

ous than seasonal flu. But there are no differences in income loss estimates. Result 1 then follows:

RESULT 1 (in support of some partisan ‘reality polarization’): Partisan reality polarization exists over the perceived health costs of COVID-19 and its seriousness relative to the flu.

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	-

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. Standard deviations are in parenthesis. *** p<0.01, ** p<0.05 , * p<0.1.

3.2 Conjoint analysis: Policy polarization in the control group

In this subsection, 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). 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).¹²

To test for the existence of partisan policy polarization, we analyze whether the estimated AMCEs for Republicans and Democrats are statistically different from one another. This happens in the first three columns of Tables 3 and 4, where we present the estimated AMCE coefficients for these two groups (columns 1 and 2) and check whether they differ in a statistical sense (column 3).¹³ This allows us to see whether the two groups have statistically distinguishable preferences from one another for each policy element. A first inspection of column 3 (in both Tables 3 and 4), reveals that they do across all three attributes.

Two significant differences are revealed with respect to the first attribute of ‘who carries the burden’. 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. Note that this revealed preference on ‘who should carry the burden’ *holds constant* respondents’ preferences over the other two dimensions (they are evaluated at mean support). That is, whatever their preferred policy mix is (e.g., mostly spending cuts for Republicans, or a balanced mix between cuts and taxes for Democrats), they want to foot the bill on that particular group *ceteris paribus*.¹⁴

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).¹⁵ Finally, with respect to where spending cuts

¹² 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 Appendix Table C1.

¹³ The full results from the corresponding regressions are presented in Appendix Table C4. We present similar results for Trump and Clinton voters in Appendix Table C5.

¹⁴ Also note that the coefficients of the first attribute for Democrats are not only statistically different from the respective ones for Republicans (see Table 3, column 3) but they are also statistically different from zero (see column 1 for the estimated confidence intervals.)

¹⁵ Again notice that the coefficient for Democrats is not only statistically different from that for Republicans but also from zero (see CIs in column 1, Table 3).

should fall (the third attribute), Democrats have a significantly stronger preference than Republicans for these cuts to fall on defense and pensions.

Looking at Table 4, where we contrast the fiscal preferences of Trump and Clinton 2016 voters, a very similar pattern emerges across all three attributes. Again a significant difference in who should carry the burden, as well as in their preferred policy tool, is revealed (see column 3 for the relevant t-tests). 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) prefer those policy features that are more redistributive and that attribute a bigger role to the state (e.g., aversion to spending cuts).

Nonetheless, these differences have not prevented some aggregate preferences from emerging. Unsurprisingly, 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 US (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, see Tables 3-4, columns 1 and 2, line 7; and statistically indistinguishable from one another (see Tables 3-4, column 3, line 7).

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

Policy elements	Dem.	Rep.	Difference	Dem.	Rep.	Difference	Dem.	Rep.	Difference
	Control			Treatment 1			Treatment 2		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Burden on									
Companies	0.07 [0.04; 0.10]	0.02 [-0.03; 0.06]	0.05** (2.29)	-0.00 [-0.04; 0.04]	-0.01 [-0.08; 0.05]	0.01 (0.37)	-0.02 [-0.06; 0.02]	0.01 [-0.05; 0.08]	-0.03 (-1.18)
Older people	-0.14 [-0.17; -0.11]	-0.15 [-0.20; -0.11]	0.01 (0.54)	0.02 [-0.02; 0.06]	0.02 [-0.04; 0.10]	-0.00 (-0.13)	-0.00 [-0.04; 0.04]	0.07 [0.00; 0.15]	-0.07* (-1.85)
Wealthy	0.10 [0.07; 0.13]	0.02 [-0.02; 0.06]	0.08*** (3.46)	0.01 [-0.03; 0.05]	0.01 [-0.05; 0.07]	-0.00 (-0.16)	0.00 [-0.04; 0.04]	0.05 [-0.01; 0.12]	-0.05 (-1.30)
Policy tool									
Mostly spending cuts	-0.02 [-0.05; -0.00]	0.03 [-0.02; 0.06]	-0.05** (-2.12)	0.02 [-0.01; 0.05]	0.02 [-0.08; 0.03]	0.04 (1.36)	0.02 [-0.01; 0.05]	-0.01 [-0.06; 0.04]	0.03 (1.02)
Mostly tax increases	-0.03 [-0.05; -0.00]	-0.05 [-0.09; -0.02]	0.02 (1.31)	0.02 [-0.01; 0.05]	0.02 [-0.03; 0.07]	0.00 (-0.02)	0.03 [-0.00; 0.06]	0.03 [-0.02; 0.08]	0.00 (0.00)
Cuts on									
Defense	0.23 [0.19; 0.28]	0.05 [-0.02; 0.11]	0.18*** (4.82)	-0.07 [-0.13; -0.01]	-0.10 [-0.20; 0.01]	0.03 (0.39)	-0.05 [-0.11; 0.00]	-0.03 [-0.11; 0.06]	0.02 (-0.47)
Environment	0.07 [0.03; 0.11]	0.09 [0.02; 0.16]	-0.02 (-0.47)	-0.03 [-0.08; 0.03]	-0.08 [-0.18; 0.01]	0.05 (0.95)	-0.06 [-0.11; 0.00]	-0.05 [-0.14; 0.05]	-0.01 (-0.18)
Foreign aid	0.18 [0.14; 0.22]	0.16 [0.09; 0.23]	0.02 (0.54)	-0.06 [-0.12; 0.00]	-0.12 [-0.22; -0.02]	0.06 (1.00)	-0.03 [-0.09; 0.03]	-0.04 [-0.14; 0.05]	0.01 (0.24)
R&D	0.11 [0.07; 0.15]	0.11 [0.06; 0.17]	0.00 (-0.17)	-0.02 [-0.08; 0.04]	-0.10 [-0.18; -0.01]	0.08 (1.49)	-0.03 [-0.08; 0.02]	-0.07 [-0.14; 0.01]	0.04 (0.83)
Pensions	0.07 [0.03; 0.11]	0.00 [-0.06; 0.05]	0.07** (1.99)	-0.05 [-0.11; 0.01]	-0.02 [-0.11; 0.08]	-0.03 (-0.54)	-0.02 [-0.08; 0.04]	-0.06 [-0.14; 0.03]	0.04 (0.74)
Social security	0.01 [-0.03; 0.05]	-0.06 [-0.13; 0.01]	0.07* (1.72)	-0.02 [-0.08; 0.04]	-0.06 [-0.16; 0.04]	0.04 (0.67)	-0.04 [-0.10; 0.02]	-0.02 [-0.12; 0.08]	-0.02 (0.29)
Public transport	0.13 [0.09; 0.17]	0.08 [0.03; 0.14]	0.05 (1.24)	-0.05 [-0.11; 0.01]	-0.08 [-0.17; 0.01]	0.03 (0.57)	-0.05 [-0.10; 0.01]	-0.02 [-0.10; 0.07]	-0.03 (-0.60)

Notes: Columns 1-2, 4-5 and 7-8 show estimated AMCEs for each policy element for Democrats (columns 1,4,7) and Republicans (columns 2,5,8). Confidence intervals are presented below in parenthesis. Columns 3, 6 and 9 display the differences in the coefficient estimates (with t-statistics reported below in parenthesis); asterisks indicating significant differences in coefficients between two groups from a Wald test of significance. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The reference policy elements 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. Appendix Table C4 displays the underlying regression results.

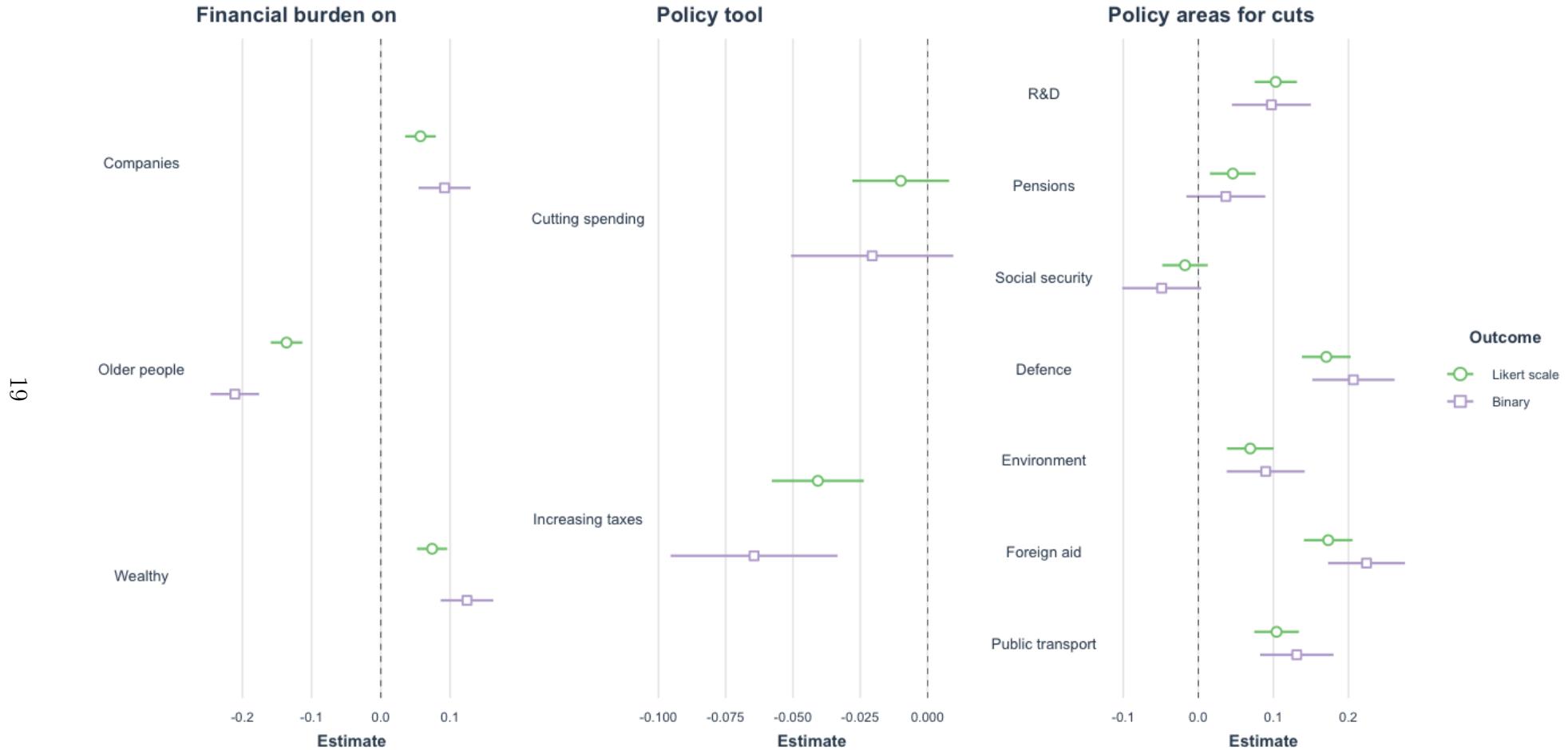
Table 4: Polarization and informational (de)polarization between Clinton and Trump voters (2016)

Policy elements	Clinton	Trump	Difference	Clinton	Trump	Difference	Clinton	Trump	Difference
	Control			Treatment 1			Treatment 2		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Burden on									
Companies	0.08 [0.05; 0.11]	0.02 [-0.02; 0.07]	0.06** (2.06)	-0.01 [-0.06; 0.04]	-0.01 [-0.08; 0.06]	0.00 (0.08)	-0.03 [-0.07; 0.01]	-0.01 [-0.08; 0.06]	-0.02 (-0.50)
Older people	-0.15 [-0.18; -0.11]	-0.15 [-0.21; -0.10]	0.00 0.26	0.04 [-0.01; 0.09]	0.04 [-0.04; 0.11]	0.00 (0.11)	0.02 [-0.03; 0.06]	0.02 [-0.06; 0.10]	0.00 (0.00)
Wealthy	0.10 [0.07; 0.14]	0.02 [-0.03; 0.07]	0.08*** (2.88)	0.02 [-0.03; 0.06]	0.03 [-0.04; 0.10]	-0.01 (-0.44)	0.01 [-0.04; 0.05]	0.02 [-0.05; 0.09]	-0.01 (-0.40)
Policy tool									
Mostly spending cuts	-0.02 [-0.04; 0.01]	0.04 [-0.01; 0.08]	-0.06** (-2.14)	0.00 [-0.04; 0.04]	-0.03 [-0.09; 0.03]	0.03 (0.91)	0.01 [-0.02; 0.05]	-0.01 [-0.07; 0.05]	0.02 (0.70)
Mostly tax increases	-0.03 [-0.05; -0.00]	-0.03 [-0.07; -0.00]	0.00 (0.38)	0.03 [-0.01; 0.06]	0.00 [-0.05; 0.05]	-0.03 (0.83)	0.02 [-0.02; 0.05]	0.01 [-0.04; 0.07]	0.01 (0.16)
Cuts on									
Defense	0.22 [0.17; 0.27]	0.05 [-0.02; 0.13]	0.17*** (3.74)	-0.05 [-0.12; 0.02]	-0.07 [-0.12; 0.02]	0.02 (0.20)	-0.03 [-0.10; 0.03]	-0.01 [-0.11; 0.08]	-0.02 (-0.34)
Environment	0.04 [-0.01; 0.09]	0.10 [0.02; 0.18]	-0.06 (-1.36)	0.01 [-0.06; 0.07]	-0.10 [-0.21; 0.00]	0.11 (1.93)	-0.02 [-0.09; 0.05]	-0.06 [-0.16; 0.04]	0.04 (0.62)
Foreign aid	0.15 [0.10; 0.20]	0.16 [0.09; 0.24]	-0.01 (-0.25)	-0.03 [-0.10; 0.04]	-0.10 [-0.21; 0.01]	0.07 (1.26)	-0.03 [-0.09; 0.04]	-0.01 [-0.10; 0.09]	-0.02 (-0.38)
R&D	0.10 [0.06; 0.15]	0.12 [0.06; 0.19]	-0.02 (-0.50)	-0.01 [-0.08; 0.05]	-0.07 [-0.17; 0.02]	0.06 (1.18)	-0.01 [-0.07; 0.06]	-0.09 [-0.18; -0.00]	0.08 (1.82)
Pensions	0.05 [0.00; 0.10]	0.01 [-0.05; 0.08]	0.03 (0.92)	-0.04 [-0.11; 0.02]	-0.03 [-0.13; 0.08]	-0.01 (-0.30)	-0.03 [-0.09; 0.03]	-0.05 [-0.14; 0.03]	0.025 (0.48)
Social security	-0.04 [-0.08; 0.01]	-0.05 [-0.13; 0.03]	0.012 (0.27)	0.01 [-0.06; 0.07]	-0.04 [-0.14; 0.07]	-0.05 (0.89)	-0.02 [-0.08; 0.05]	-0.04 [-0.14; 0.06]	0.02 (0.41)
Public transport	0.11 [0.06; 0.15]	0.07 [0.00; 0.14]	0.03 (0.79)	-0.03 [-0.09; 0.03]	-0.03 [-0.13; 0.06]	0.00 (0.08)	-0.03 [-0.09; 0.03]	-0.00 [-0.10; 0.09]	-0.03 (-0.50)

Notes: Table shows estimated AMCEs for each policy element for Clinton voters (columns 1,5,9) and Trump voters (columns 2,6,10). Confidence intervals are presented below in parenthesis. Columns 4, 8, and 12 display the t-statistics indicating significant differences in coefficients between two groups from a Wald test of significance. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

The reference policy elements 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 C5 in the Appendix displays the underlying regression results.

Figure 1: Aggregate 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 policy elements 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 C1 displays the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

This bi-partisan agreement on fiscal adjustments that oppose relying 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 1 (see also Table C1), which also include independents and non-affiliated voters.¹⁶ This documented aversion to reliance on tax increases (relative to a balanced policy mix of both taxes and cuts), 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 (see Tables D1 and D2, for similar t-tests for fiscal preference questions in the 2016 American National Election Study (ANES) by partisanship, income, and age). Result 2 follows:

RESULT 2 (in support of ‘partisan policy polarization’): There is partisan polarization over the COVID-19 fiscal adjustment preferences with respect to (a) who should carry the burden of fiscal adjustment (i.e., the wealthy and companies), (b) the policy tool (i.e., over greater reliance on spending cuts), and (c) where cuts should fall (i.e., pensions and defense).

This COVID-19 polarization reproduces the normal patterns of partisan fiscal policy preference polarization (e.g., [Bartels 2008](#); [Franko et al. 2013](#); [Ballard-Rosa et al. 2017](#)).

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; that is, we contrast the differences in column 3 with the estimates presented in columns 6 and 9 (in Tables 3 and 4). The most striking result is that almost all of

¹⁶ 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. Section A provides more detail to this method. Table C3 reports regression results with partisanship-adjusted weights.

the partisan policy polarization disappears: nothing is significant at the 5% level in columns 6 and 9 in Tables 3 and 4. In other words, information appears to depolarize the ‘business as usual’ partisan policy differences we found in the control group.

In particular, we reproduce the control group analysis of AMCEs for the information treatment 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, we check whether statistically significant differences between the estimated AMCEs (for Democrats and Republicans) persist when information is provided (Treatments 1 and 2). In Tables 3 and 4 we present those results in columns 4-6 (Treatment 1) and 7-9 (Treatment 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, those partisan differences are no longer statistically different in the two treatment groups (see columns 6 and 9 in Tables 3-4). In other words, the polarized preferences (along partisan lines) that we identified in the control group now depolarize with the provision of factual information; in practice, not a single difference survives. This is true for both treatments.

It is worth noting 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 shift 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 shifted toward 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.

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.

It is apparent from Tables 3 (and Table C4) and 4 (and Table C5) that the information not only had a partisan depolarizing 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 2 and Table C1).

With respect to the first attribute of ‘who shoulders the fiscal burden’, there was a clear aggregate preference for companies and the wealthy to do so, relative to people sharing the burden *in proportion to their income*; the preferences disappears in both information treatments. 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 it. 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. Under the information treatments, there is no such hierarchy as all spending areas are equally preferred for cuts relative to health care. What is also notable is that in the control group there was a clear ordering in terms of where cuts should fall. By contrast, under both information treatments, there is no such hierarchy as all spending areas are equally preferred for cuts relative to the baseline. This is another testament to the depolarizing effect that information provision had because the clear ordering that we found in the control was mostly driven by partisan differences.

The wholesale switch, however, from disliking a predominant reliance on tax increases to actually preferring such reliance cannot be understood as depolarization. This is a wholesale movement in everyone’s policy preferences. We show this in Tables 5 and 6, where we look at informational reversals not between but within groups this time; that is, comparing

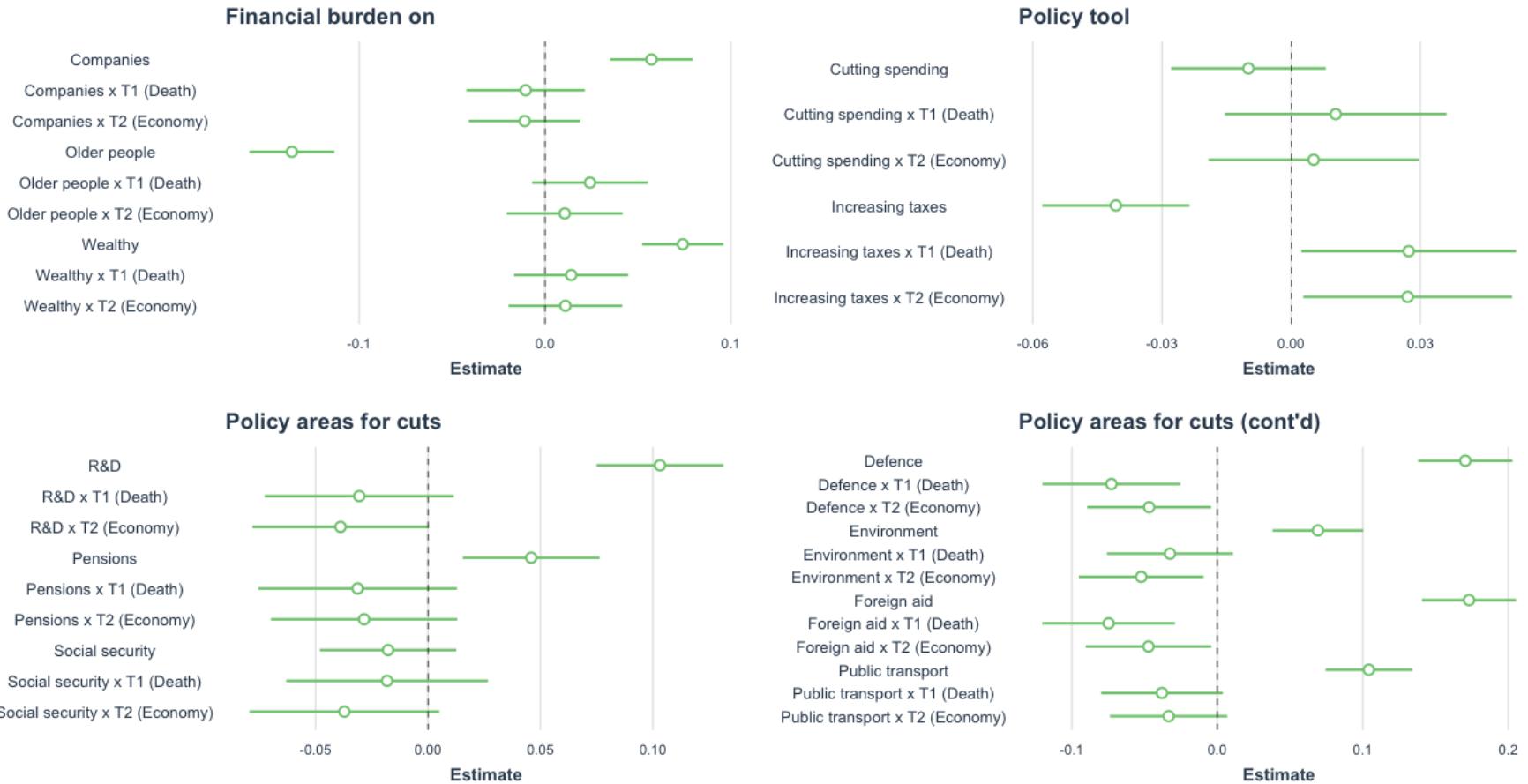
coefficients of control and treatments within Democrats and Republicans.¹⁷ The switch is all the more remarkable because fiscal policy preferences in ‘normal times’ tend to be what we found in the control group: both Democrats and Republicans expressed a clear aversion to tax hikes in the control group (both coefficients were negative and significantly different from zero, see line 7 in columns 1 and 2, in both Tables 3 and 4); this is now reversed as both groups support/do not oppose tax increases (the difference of coefficients between treatments and the control for both groups (within group comparison) is statistically significant indicating a shift in preferences; see Tables 5 and 6, line 7 in columns 4 and 5). In this sense, information provision not only depolarizes but also resets the normal presumption against tax increases that have been a long-standing feature of US politics.

Note that this fiscal policy preference reset does not contradict our previous finding on the depolarizing effects of information (Result 3) due to the *relative nature* of elicited preferences. That is, the aggregate preference for relying mostly on taxes for fiscal adjustment purposes is not incompatible with the fact that now there is no relative preference for placing a *disproportionately higher* burden on the wealthy (or companies). This is because the baseline attribute (all contribute proportionally to their income) would still have the rich paying more (in absolute terms) when additional taxes are levied.¹⁸

¹⁷ See also Tables B1 and B2 for Clinton and Trump voters, respectively.

¹⁸ In fact, in the case of the US, even the baseline preference of ‘everyone should carry a fiscal burden proportional to their income’, which is *progressive*, already represents a *relative worsening of terms* for wealthy Americans and corporations compared to the current status quo.

Figure 2: 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 policy elements 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 C1 displays the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

Table 5: Informational reversals among Democrats

	Estimates			Differences in estimates	
	Control	T1	T2	Control-T1	Control-T2
	(1)	(2)	(3)	(4)	(5)
Burden on					
Companies	0.07	-0.00	-0.02	0.07**	0.09***
Older people	-0.14	0.02	-0.00	-0.16***	-0.14***
Wealthy	0.10	0.01	0.00	0.09***	0.10***
Policy tool					
Mostly spending cuts	-0.03	0.02	0.02	-0.05*	-0.05*
Mostly tax increases	-0.03	0.02	0.03	-0.05*	-0.06**
Cuts on					
Defense	0.23	-0.07	-0.05	0.30***	0.28***
Environment	0.07	-0.03	-0.06	0.10**	0.13***
Foreign aid	0.18	-0.06	-0.03	0.24***	0.21***
R&D	0.11	-0.02	-0.03	0.13***	0.14***
Pension	0.07	-0.05	-0.02	0.12**	0.09*
Social security	0.01	-0.02	-0.04	0.03	0.05
Public transport	0.13	-0.05	-0.05	0.18***	0.18***

Notes: Columns 1-3 report estimated AMCEs for each policy element within Democrats across control and treatment groups (reproducing the results already presented in columns 1, 4, and 7 in Table 3; CI's omitted for brevity but are otherwise identical). Columns 4 and 5 present the differences in estimates (within Democrats) between the two treatments and the control. Asterisks indicate significant differences in coefficients between two groups from a Wald test of significance. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The reference policy elements 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. Appendix Table C4 displays the underlying regression results.

This cross-partisan reset in favor of taxation-based fiscal adjustment is strong enough to be borne out in the aggregate results (see again Figure 2). In fact, it is among the very few clear fiscal policy preferences that emerge in the aggregate post-information treatment. Thus our fourth result follows:

RESULT 4 (Post-pandemic fiscal policy reset): COVID-19-related information provision shifts citizens' preferences, independent of partisan affiliation, from opposing the predominant use of tax increases in fiscal consolidation to supporting it.

Table 6: Informational reversals among Republicans

	Estimates			Differences in estimates	
	Control	T1	T2	Control-T1	Control-T2
	(1)	(2)	(3)	(4)	(5)
Burden on					
Companies	0.02	-0.01	0.01	0.03	0.01
Older people	-0.15	0.03	0.08	-0.18***	-0.22***
Wealthy	0.02	0.01	0.05	0.01	-0.03
Policy tool					
Mostly spending cuts	0.02	-0.02	-0.01	0.04	0.03
Mostly tax increases	-0.05	0.02	0.03	-0.07*	-0.08**
Cuts on					
Defense	0.05	-0.10	-0.03	0.15*	0.08
Environment	0.09	-0.08	-0.05	0.17**	0.14*
Foreign aid	0.16	-0.12	-0.04	0.28***	0.20***
R&D	0.11	-0.10	-0.07	0.21***	0.18***
Pension	-0.00	-0.02	-0.06	0.02	0.06
Social security	-0.06	-0.06	-0.02	0.00	-0.04
Public transport	0.08	-0.08	-0.02	0.16**	0.10

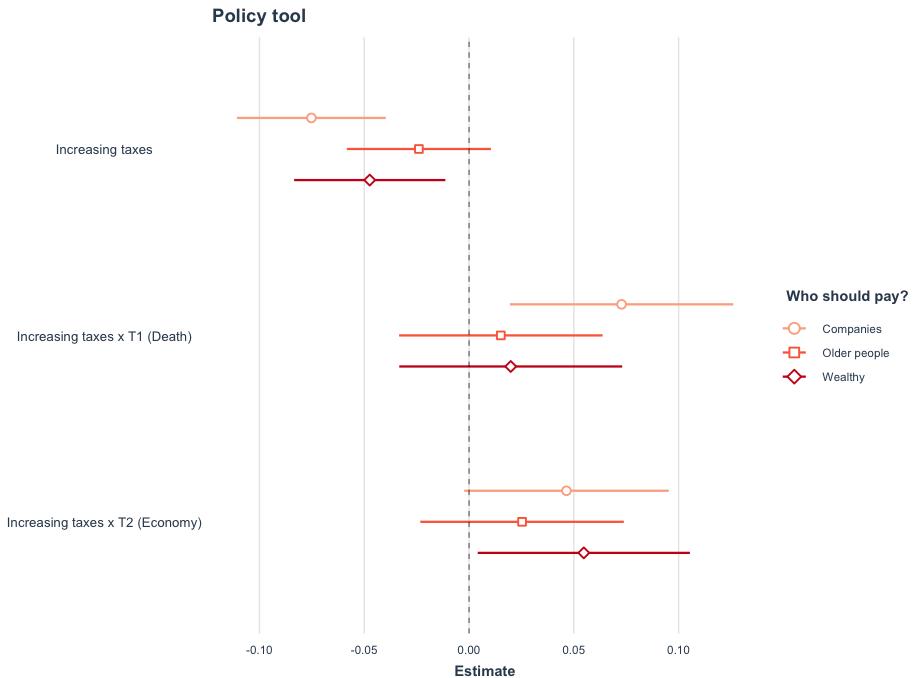
Notes: Columns 1-3 report estimated AMCEs for each policy element within Republicans across control and treatment groups (reproducing the results already presented in columns 2, 5, and 8 in Table 3; CI's omitted for brevity but are otherwise identical). Columns 4 and 5 present the differences in estimates (within Republicans) between the two treatments and the control. Asterisks indicate significant differences in coefficients between two groups from a Wald test of significance. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The reference policy elements 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. Appendix Table C4 displays the underlying regression results.

3.4 Policy implications

While the AMCEs are useful for the identification of causal effects on preferences over multi-dimensional policies – thus helping us detect significant depolarization (and the possibility of a fiscal policy reset) – 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 a shift in preferences towards tax increases, but does this mean a policy of tax increases would

actually win an election? In other words, is a post-pandemic fiscal ‘New Deal’ politically feasible?

Figure 3: 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 C7 displays the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

The next set of exercises helps address this question by providing a key substantive interpretation of our findings. First, in Figure 3, we condition a tax-based fiscal adjustment policy on the ‘who should pay?’ question (see also Table C7). 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).¹⁹

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 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 (combinations of policy elements).

Figure 4 presents estimates of popular support over a series of plans (see also Appendix Tables E1, E2 and E3). This is again a conditional exercise: given the preferences we have identified (*ceteris paribus*) in favor of tax-based fiscal adjustment policies, which particular policy package is *the most popular* among them? This is done in the first panel of Figure 4, where 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 panels, we show estimates of policy packages based on spending cuts and an equal mix of taxes and spending cuts, respectively.

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 E1, E2 and E3. Considering all possible fiscal consolidation packages, the most favorable ones appear to be those that rely on increasing taxes (post-treatment); and among them, those putting the burden either on wealthy Americans or on

¹⁹ Note that, as we have shown in Figure 2, *any* tax-based fiscal adjustment is preferred irrespective of who will shoulder the burden. Our point here is a conditional one: among those in the majority who support policies that rely mostly on raising taxes, whom do they wish to see carrying the burden relative to the baseline of ‘everyone paying in proportion to their incomes’ – which notably is still *progressive* in a sense.

companies, with an *average of 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 4). 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%.

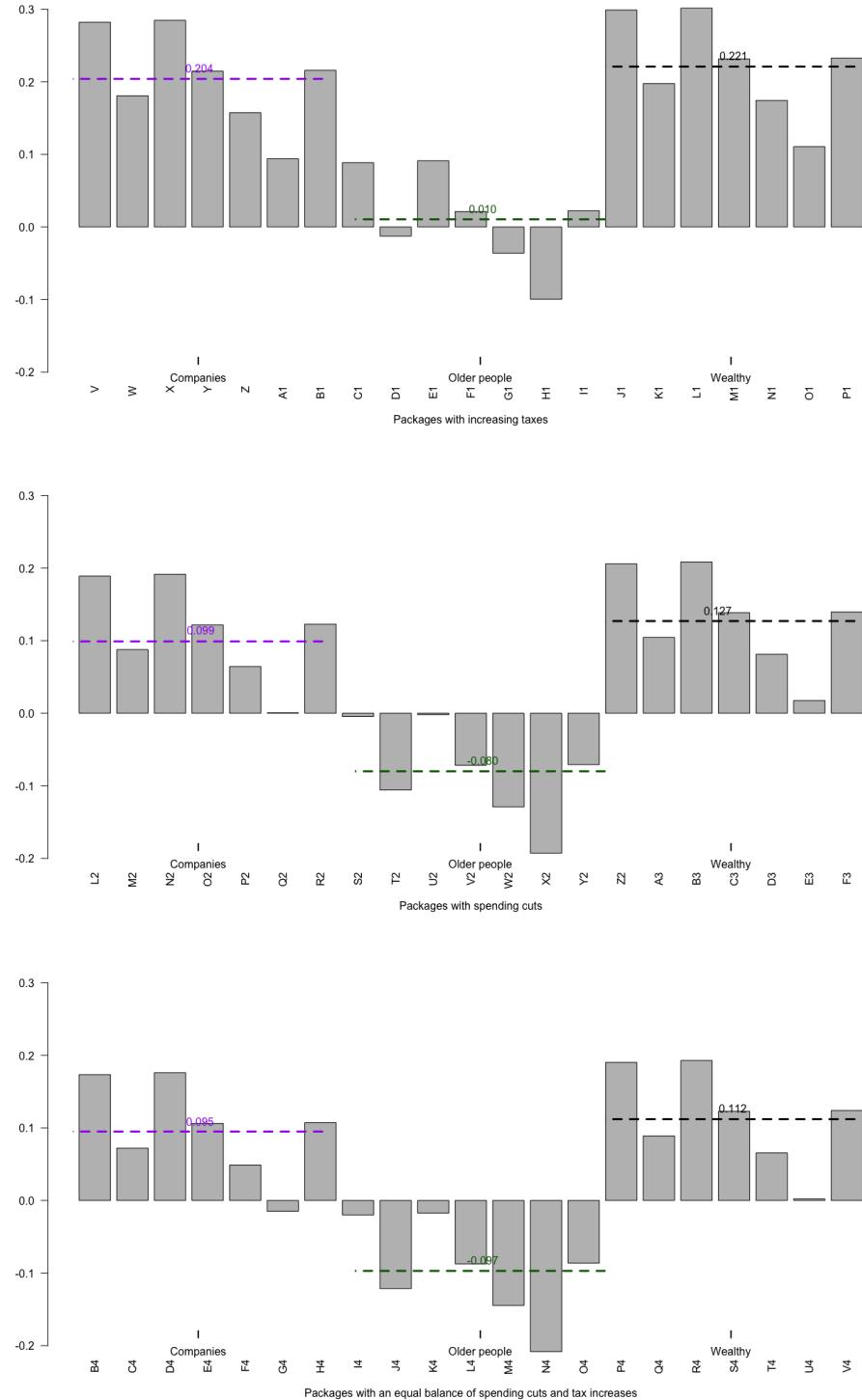
To see how much of this 20 percentage point margin is due to tax increases and how much is due to the burden falling on companies or the wealthy, we now compare this policy with a status quo where again there is a balance of tax and increases and spending cuts, but this time the burden falls on either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

Again in Figure 4, 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 policy elements). Hence, tax-based plans would carry a 55-45% majority of the American public, *ceteris paribus*.²⁰

In sum, our findings suggest not only that information on COVID-19 produces a policy preference reset, but this reset also creates a politically feasible outcome with tax increases, in the sense that such a policy package with tax increases could command an electoral majority. Yet, what mechanism drives the policy preference reset, and how durable is it?

²⁰ In 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.

Figure 4: Support for fiscal adjustment policy packages (marginal means)



Notes: Three panels demonstrate policy combinations with increasing taxes (first panel: packages V to P1), spending cuts (second panel: packages L2 to F3) and a balance between the two (third panel: packages B4 to V4). Each bar demonstrates linear estimates for a particular policy package as presented in Appendix Tables E1, E2 and E3 – 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 areas when policy areas for cuts are averaged across.

4 Possible information mechanisms

4.1 Bayesian updating

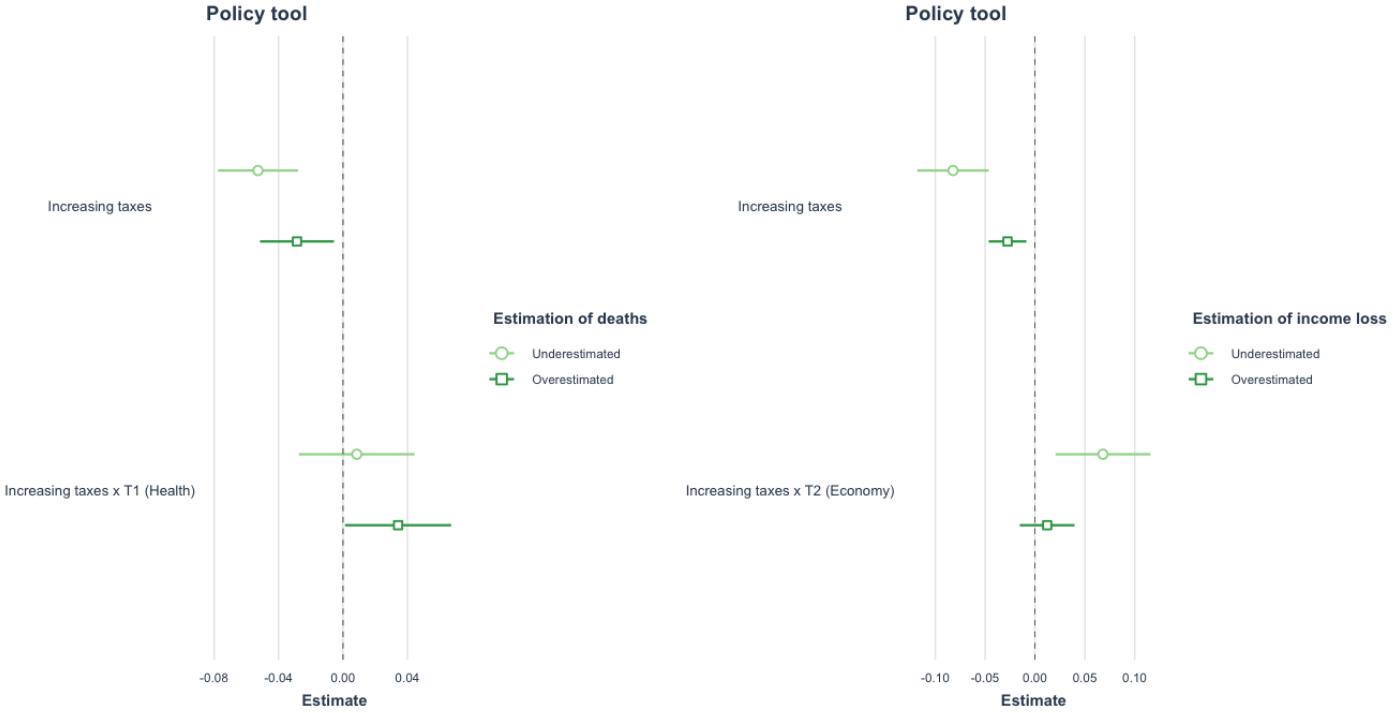
One possible explanation for the apparent depolarizing effect of information in our experiment follows Friedman’s (1953) conjecture. Our respondents in the information treatments were given information and therefore had reason to update their priors regarding the costs of COVID-19. In so far as differences in priors in the control contributed to the party polarized policy preferences observed in the control, we would expect that differences in belief about COVID-19 seriousness will narrow through updating in the information treatment and so will policy preferences.

This is, of course, what we find with our de-polarization result. But we check on this mechanism in more detail by examining whether the information treatment has different effects on the policy preferences of those who initially held priors for these costs below our information numbers as compared with those who initially held higher prior expectations regarding these costs.²¹ The initial ‘under-estimators’ and ‘over-estimators’ should adjust differently to our information if they are Bayesians. There is no evidence that this is the case. Figure 5 below shows that those who underestimated income loss seem to be moving more than those who overestimated in the income loss treatment.

It is less clear why any form of updating would in principle cause the other preference change that we observe: the shift by both Democrat and Republican voters towards preferring an increase in taxes. Nevertheless, there is no evidence that such updating underpinned the change because, again, as Figure 5 reports, the under-, and over-estimators do not seem to have responded differently on the matter of increasing taxes to the provision of the information.

²¹ More specifically, prior to treatment, respondents were asked to answer questions regarding their likely estimates of COVID-19 deaths and income loss given the lockdown in April. A respondent’s estimate is categorized as correct if it falls within a range of +/- 5,000 deaths or +/- 1% income loss, respectively, relative to the IHME and IMF estimates. An estimate below the specified range is categorized as an underestimate, and an estimate above the specified range is categorized as an overestimate.

Figure 5: Estimation of deaths and income loss 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. The bars represent 95% confidence intervals; standard errors are clustered by respondent. Tables C8 and C9 display the underlying regression results.

4.2 Psychological mechanism

In psychology experiments, information is often used to prime ‘mood’ or ‘affect’. These changes in a person’s emotional state, in turn, are thought to influence their decision-making (see, for a review, [Forgas 2002](#)). There are a variety of possible ways in which mood might affect decision making but it seems there is one that we can discount. In so far as the information that we provide is compared by our subjects with their priors, some will receive ‘good’ news from our information prime because they had previously overestimated the costs, while others will receive ‘bad’ news because they had underestimated the costs. Since good and bad news typically have different effects in the psychological mood literature, we would again expect that, if this was the mechanism, then the mood effect on decision making in

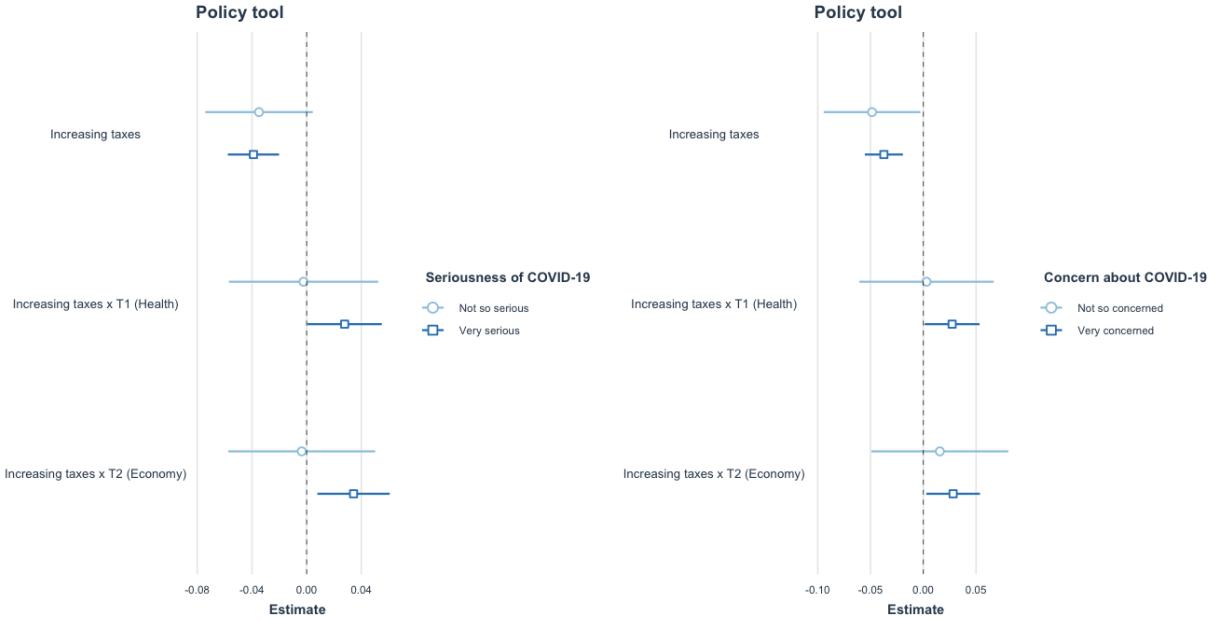
the information treatments would vary depending on whether the respondent was an over- or under-estimator, along the same lines as we would expect from a Bayesian updater. This is not what we found above.

Respondents may, however, be influenced by our information in a different way. They may not compare the information with their priors. Instead, it is possible that the information makes the fact of COVID-19 more salient to all our respondents; and, because COVID-19 is a ‘bad’ event, this is experienced as ‘bad’ news causing anxiety. Bad news inducing anxiety could either directly induce preference change and/or it can produce a cognitive change (i.e., a change in the way that people process information).

The direct psychological mechanisms may explain why there was a wholesale shift in preferences towards increasing taxes. There is evidence, for instance, that people had already become more anxious as a result of COVID-19 (Fetzer et al. 2020; Binder 2020). So, if our information prime heightened this sense of anxiety, then we might expect that those who were already highly anxious would be those who were most likely to be affected by a bad news prime. The highly anxious were, in effect, more receptive to the information prime. There is some evidence to support this view.

First, with respect to the wholesale shift in preferences towards tax increases. In Figure 6, we begin by capturing respondent anxiety by how they answer our question regarding their perception of the seriousness of COVID-19 compared to flu. We then use our respondents’ responses to how concerned they are (for themselves and their families) about COVID-19. The results show that the shift in tax preferences is exclusively driven by those who think COVID-19 is more serious and are relatively more concerned about it.

Figure 6: Information effects on variables of health anxiety



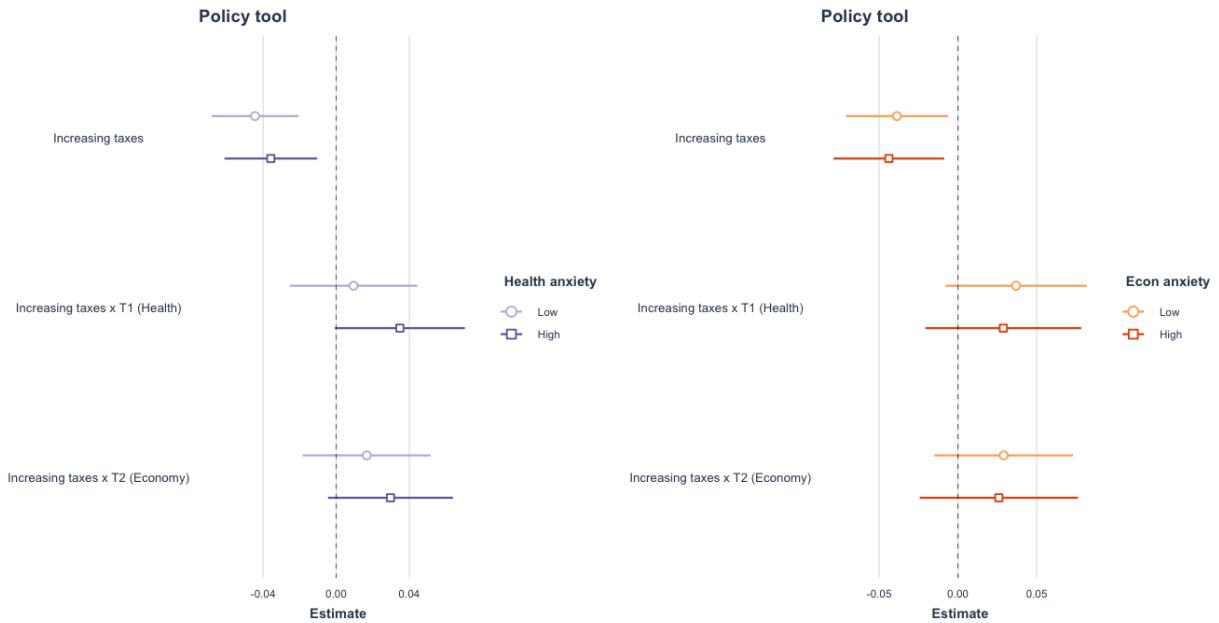
Notes: The plot shows estimates of the effect of randomly assigned values of fiscal adjustment attributes on the probability of a policy package to be preferred. Tables C10 and C11 display the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

Next, using polychoric principal component analysis (PCA), we construct an index of health anxiety.²² We also construct an index of economic vulnerability.²³ We then divide the sample by the variables' median values to create subgroups of people with low and high levels of health anxiety and low and high levels of economic vulnerability. Figure 7 presents control and information treatment effects for those four subgroups. The results indicate a clear switch from disliking taxes to favoring them for those people with higher levels of health-related anxiety. However, we do not see such an effect on those people who feel more vulnerable financially.

²² Our analysis determined that we should use the following variables: (i) how serious respondents think COVID-19 is compared to flu, (ii) how concerned they are over their own and family's health, (iii) how healthy they have felt in the last week and finally (iv) whether they see themselves as belonging to the high-risk group regarding COVID-19.

²³ Similarly, economic vulnerability is constructed using polychoric PCA with the following elements: (i) employment, (ii) self-employment, (iii) earnings change during the pandemic and (iv) general income.

Figure 7: Information effects on health anxiety and economic vulnerability



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. Tables C12 and C13 display the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

This asymmetry between health anxiety and economic vulnerability may seem somewhat surprising, but it fits with what else is known about the way bad news directly affects preferences. There is, for example, some complementary evidence from experiments in psychology that specifically link anxiety and stress to pro-social changes in behavior (see [Meredith et al. 2009](#); [von Dawans et al. 2012](#)). The pro-social behavioral response is typically understood as a mechanism for coping with stress in this literature (see [Midlarsky 1991](#); [Taylor et al. 2000](#); [Raposa et al. 2016](#)), and there is some evidence that the oxytocin generated by pro-social behavior helps regulate stress (see [Heinrichs and Domes 2008](#); [Preston 2013](#)). Thus, in so far as the shift to increasing taxes is regarded as a pro-social change in behavior, then these experiments on the effect of stress on pro-sociality are consistent with our results linking those with high levels of anxiety to the shift in preferences to increasing taxes.

There is also growing literature on how bad events are associated with preference changes in economics that are partially consistent with what we have found. 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 ([Gualtieri et al. 2019](#)). The latter may be due to disasters being perceived as bad luck ([Cappelen et al. 2013](#)), but equally, these natural shocks plausibly increased anxiety among the population.

Against this, there is evidence, though, that specifically economic shocks are associated with more selfish responses (e.g., [Margalit 2013](#); [Fisman et al. 2015](#); [Colantone and Stanig 2018](#)), with decreased support for redistributive policies ([Brunner et al. 2011](#)), and with moves to the right of the political spectrum ([Funke et al. 2016](#)). This contrary evidence on economic shocks is, however, consistent with what we have found earlier about the way that, seemingly, the anxiety effects of our information treatments worked through health anxiety and not economic vulnerability. Had heightened economic vulnerability, instead, been a mechanism, we would have expected, on the basis of this evidence on the effect of economic shocks on selfishness, that the boost to economic vulnerability would count against the pro-social preference change for tax increases. Nevertheless, this leaves a puzzle as to why the COVID-19 information prime did not appear to heighten economic vulnerability. It is possible that government economic policy responses of cash benefits in one form or another served to make the economic aspects of COVID-19 play a minor role in the anxiety that it generated when contrasted with the health ones.

We turn now to the second possible, cognitive mechanism that is triggered by ‘bad’ news inducing anxiety and consider how this might explain the other change in policy preferences that we observe in the information treatments: the depolarizing convergence in policy preferences between Democrats and Republicans. In particular, there are experiments where changes in mood lead people to adopt different reasoning styles (see, for a discussion, [Forgas 2002](#)). Good news, in this literature, is apparently typically processed in a top-down manner,

whereas bad news prompts a bottom-up process of reasoning. The key difference between these styles concerns whether the information prompts a detailed and serious engagement with the evidence (bottom-up) or a quick and less demanding response (top-down). The difference has been likened to Piaget's distinct schemas for processing information (see, e.g., [Fiedler and Bless 2013](#)): in one such schema ('assimilative'), information is assimilated within an existing framework (top-down), in the other ('accommodation'), the information prompts a change in the framework itself (bottom-up). There is also evidence that the media and public opinion respond in a similar asymmetric fashion to good and bad news (see [Soroka 2006](#)).

From this cognitive perspective, our bad news prime would have triggered the kind of bottom-up detailed reasoning style associated with a serious engagement with the facts of COVID-19. Instead of assimilating the facts to a pre-existing (top-down) way of thinking about fiscal policy issues, like those of political affiliation, the bad news encouraged our respondents to set such predispositions aside to some degree and think about the fiscal problems afresh (from the 'bottom-up'). Without the in-built polarity of those political affiliations to guide thinking, policy preferences became less polarized. This is, of course, a version of the Friedman prediction, but the mechanism is rather different from that envisaged by most economists and would only hold for the bad news.

To conclude, therefore, we find some evidence that is consistent with a psychological underpinning to our information treatment results regarding the convergence in policy preferences and the wholesale shift to increasing taxes. On this account, the COVID-19 information prime rendered the pandemic more salient and so primed anxiety and bottom-up ways of thinking. With the former, we could expect that this would encourage pro-sociality, especially when the COVID-19 information primes health anxiety. This, in turn, would be consistent with a wholesale policy preference change towards the use of increasing taxes. With the latter, bottom-up thinking is less guided by prior frameworks than top-down style and so may make our subjects more open to influence by the actual evidence on COVID.

When people are exposed to the same evidence and become more likely to be influenced by that evidence, it is not surprising that their policy preferences become more convergent.

Two things are worth noting by way of a conclusion to this discussion of the possible psychological mechanisms that might underpin our results. One is that the putative psychological influence of mood on reasoning style provides some support for a Friedman-like convergence of policy preferences – but only in the case of bad news. The other is that these mood effects on behavior are typically thought to be short-lived in psychology. Are they here too? This is the final question to which we now turn. Did the depolarization and shift to a preference for increasing taxes persist in our October wave?

5 October wave: Do the depolarizing effects endure?

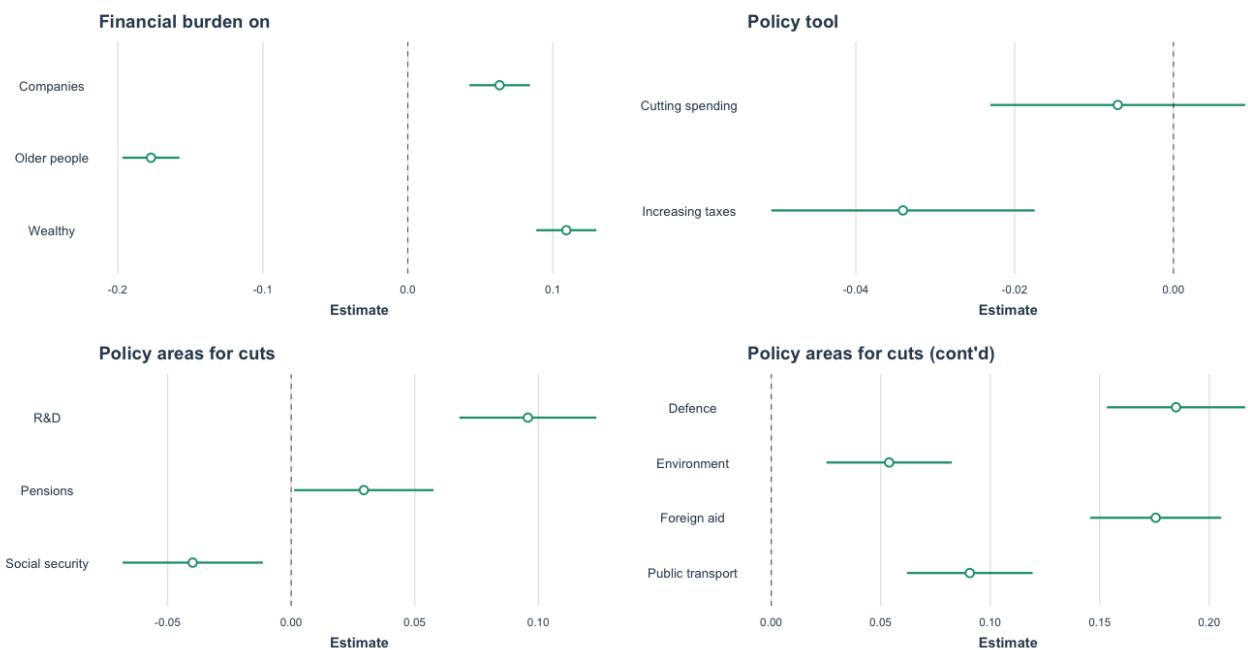
In the main wave of our survey, conducted in April 2020, we compared the policy preferences of respondents in the treatment arms with those of respondents in the control group to analyze the impact of information provision. In a similar vein, we now use the conjoint survey experiment in the second wave of our survey – conducted between October 28 and November 3, 2020 – to assess the endurance of the information effects. More specifically, we first compare the policy preferences of respondents in the treatment arms in April to their preferences in October (within-subject), and then we compare the preferences expressed in October by, respectively, respondents who were treated and not treated in April (between-subject).

Figure 8 presents the within-subject comparison to understand the persistence of the information effects over time. In this figure, we display the October preferences of respondents who were in one of the treatment groups in the April wave. As the figure shows, the depolarizing effect of information seems to have faded away in October. Importantly, the preference for increasing taxes that we observed in the treated groups in April does not persist in October. In the aggregate, there is a 6 percentage points difference between the

April and October preferences of the same respondents for the policy element on increasing taxes. Respondents who were treated with information in April and supported increasing taxes by 3 percentage points disliked increasing taxes by 3 percentage points in October.

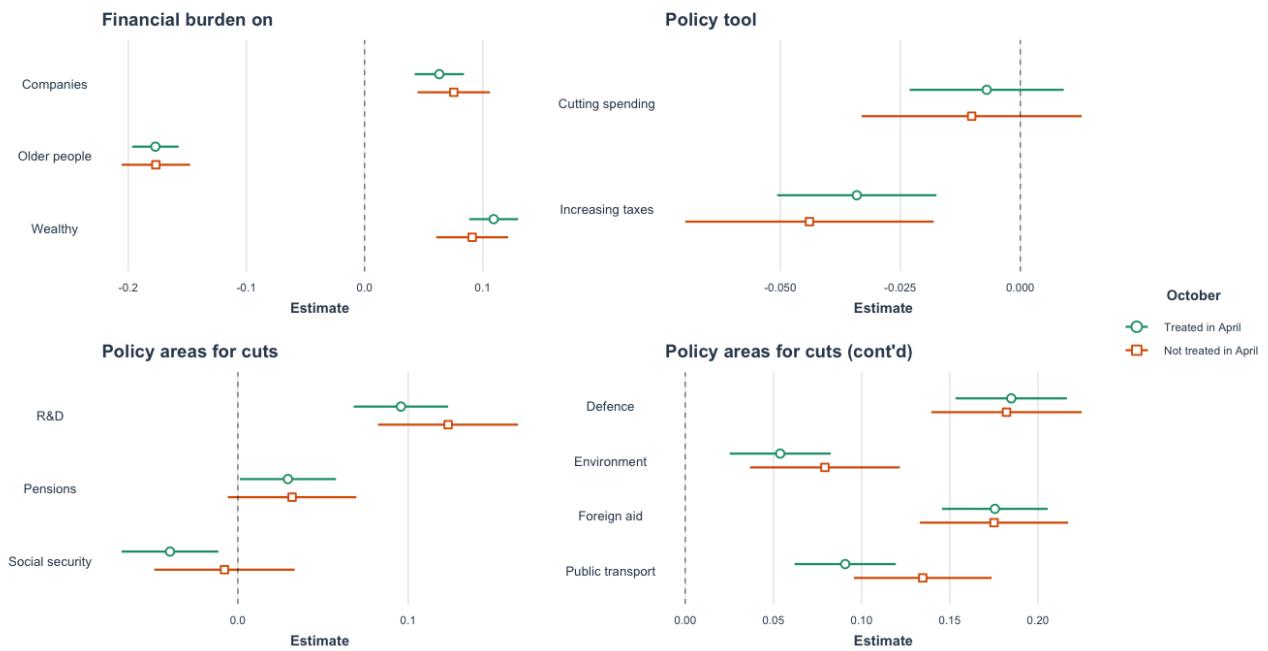
The second test can be found in Figure 9. In this figure, we compare the October policy preferences of respondents who were treated and not treated with information in April. We do not find any statistically significant differences between April-treated and non-treated respondents in the October wave, thus raising doubt about any enduring effect of the information treatments. Finally, in Table 7, we also check the polarization between Democrats and Republicans in the October wave (among those who were in the treatment arms). The results suggest that the informational depolarization between Democrats and Republicans in 'who pays aspect of tax policy' in April disappeared in the October wave.

Figure 8: Aggregate information treatment effects on the conjoint experiment, within-subject wave comparison (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 policy elements 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 F1 displays the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

Figure 9: Aggregate information treatment effects on the conjoint experiment, between subjects, October wave (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 policy elements 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 F2 displays the underlying regression results. The bars represent 95% confidence intervals; standard errors are clustered by respondent.

Table 7: Polarization between Democrats and Republicans, October wave

Policy Elements	Dem.	Rep.	Difference
	October wave		
	(1)	(2)	(3)
Burden on			
Companies	0.10* [0.07; 0.13]	0.01 [-0.03; 0.06]	0.09*** (3.35)
Older people	-0.19* [-0.22; -0.16]	-0.16* [-0.21; -0.11]	0.03 (-1.11)
Wealthy	0.13* [0.10; 0.16]	0.05* [0.01; 0.10]	0.08** (2.95)
Policy tool			
Mostly spending cuts	-0.01 [-0.03; 0.01]	0.01 [-0.03; 0.04]	0.02 (-0.98)
Mostly tax increases	-0.02 [-0.04; 0.00]	-0.07* [-0.10; -0.03]	0.05** (2.37)
Cuts on			
Defense	0.24* [0.20; 0.29]	0.06 [-0.01; 0.13]	0.18*** (4.45)
Environment	0.07* [0.04; 0.11]	0.06 [-0.01; 0.13]	0.1 (0.24)
Foreign aid	0.21* [0.17; 0.25]	0.12* [0.06; 0.18]	0.09** (2.39)
R&D	0.11* [0.07; 0.15]	0.10* [0.04; 0.15]	0.01 0.28
Pensions	0.07* [0.03; 0.10]	-0.02 [-0.08; 0.04]	0.09** (2.50)
Social security	-0.04 [-0.07; 0.00]	-0.06 [-0.12; 0.00]	0.02 (0.61)
Public transport	0.12* [0.08; 0.16]	0.04 [-0.02; 0.10]	0.08** (2.17)

Notes: Confidence intervals are presented below in parenthesis (with t-statistics reported below in parenthesis); asterisks indicating significant differences in coefficients between two groups from a Wald test of significance. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The reference policy elements 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. Appendix Table F3 displays the underlying regression results.

6 Conclusions

In this study, we have aimed to shed light on the impact of information about the world on partisan policy polarization. We embedded two pieces of information – about the health and economic implications of the COVID-19 pandemic – in a conjoint survey experiment on post-pandemic fiscal adjustment in the United States, an issue that not only quickly became salient during the pandemic, but is also traditionally associated with partisan polarization. At the height of the first wave of the pandemic, we observed partisan polarization on both the reality of COVID-19 and fiscal adjustment preferences. Yet, importantly, the partisan policy differences disappeared when respondents were exposed to information about COVID-19 deaths and income losses. Furthermore, the analysis showed a cross-party reset in preferences for the use of taxation as a tool to adjust, from opposition to endorsement.

Yet, exploring the underlying mechanism, we found little evidence of Bayesian updating. Instead, our analysis suggests that information about the (severe) health and economic implications of the pandemic affected the mood of respondents and triggered anxiety. These changes in the affective state of respondents are, in turn, associated with a short-term change in preferences. Further support for such a psychological mechanism was provided by the comparison of conjoint policy preferences in the first and second wave of our survey (May and October 2020, respectively), which showed that the reset of preferences had not endured.

These findings have two sets of implications. First, they provide insight into the question of why so-called ‘policy windows’ (Kingdon 1984) emerge and are politically exploited, in periods of crisis. Our study suggests that the available information about the implications of a crisis may impact the affective state of people, which in turn changes policy preferences, and moves them away from their ‘normal state’. Thus, it may be a psychological mechanism – in citizens and/or politicians – that facilitates the opening up of windows for policy change. Yet, our analysis also suggests that these windows may be small, with preferences and polarization rather quickly returning to old levels. Hence, there may be time limits to policy change in times of crisis.

Second, our findings provide only limited support for Friedman's optimism about the impact of policy-relevant information about the world. While information seems capable of eliminating (partisan) differences temporarily, this appears to be less a function of learning and updating, and more the product of psychology. Ultimately, 'fundamental differences in basic values' may re-emerge, and there may be little that positive economics can do.

The lack of an enduring information effect has one more important implication: it raises questions about the use of information treatments in the laboratory or survey experiments. If the effects we find may be driven by short-term psychological mechanisms rather than by learning and updating, the conclusions we draw about (the endurance of) our results may need to be formulated more carefully.

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A Data 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.

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 experiment

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?

Financial Package A:

—

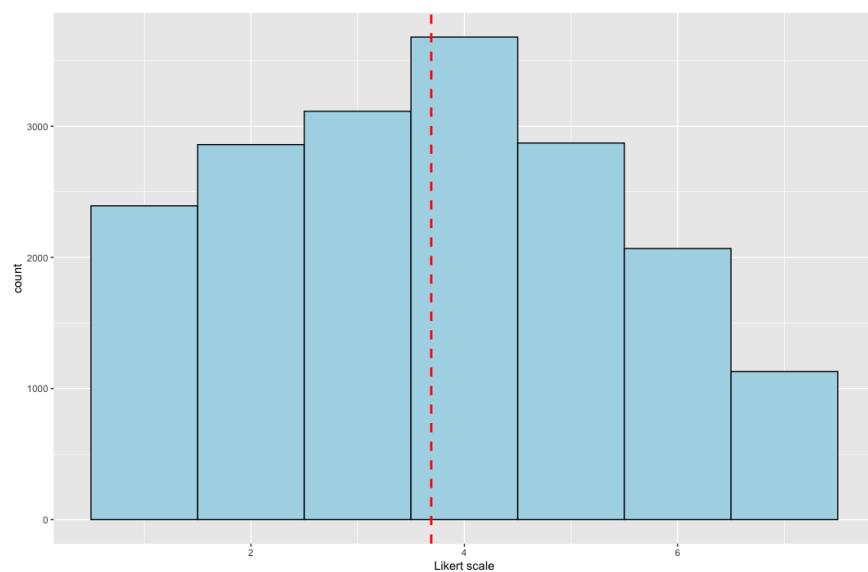
Financial Package B:

○

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

Financial Package A	○	Financial Package B	○
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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.

Our two information treatments 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 to the coronavirus by August 4. This means that the number of COVID-19 deaths per one million people would be 210.²⁴*

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 5.9% equates to a loss of around \$3,848 per person in 2020 compared with 2019.*

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. Table [C3](#) show regression results of aggregate preferences with partisanship-adjusted weights.

Tables [A1](#) and [A2](#) tests balance across covariates and policy elements, respectively.

²⁴ *Ex post*, this number proved to be an under-estimate of the actual death toll of almost 200,000 people. But at the time of the survey, this was indeed the best available estimate. Moreover, no one –including our subjects– expected such a high toll. For instance, in our survey, almost 40% of our respondents provided an estimate smaller than 70,000, while only 15% of them got actually close (by 10k or so) to the actual number; the median estimate of COVID-19 deaths provided was slightly below 100,000. As a result, there was an equal number of subjects who provided a lower/higher estimate than the one provided by IHME. Or, in other words, our subjects were not remarkably more accurate regarding their predictions.

Table A1: Balance of treatment and control across covariates

	Control	T1	T2
Republican	0.00 [-0.02; 0.02]	0.01 [-0.00; 0.03]	-0.02 [-0.03; 0.00]
Independent	-0.01 [-0.02; 0.01]	0.01 [-0.01; 0.03]	-0.01 [-0.03; 0.01]
25-34	-0.00 [-0.03; 0.02]	-0.06* [-0.08; -0.03]	0.06* [0.04; 0.09]
35-44	-0.00 [-0.03; 0.02]	-0.02 [-0.04; 0.00]	0.02 [-0.00; 0.05]
45-64	-0.02 [-0.04; 0.01]	-0.02* [-0.05; -0.00]	0.04* [0.02; 0.06]
65+	0.03* [0.00; 0.06]	-0.05* [-0.08; -0.02]	0.02 [-0.01; 0.04]
20-60k	0.02 [-0.00; 0.03]	0.02* [0.01; 0.04]	-0.04* [-0.06; -0.02]
60-100k	-0.03* [-0.05; -0.01]	0.08* [0.06; 0.10]	-0.05* [-0.07; -0.03]
100k+	0.01 [-0.01; 0.03]	0.01 [-0.01; 0.03]	-0.02 [-0.04; 0.00]
R ²	0.00	0.01	0.00
Adj. R ²	0.00	0.00	0.00
Observations	18, 128	18, 128	18, 128
RMSE	0.47	0.47	0.47

* Null hypothesis value outside the confidence interval.

Table A2: Balance of treatment and control across policy elements

	Control	T1	T2
Companies	0.02 [-0.00; 0.04]	-0.01 [-0.03; 0.01]	-0.01 [-0.03; 0.01]
Older people	0.01 [-0.01; 0.03]	-0.00 [-0.02; 0.02]	-0.01 [-0.02; 0.01]
Wealthy	0.02* [0.00; 0.04]	0.00 [-0.02; 0.02]	-0.03* [-0.05; -0.01]
Cutting spending	-0.00 [-0.02; 0.01]	0.00 [-0.02; 0.02]	0.00 [-0.01; 0.02]
Increasing taxes	-0.01 [-0.03; 0.00]	0.01 [-0.01; 0.03]	0.00 [-0.01; 0.02]
Defense	0.01 [-0.02; 0.04]	-0.01 [-0.04; 0.02]	0.00 [-0.02; 0.03]
Environment	-0.00 [-0.03; 0.02]	0.02 [-0.01; 0.05]	-0.02 [-0.04; 0.01]
Foreign aid	0.01 [-0.02; 0.03]	-0.02 [-0.04; 0.01]	0.01 [-0.02; 0.04]
R&D	0.01 [-0.02; 0.04]	-0.01 [-0.03; 0.02]	-0.00 [-0.03; 0.03]
Pensions	0.02 [-0.01; 0.05]	-0.02 [-0.04; 0.01]	-0.01 [-0.03; 0.02]
Social security	-0.01 [-0.03; 0.02]	0.00 [-0.03; 0.03]	0.01 [-0.02; 0.03]
Public transport	0.00 [-0.02; 0.03]	-0.00 [-0.03; 0.03]	-0.00 [-0.03; 0.03]
R ²	0.00	0.00	0.00
Adj. R ²	0.00	0.00	0.00
Observations	18136	18136	18136
RMSE	0.47	0.47	0.47

* Null hypothesis value outside the confidence interval.

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 among Democrats and Republicans. Tables B1 and B2 show the same, but now among Clinton and Trump voters, respectively.

Table B1: Informational reversals among Clinton voters (2016)

	Estimates			Differences in estimates		p-value	
	Control	T1	T2	Control-T1	Control-T2	Control-T1	Control-T2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fiscal burden on							
Companies	0.082	-0.009	-0.030	0.091	0.112	0.0138**	0.0018***
Older people	-0.145	0.040	0.017	-0.185	-0.162	<0.001***	<0.001***
Wealthy	0.102	0.015	0.006	0.087	0.096	0.018**	0.008***
Policy tool							
Spending cuts	-0.018	0.001	0.012	-0.019	-0.03	0.509	0.286
Tax increases	-0.025	0.026	0.017	-0.051	-0.042	0.075*	0.116
Policy cuts							
Defense	0.221	-0.051	-0.031	0.272	0.252	<0.001***	<0.001***
Environment	0.039	0.005	-0.021	0.034	0.06	0.506	0.246
Foreign aid	0.152	-0.026	-0.028	0.178	0.18	0.001***	<0.001***
R&D	0.104	-0.011	-0.005	0.115	0.109	0.020*	0.024**
Pension	0.050	-0.043	-0.029	0.093	0.079	0.086*	0.135
Social security	-0.038	0.005	-0.017	-0.043	-0.021	0.409	0.687
Public transport	0.106	-0.030	-0.027	0.136	0.133	0.008***	0.007***

Notes: Table shows estimated AMCEs for each policy element within Clinton voters across control and treatment groups. Asterisks indicate significant differences in coefficients between two groups from a Wald test of significance. *** p<0.01, ** p<0.05, * p<0.1. The reference policy elements 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 C5 in the Appendix displays the underlying regression results.

Table B2: Informational reversals within Trump 2016 voters

	Estimates			Differences in estimates		p-value	
	Control	T1	T2	Control-T1	Control-T2	Control-T1	Control-T2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fiscal burden on							
Companies	0.024	-0.012	-0.011	0.036	0.035	0.480	0.481
Older people	-0.154	0.036	0.017	-0.19	-0.171	0.003***	0.007***
Wealthy	0.019	0.032	0.022	-0.013	-0.003	0.819	0.966
Policy tool							
Spending cuts	0.035	-0.028	-0.009	0.063	0.044	0.183	0.343
Tax increases	-0.034	0.000	0.012	-0.034	-0.046	0.403	0.259
Policy cuts							
Defense	0.053	-0.065	-0.013	0.118	0.066	0.175	0.396
Environment	0.102	-0.102	-0.057	0.204	0.159	0.019**	0.055*
Foreign aid	0.163	-0.100	-0.006	0.263	0.169	0.002***	0.038**
R&D	0.124	-0.074	-0.091	0.198	0.215	0.008***	0.002***
Pension	0.012	-0.026	-0.054	0.038	0.066	0.614	0.351
Social security	-0.050	-0.036	-0.041	-0.014	-0.009	0.867	0.913
Public transport	0.073	-0.034	-0.002	0.107	0.071	0.165	0.307

Notes: Table shows estimated AMCEs for each policy element within Trump voters across control and treatment groups. Asterisks indicate significant differences in coefficients between two groups from a Wald test of significance. *** p<0.01, ** p<0.05, * p<0.1. The reference policy elements 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 C5 in the Appendix displays the underlying regression results.

C Regression tables

- C.1 Aggregate results (Likert scale and binary outcome)
- C.2 Aggregate results with state fixed effects
- C.3 Aggregate results with partisanship-adjusted entropy weights
- C.4 Heterogeneous effects with party
- C.5 Heterogeneous effects with presidential vote
- C.6 Heterogeneous effects with value of life
- C.7 Heterogeneous effects with aggregate preferences for the al-location of tax burden (Conditioning on “Who should pay?”)
- C.8 Heterogeneous effects with the estimation of deaths
- C.9 Heterogeneous effects with the estimation of income loss
- C.10 Heterogeneous effects with seriousness of the pandemic
- C.11 Heterogeneous effects with concern about COVID-19
- C.12 Heterogeneous effects with health anxiety
- C.13 Heterogeneous effects with economic vulnerability

Table C1: Aggregate results

	Likert scale	Binary
(Intercept)	0.38* [0.35; 0.41]	0.43* [0.38; 0.48]
Companies	0.06* [0.04; 0.08]	0.09* [0.05; 0.13]
Companies x T1	-0.01 [-0.04; 0.02]	-0.01 [-0.06; 0.05]
Companies x T2	-0.01 [-0.04; 0.02]	0.01 [-0.04; 0.07]
Older people	-0.14* [-0.16; -0.11]	-0.21* [-0.25; -0.18]
Older people x T1	0.02 [-0.01; 0.06]	0.00 [-0.05; 0.05]
Older people x T2	0.01 [-0.02; 0.04]	0.02 [-0.03; 0.07]
Wealthy	0.07* [0.05; 0.10]	0.12* [0.09; 0.16]
Wealthy x T1	0.01 [-0.02; 0.04]	0.04 [-0.02; 0.09]
Wealthy x T2	0.01 [-0.02; 0.04]	0.01 [-0.04; 0.06]
Cutting spending	-0.01 [-0.03; 0.01]	-0.02 [-0.05; 0.01]
Cutting spending x T1	0.01 [-0.02; 0.04]	0.02 [-0.03; 0.06]
Cutting spending x T2	0.01 [-0.02; 0.03]	0.00 [-0.04; 0.04]
Increasing taxes	-0.04* [-0.06; -0.02]	-0.06* [-0.10; -0.03]
Increasing taxes x T1	0.03* [0.00; 0.05]	0.03 [-0.02; 0.07]
Increasing taxes x T2	0.03* [0.00; 0.05]	0.01 [-0.03; 0.05]
Defense	0.17* [0.14; 0.20]	0.21* [0.15; 0.26]
Defense x T1	-0.07* [-0.12; -0.03]	-0.02 [-0.09; 0.06]
Defense x T2	-0.05* [-0.09; -0.00]	-0.02 [-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]
R&D	0.10*	0.10*
	[0.07; 0.13]	[0.04; 0.15]
R&D x T1	-0.03	0.03
	[-0.07; 0.01]	[-0.04; 0.11]
R&D 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 C2: Aggregate results with state fixed effects

	Likert scale	Binary
Companies	0.06* [0.04; 0.08]	0.09* [0.06; 0.13]
Companies x T1	-0.01 [-0.04; 0.02]	-0.01 [-0.06; 0.05]
Companies x T2	-0.01 [-0.04; 0.02]	0.01 [-0.04; 0.06]
Older people	-0.14* [-0.16; -0.11]	-0.21* [-0.25; -0.18]
Older people x T1	0.02 [-0.01; 0.06]	0.00 [-0.05; 0.05]
Older people x T2	0.01 [-0.02; 0.04]	0.02 [-0.03; 0.07]
Wealthy	0.08* [0.05; 0.10]	0.13* [0.09; 0.16]
Wealthy x T1	0.01 [-0.02; 0.04]	0.04 [-0.02; 0.09]
Wealthy x T2	0.01 [-0.02; 0.04]	0.01 [-0.04; 0.06]
Cutting spending	-0.01 [-0.03; 0.01]	-0.02 [-0.05; 0.01]
Cutting spending x T1	0.01 [-0.02; 0.04]	0.02 [-0.02; 0.06]
Cutting spending x T2	0.01 [-0.02; 0.03]	0.00 [-0.04; 0.04]
Increasing taxes	-0.04* [-0.06; -0.02]	-0.06* [-0.10; -0.03]
Increasing taxes x T1	0.03* [0.00; 0.05]	0.03 [-0.02; 0.07]
Increasing taxes x T2	0.03* [0.00; 0.05]	0.01 [-0.03; 0.05]
Defense	0.17* [0.14; 0.20]	0.21* [0.15; 0.26]
Defense x T1	-0.07* [-0.12; -0.03]	-0.02 [-0.10; 0.06]
Defense x T2	-0.05* [-0.09; -0.00]	-0.02 [-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]
R&D	0.10*	0.10*
	[0.08; 0.13]	[0.05; 0.15]
R&D x T1	-0.03	0.03
	[-0.07; 0.01]	[-0.04; 0.11]
R&D 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 C3: Aggregate results with partisanship-adjusted entropy weights

	Quota weight	Partisanship adjusted weight
(Intercept)	0.38* [0.35; 0.41]	0.40* [0.36; 0.43]
Companies	0.06* [0.04; 0.08]	0.04* [0.02; 0.07]
Companies x T1	-0.01 [-0.04; 0.02]	-0.01 [-0.04; 0.03]
Companies x T2	-0.01 [-0.04; 0.02]	-0.00 [-0.04; 0.03]
Older people	-0.14* [-0.16; -0.11]	-0.14* [-0.17; -0.12]
Older people x T1	0.02 [-0.01; 0.06]	0.03 [-0.01; 0.06]
Older people x T2	0.01 [-0.02; 0.04]	0.03 [-0.01; 0.07]
Wealthy	0.07* [0.05; 0.10]	0.06* [0.04; 0.08]
Wealthy x T1	0.01 [-0.02; 0.04]	0.01 [-0.02; 0.05]
Wealthy x T2	0.01 [-0.02; 0.04]	0.02 [-0.01; 0.06]
Cutting spending	-0.01 [-0.03; 0.01]	-0.00 [-0.02; 0.02]
Cutting spending x T1	0.01 [-0.02; 0.04]	-0.00 [-0.03; 0.03]
Cutting spending x T2	0.01 [-0.02; 0.03]	0.00 [-0.02; 0.03]
Increasing taxes	-0.04* [-0.06; -0.02]	-0.04* [-0.06; -0.02]
Increasing taxes x T1	0.03* [0.00; 0.05]	0.02 [-0.01; 0.05]
Increasing taxes x T2	0.03* [0.00; 0.05]	0.03* [0.00; 0.06]
Defense	0.17* [0.14; 0.20]	0.14* [0.10; 0.18]
Defense x T1	-0.07* [-0.12; -0.03]	-0.08* [-0.14; -0.02]
Defense x T2	-0.05* [-0.09; -0.00]	-0.04 [-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]
R&D	0.10*	0.11*
	[0.07; 0.13]	[0.08; 0.14]
R&D x T1	-0.03	-0.05*
	[-0.07; 0.01]	[-0.10; -0.00]
R&D 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 T1	-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 C4: Heterogeneous effects with party

	Republican	Democrat	Independent
(Intercept)	0.43* [0.38; 0.49]	0.36* [0.32; 0.40]	0.37* [0.30; 0.44]
Companies	0.02 [−0.03; 0.06]	0.07* [0.04; 0.10]	0.07* [0.01; 0.13]
Companies x T1	−0.01 [−0.08; 0.05]	−0.00 [−0.04; 0.04]	−0.03 [−0.11; 0.06]
Companies x T2	0.01 [−0.05; 0.08]	−0.02 [−0.06; 0.02]	−0.01 [−0.09; 0.06]
Older people	−0.15* [−0.20; −0.11]	−0.14* [−0.17; −0.11]	−0.10* [−0.16; −0.05]
Older people x T1	0.03 [−0.04; 0.10]	0.02 [−0.02; 0.06]	0.03 [−0.05; 0.10]
Older people x T2	0.08* [0.00; 0.15]	−0.00 [−0.04; 0.04]	−0.01 [−0.09; 0.07]
Wealthy	0.02 [−0.02; 0.06]	0.10* [0.07; 0.13]	0.08* [0.03; 0.14]
Wealthy x T1	0.01 [−0.05; 0.07]	0.01 [−0.03; 0.05]	0.02 [−0.06; 0.09]
Wealthy x T2	0.05 [−0.01; 0.12]	0.00 [−0.04; 0.04]	0.00 [−0.07; 0.08]
Cutting spending	0.02 [−0.02; 0.06]	−0.03* [−0.05; −0.00]	−0.01 [−0.06; 0.04]
Cutting spending x T1	−0.02 [−0.08; 0.03]	0.02 [−0.01; 0.05]	0.04 [−0.02; 0.11]
Cutting spending x T2	−0.01 [−0.06; 0.04]	0.02 [−0.01; 0.05]	−0.00 [−0.07; 0.06]
Increasing taxes	−0.05* [−0.09; −0.02]	−0.03* [−0.05; −0.00]	−0.06* [−0.11; −0.02]
Increasing taxes x T1	0.02 [−0.03; 0.07]	0.02 [−0.01; 0.05]	0.07* [0.00; 0.13]
Increasing taxes x T2	0.03 [−0.02; 0.08]	0.03 [−0.00; 0.06]	0.03 [−0.04; 0.10]
Defense	0.05 [−0.02; 0.11]	0.23* [0.19; 0.28]	0.12* [0.03; 0.21]
Defense x T1	−0.10 [−0.20; 0.01]	−0.07* [−0.13; −0.01]	−0.01 [−0.12; 0.10]
Defense x T2	−0.03 [−0.11; 0.06]	−0.05 [−0.11; 0.00]	−0.01 [−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]
R&D	0.11*	0.11*	0.07
	[0.06; 0.17]	[0.07; 0.15]	[-0.01; 0.14]
R&D x T1	-0.10*	-0.02	0.04
	[-0.18; -0.01]	[-0.08; 0.04]	[-0.06; 0.13]
R&D 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 C5: Heterogeneous effects with presidential vote

	D.Trump	H.Clinton	Did not vote
(Intercept)	0.41* [0.34; 0.48]	0.37* [0.33; 0.42]	0.38* [0.32; 0.43]
Companies	0.02 [−0.02; 0.07]	0.08* [0.05; 0.11]	0.05* [0.01; 0.09]
Companies x T1	−0.01 [−0.08; 0.06]	−0.01 [−0.06; 0.04]	−0.03 [−0.09; 0.03]
Companies x T2	−0.01 [−0.08; 0.06]	−0.03 [−0.07; 0.01]	0.03 [−0.03; 0.08]
Older people	−0.15* [−0.21; −0.10]	−0.15* [−0.18; −0.11]	−0.12* [−0.16; −0.08]
Older people x T1	0.04 [−0.04; 0.11]	0.04 [−0.01; 0.09]	−0.01 [−0.07; 0.05]
Older people x T2	0.02 [−0.06; 0.10]	0.02 [−0.03; 0.06]	0.00 [−0.05; 0.06]
Wealthy	0.02 [−0.03; 0.07]	0.10* [0.07; 0.14]	0.08* [0.03; 0.12]
Wealthy x T1	0.03 [−0.04; 0.10]	0.02 [−0.03; 0.06]	−0.01 [−0.07; 0.06]
Wealthy x T2	0.02 [−0.05; 0.09]	0.01 [−0.04; 0.05]	0.02 [−0.04; 0.08]
Cutting spending	0.04 [−0.01; 0.08]	−0.02 [−0.04; 0.01]	−0.03 [−0.07; 0.00]
Cutting spending x T1	−0.03 [−0.09; 0.03]	0.00 [−0.04; 0.04]	0.03 [−0.02; 0.07]
Cutting spending x T2	−0.01 [−0.07; 0.05]	0.01 [−0.02; 0.05]	0.01 [−0.04; 0.05]
Increasing taxes	−0.03 [−0.07; 0.00]	−0.03* [−0.05; −0.00]	−0.05* [−0.09; −0.02]
Increasing taxes x T1	0.00 [−0.05; 0.05]	0.03 [−0.01; 0.06]	0.05 [−0.00; 0.09]
Increasing taxes x T2	0.01 [−0.04; 0.07]	0.02 [−0.02; 0.05]	0.02 [−0.02; 0.07]
Defense	0.05 [−0.02; 0.13]	0.22* [0.17; 0.27]	0.18* [0.12; 0.25]
Defense x T1	−0.07 [−0.18; 0.05]	−0.05 [−0.12; 0.02]	−0.09* [−0.18; −0.00]
Defense x T2	−0.01 [−0.11; 0.08]	−0.03 [−0.10; 0.03]	−0.10* [−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]
R&D	0.12*	0.10*	0.11*
	[0.06; 0.19]	[0.06; 0.15]	[0.06; 0.17]
R&D x T1	-0.07	-0.01	-0.06
	[-0.17; 0.02]	[-0.08; 0.05]	[-0.14; 0.02]
R&D 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 C6: Heterogeneous effects with top value of life

	Top value of life	Not top value of life
(Intercept)	0.34* [0.31; 0.38]	0.40* [0.36; 0.44]
Companies	0.08* [0.05; 0.11]	0.05* [0.02; 0.08]
Companies x T1	-0.00 [-0.04; 0.04]	-0.01 [-0.06; 0.03]
Companies x T2	-0.03 [-0.07; 0.01]	-0.00 [-0.04; 0.04]
Older people	-0.16* [-0.19; -0.13]	-0.11* [-0.14; -0.08]
Older people x T1	0.05* [0.00; 0.09]	0.01 [-0.03; 0.05]
Older people x T2	0.02 [-0.03; 0.06]	-0.00 [-0.04; 0.04]
Wealthy	0.09* [0.06; 0.12]	0.07* [0.04; 0.10]
Wealthy x T1	0.04 [-0.00; 0.09]	-0.01 [-0.05; 0.03]
Wealthy x T2	-0.00 [-0.05; 0.04]	0.01 [-0.03; 0.05]
Cutting spending	-0.02 [-0.05; 0.00]	0.00 [-0.02; 0.03]
Cutting spending x T1	0.03 [-0.00; 0.07]	-0.02 [-0.05; 0.01]
Cutting spending x T2	0.02 [-0.01; 0.06]	-0.02 [-0.05; 0.01]
Increasing taxes	-0.05* [-0.08; -0.02]	-0.03* [-0.05; -0.01]
Increasing taxes x T1	0.04 [-0.00; 0.08]	0.01 [-0.02; 0.04]
Increasing taxes x T2	0.06* [0.02; 0.09]	0.00 [-0.03; 0.03]
Defense	0.20* [0.16; 0.25]	0.15* [0.10; 0.19]
Defense x T1	-0.08* [-0.14; -0.01]	-0.05 [-0.11; 0.01]
Defense x T2	-0.06 [-0.12; 0.01]	-0.04 [-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]
R&D	0.11*	0.08*
	[0.07; 0.15]	[0.05; 0.12]
R&D x T1	-0.03	-0.03
	[-0.09; 0.03]	[-0.08; 0.03]
R&D 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 C7: Heterogeneous effects with aggregate preferences for the allocation of tax burden (Conditioning on “Who should pay?”)

	Companies	Older people	Wealthy
(Intercept)	0.44* [0.39; 0.48]	0.28* [0.23; 0.32]	0.44* [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 [-0.04; 0.04]	0.00 [-0.03; 0.04]	-0.06* [-0.09; -0.02]
Cutting spending x T1	-0.00 [-0.05; 0.05]	-0.01 [-0.06; 0.04]	0.05 [-0.01; 0.10]
Cutting spending x T2	0.01 [-0.04; 0.06]	-0.01 [-0.06; 0.04]	0.04 [-0.01; 0.10]
Increasing taxes	-0.08* [-0.11; -0.04]	-0.02 [-0.06; 0.01]	-0.05* [-0.08; -0.01]
Increasing taxes x T1	0.07* [0.02; 0.13]	0.02 [-0.03; 0.06]	0.02 [-0.03; 0.07]
Increasing taxes x T2	0.05 [-0.00; 0.10]	0.03 [-0.02; 0.07]	0.05* [0.00; 0.11]
Defense	0.20* [0.13; 0.26]	0.09* [0.03; 0.14]	0.21* [0.15; 0.27]
Defense x T1	-0.06 [-0.15; 0.03]	-0.08 [-0.17; 0.01]	-0.04 [-0.13; 0.05]
Defense x T2	-0.05 [-0.13; 0.04]	-0.04 [-0.12; 0.04]	-0.07 [-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]
R&D	0.12* [0.06; 0.18]	0.04 [-0.01; 0.10]	0.14* [0.08; 0.19]
R&D x T1	-0.02 [-0.11; 0.06]	-0.03 [-0.11; 0.06]	-0.03 [-0.12; 0.05]
R&D 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.

Table C8: Heterogeneous effects with with the estimation of deaths

	Underestimate	Overestimate
(Intercept)	0.38* [0.34; 0.43]	0.37* [0.33; 0.41]
Companies	0.05* [0.02; 0.08]	0.07* [0.04; 0.10]
Companies x T1	-0.00 [-0.05; 0.04]	-0.01 [-0.05; 0.03]
Companies x T2	-0.03 [-0.07; 0.01]	0.01 [-0.03; 0.05]
Older people	-0.13* [-0.17; -0.10]	-0.13* [-0.16; -0.10]
Older people x T1	0.05* [0.00; 0.09]	0.01 [-0.03; 0.05]
Older people x T2	0.01 [-0.04; 0.06]	0.01 [-0.03; 0.05]
Wealthy	0.07* [0.04; 0.10]	0.09* [0.06; 0.12]
Wealthy x T1	0.01 [-0.03; 0.06]	0.01 [-0.03; 0.06]
Wealthy x T2	-0.02 [-0.06; 0.02]	0.04 [-0.00; 0.08]
Cutting spending	-0.02 [-0.04; 0.01]	-0.00 [-0.03; 0.02]
Cutting spending x T1	-0.00 [-0.04; 0.03]	0.01 [-0.02; 0.05]
Cutting spending x T2	0.01 [-0.02; 0.05]	-0.01 [-0.04; 0.02]
Increasing taxes	-0.05* [-0.08; -0.03]	-0.03* [-0.05; -0.01]
Increasing taxes x T1	0.01 [-0.03; 0.04]	0.03* [0.00; 0.07]
Increasing taxes x T2	0.04* [0.00; 0.07]	0.01 [-0.02; 0.05]
Defence	0.18* [0.13; 0.23]	0.17* [0.13; 0.21]
Defence x T1	-0.09* [-0.15; -0.02]	-0.06* [-0.13; -0.00]
Defence x T2	-0.08* [-0.15; -0.02]	-0.03 [-0.09; 0.03]

Environment	0.08*	0.07*
	[0.03; 0.13]	[0.03; 0.11]
Environment x T1	-0.03	-0.04
	[-0.10; 0.03]	[-0.10; 0.02]
Environment x T2	-0.06	-0.06*
	[-0.13; 0.00]	[-0.12; -0.01]
Foreign aid	0.19*	0.17*
	[0.15; 0.24]	[0.12; 0.21]
Foreign aid x T1	-0.08*	-0.07*
	[-0.15; -0.01]	[-0.13; -0.01]
Foreign aid x T2	-0.08*	-0.03
	[-0.14; -0.01]	[-0.09; 0.02]
Research development	0.11*	0.09*
	[0.06; 0.15]	[0.05; 0.13]
Research development x T1	-0.02	-0.05
	[-0.08; 0.05]	[-0.10; 0.01]
Research development x T2	-0.05	-0.02
	[-0.11; 0.01]	[-0.07; 0.03]
Pensions	0.06*	0.05*
	[0.01; 0.10]	[0.01; 0.09]
Pensions x T1	-0.02	-0.05
	[-0.08; 0.05]	[-0.11; 0.01]
Pensions x T2	-0.08*	0.01
	[-0.14; -0.02]	[-0.04; 0.07]
Social security	-0.03	0.02
	[-0.08; 0.01]	[-0.02; 0.06]
Social security x T1	0.02	-0.05
	[-0.05; 0.08]	[-0.11; 0.01]
Social security x T2	-0.01	-0.06*
	[-0.08; 0.05]	[-0.11; -0.00]
Public transport	0.11*	0.10*
	[0.07; 0.16]	[0.06; 0.14]
Public transport x T1	-0.05	-0.04
	[-0.11; 0.01]	[-0.09; 0.02]
Public transport x T2	-0.05	-0.02
	[-0.11; 0.01]	[-0.08; 0.03]
T1	0.01	0.01
	[-0.04; 0.07]	[-0.04; 0.07]
T2	0.06	0.01
	[-0.00; 0.12]	[-0.04; 0.06]
R ²	0.10	0.12
Adj. R ²	0.10	0.12
Num. obs.	8197	9792
RMSE	0.28	0.27
N Clusters	1011	1218

* Null hypothesis value outside the confidence interval.

Table C9: Heterogeneous effects with with the estimation of income loss

	Underestimate	Overestimate
(Intercept)	0.41* [0.34; 0.48]	0.37* [0.34; 0.40]
Companies	0.06* [0.01; 0.11]	0.06* [0.04; 0.09]
Companies x T1	-0.01 [-0.08; 0.07]	-0.01 [-0.04; 0.02]
Companies x T2	-0.01 [-0.08; 0.05]	-0.01 [-0.04; 0.02]
Older people	-0.11* [-0.16; -0.06]	-0.13* [-0.16; -0.11]
Older people x T1	-0.01 [-0.08; 0.06]	0.03 [-0.01; 0.06]
Older people x T2	-0.03 [-0.09; 0.03]	0.01 [-0.02; 0.05]
Wealthy	0.06* [0.01; 0.11]	0.08* [0.06; 0.11]
Wealthy x T1	0.02 [-0.05; 0.09]	0.01 [-0.02; 0.05]
Wealthy x T2	0.01 [-0.05; 0.07]	0.01 [-0.02; 0.05]
Cutting spending	-0.03 [-0.07; 0.01]	-0.00 [-0.02; 0.02]
Cutting spending x T1	0.03 [-0.03; 0.08]	-0.00 [-0.03; 0.02]
Cutting spending x T2	0.04 [-0.01; 0.09]	-0.01 [-0.04; 0.02]
Increasing taxes	-0.08* [-0.12; -0.04]	-0.03* [-0.05; -0.01]
Increasing taxes x T1	0.07* [0.02; 0.13]	0.01 [-0.02; 0.04]
Increasing taxes x T2	0.06* [0.01; 0.11]	0.01 [-0.02; 0.04]
Defence	0.15* [0.08; 0.21]	0.18* [0.14; 0.22]
Defence x T1	0.00 [-0.10; 0.10]	-0.09* [-0.14; -0.04]
Defence x T2	-0.02 [-0.11; 0.08]	-0.06* [-0.11; -0.01]

Environment	0.07	0.07*
	[-0.01; 0.14]	[0.04; 0.11]
Environment x T1	0.01	-0.04
	[-0.09; 0.11]	[-0.09; 0.01]
Environment x T2	-0.04	-0.06*
	[-0.14; 0.06]	[-0.11; -0.01]
Foreign aid	0.14*	0.19*
	[0.07; 0.21]	[0.15; 0.22]
Foreign aid x T1	-0.02	-0.08*
	[-0.12; 0.07]	[-0.13; -0.04]
Foreign aid x T2	-0.05	-0.04
	[-0.15; 0.05]	[-0.09; 0.00]
Research development	0.06	0.10*
	[-0.01; 0.13]	[0.07; 0.13]
Research development x T1	0.02	-0.04
	[-0.08; 0.12]	[-0.08; 0.01]
Research development x T2	-0.01	-0.03
	[-0.10; 0.08]	[-0.08; 0.01]
Pensions	0.02	0.05*
	[-0.05; 0.10]	[0.02; 0.09]
Pensions x T1	-0.02	-0.03
	[-0.12; 0.09]	[-0.08; 0.01]
Pensions x T2	-0.01	-0.03
	[-0.11; 0.08]	[-0.07; 0.02]
Social security	-0.03	0.00
	[-0.11; 0.04]	[-0.03; 0.03]
Social security x T1	-0.01	-0.02
	[-0.11; 0.09]	[-0.07; 0.03]
Social security x T2	-0.04	-0.03
	[-0.13; 0.05]	[-0.08; 0.01]
Public transport	0.14*	0.09*
	[0.07; 0.20]	[0.06; 0.13]
Public transport x T1	-0.06	-0.03
	[-0.15; 0.04]	[-0.08; 0.01]
Public transport x T2	-0.08	-0.01
	[-0.17; 0.00]	[-0.06; 0.03]
T1	-0.06	0.03
	[-0.16; 0.04]	[-0.02; 0.07]
T2	0.01	0.04
	[-0.08; 0.10]	[-0.01; 0.08]
R ²	0.13	0.11
Adj. R ²	0.12	0.11
Num. obs.	3792	13909
RMSE	0.27	0.28
N Clusters	470	1723

* Null hypothesis value outside the confidence interval.

Table C10: Heterogeneous effects with seriousness of the pandemic

	Not serious	Serious
(Intercept)	0.38*	0.38*
	[0.32; 0.44]	[0.34; 0.41]
Companies	0.06*	0.06*
	[0.01; 0.10]	[0.04; 0.08]
Companies x T1	-0.07*	0.01
	[-0.14; -0.00]	[-0.03; 0.04]
Companies x T2	-0.02	-0.01
	[-0.09; 0.04]	[-0.04; 0.03]
Older people	-0.09*	-0.14*
	[-0.14; -0.04]	[-0.16; -0.11]
Older people x T1	-0.05	0.04*
	[-0.11; 0.02]	[0.01; 0.07]
Older people x T2	-0.01	0.01
	[-0.07; 0.06]	[-0.02; 0.04]
Wealthy	0.04	0.09*
	[-0.01; 0.08]	[0.06; 0.11]
Wealthy x T1	-0.02	0.02
	[-0.08; 0.05]	[-0.01; 0.06]
Wealthy x T2	0.01	0.01
	[-0.05; 0.08]	[-0.02; 0.04]
Cutting spending	0.00	-0.01
	[-0.04; 0.04]	[-0.03; 0.01]
Cutting spending x T1	-0.02	0.01
	[-0.08; 0.04]	[-0.02; 0.04]
Cutting spending x T2	-0.00	0.00
	[-0.05; 0.05]	[-0.03; 0.03]
Increasing taxes	-0.03	-0.04*
	[-0.07; 0.00]	[-0.06; -0.02]
Increasing taxes x T1	-0.00	0.03*
	[-0.06; 0.05]	[0.00; 0.05]
Increasing taxes x T2	-0.00	0.03*
	[-0.06; 0.05]	[0.01; 0.06]
Defence	0.04	0.20*
	[-0.03; 0.10]	[0.17; 0.24]
Defence x T1	-0.02	-0.07*
	[-0.11; 0.08]	[-0.12; -0.02]
Defence x T2	0.01	-0.05*
	[-0.08; 0.09]	[-0.10; -0.01]

Environment	0.07*	0.08*
	[0.00; 0.14]	[0.04; 0.11]
Environment x T1	0.02	-0.05*
	[-0.07; 0.11]	[-0.10; -0.00]
Environment x T2	-0.08	-0.05*
	[-0.17; 0.02]	[-0.10; -0.01]
Foreign aid	0.15*	0.19*
	[0.08; 0.22]	[0.15; 0.22]
Foreign aid x T1	-0.08	-0.07*
	[-0.17; 0.01]	[-0.12; -0.02]
Foreign aid x T2	-0.00	-0.07*
	[-0.10; 0.09]	[-0.11; -0.02]
Research development	0.10*	0.10*
	[0.04; 0.16]	[0.07; 0.13]
Research development x T1	-0.07	-0.02
	[-0.16; 0.01]	[-0.07; 0.03]
Research development x T2	-0.03	-0.04
	[-0.12; 0.05]	[-0.08; 0.01]
Pensions	0.01	0.06*
	[-0.05; 0.07]	[0.03; 0.10]
Pensions x T1	0.01	-0.05
	[-0.08; 0.10]	[-0.10; 0.00]
Pensions x T2	0.02	-0.05
	[-0.07; 0.11]	[-0.09; 0.00]
Social security	-0.04	-0.00
	[-0.10; 0.03]	[-0.03; 0.03]
Social security x T1	0.00	-0.02
	[-0.08; 0.09]	[-0.07; 0.03]
Social security x T2	0.01	-0.05
	[-0.07; 0.10]	[-0.09; 0.00]
Public transport	0.04	0.12*
	[-0.03; 0.10]	[0.09; 0.15]
Public transport x T1	0.01	-0.05*
	[-0.08; 0.09]	[-0.09; -0.00]
Public transport x T2	0.03	-0.05*
	[-0.05; 0.11]	[-0.09; -0.00]
T1	0.05	0.00
	[-0.04; 0.13]	[-0.04; 0.05]
T2	0.01	0.04
	[-0.07; 0.10]	[-0.00; 0.08]
R ²	0.08	0.13
Adj. R ²	0.07	0.12
Num. obs.	3757	14287
RMSE	0.27	0.28
N Clusters	467	1770

* Null hypothesis value outside the confidence interval.

Table C11: Heterogeneous effects with concern about COVID-19

	Not concerned	Concerned
(Intercept)	0.37* [0.30; 0.44]	0.38* [0.35; 0.41]
Companies	0.06* [0.01; 0.12]	0.06* [0.04; 0.08]
Companies x T1	-0.07 [-0.16; 0.01]	0.00 [-0.03; 0.04]
Companies x T2	-0.02 [-0.09; 0.06]	-0.01 [-0.04; 0.02]
Older people	-0.11* [-0.17; -0.06]	-0.13* [-0.16; -0.11]
Older people x T1	0.01 [-0.07; 0.08]	0.03 [-0.01; 0.06]
Older people x T2	0.00 [-0.07; 0.08]	0.01 [-0.03; 0.04]
Wealthy	0.05 [-0.01; 0.11]	0.08* [0.06; 0.11]
Wealthy x T1	-0.03 [-0.11; 0.05]	0.02 [-0.01; 0.06]
Wealthy x T2	0.02 [-0.06; 0.10]	0.01 [-0.03; 0.04]
Cutting spending	0.00 [-0.05; 0.05]	-0.01 [-0.03; 0.01]
Cutting spending x T1	0.00 [-0.07; 0.07]	0.01 [-0.02; 0.03]
Cutting spending x T2	-0.01 [-0.07; 0.06]	0.00 [-0.02; 0.03]
Increasing taxes	-0.05* [-0.09; -0.00]	-0.04* [-0.06; -0.02]
Increasing taxes x T1	0.00 [-0.06; 0.07]	0.03* [0.00; 0.05]
Increasing taxes x T2	0.02 [-0.05; 0.08]	0.03* [0.00; 0.05]
Defence	0.11* [0.04; 0.19]	0.18* [0.15; 0.22]
Defence x T1	-0.11 [-0.23; 0.02]	-0.06* [-0.11; -0.01]
Defence x T2	-0.03 [-0.14; 0.08]	-0.05* [-0.10; -0.01]

Environment	0.06	0.08*
	[-0.03; 0.14]	[0.04; 0.11]
Environment x T1	-0.01	-0.04
	[-0.12; 0.11]	[-0.09; 0.00]
Environment x T2	-0.09	-0.05*
	[-0.21; 0.02]	[-0.10; -0.01]
Foreign aid	0.20*	0.17*
	[0.13; 0.26]	[0.14; 0.21]
Foreign aid x T1	-0.16*	-0.06*
	[-0.26; -0.05]	[-0.11; -0.01]
Foreign aid x T2	-0.06	-0.05*
	[-0.17; 0.05]	[-0.10; -0.00]
Research development	0.10*	0.10*
	[0.02; 0.17]	[0.07; 0.13]
Research development x T1	-0.08	-0.02
	[-0.18; 0.02]	[-0.07; 0.02]
Research development x T2	-0.02	-0.04
	[-0.12; 0.08]	[-0.08; 0.01]
Pensions	0.06	0.05*
	[-0.02; 0.13]	[0.02; 0.09]
Pensions x T1	-0.09	-0.02
	[-0.19; 0.01]	[-0.07; 0.02]
Pensions x T2	-0.02	-0.03
	[-0.12; 0.08]	[-0.08; 0.01]
Social security	-0.02	-0.01
	[-0.10; 0.06]	[-0.04; 0.03]
Social security x T1	-0.05	-0.01
	[-0.16; 0.05]	[-0.06; 0.03]
Social security x T2	-0.03	-0.03
	[-0.13; 0.07]	[-0.08; 0.01]
Public transport	0.08*	0.11*
	[0.00; 0.16]	[0.07; 0.14]
Public transport x T1	-0.06	-0.03
	[-0.17; 0.04]	[-0.08; 0.01]
Public transport x T2	-0.02	-0.03
	[-0.12; 0.09]	[-0.08; 0.01]
T1	0.04	0.01
	[-0.06; 0.14]	[-0.04; 0.05]
T2	0.03	0.03
	[-0.07; 0.12]	[-0.01; 0.08]
R ²	0.09	0.12
Adj. R ²	0.08	0.11
Num. obs.	2653	15463
RMSE	0.28	0.27
N Clusters	325	1920

* Null hypothesis value outside the confidence interval.

Table C12: Heterogeneous effects with health anxiety

	Not anxious	Anxious
(Intercept)	0.37* [0.33; 0.41]	0.37* [0.33; 0.42]
Companies	0.07* [0.04; 0.10]	0.06* [0.03; 0.09]
Companies x T1	-0.03 [-0.07; 0.01]	0.01 [-0.03; 0.06]
Companies x T2	-0.03 [-0.07; 0.01]	0.00 [-0.04; 0.04]
Older people	-0.11* [-0.14; -0.08]	-0.14* [-0.18; -0.11]
Older people x T1	-0.00 [-0.04; 0.04]	0.04 [-0.01; 0.08]
Older people x T2	-0.00 [-0.04; 0.04]	0.02 [-0.03; 0.06]
Wealthy	0.06* [0.03; 0.09]	0.10* [0.06; 0.13]
Wealthy x T1	0.01 [-0.04; 0.05]	0.02 [-0.03; 0.07]
Wealthy x T2	0.02 [-0.03; 0.06]	0.00 [-0.04; 0.05]
Cutting spending	-0.00 [-0.03; 0.02]	-0.01 [-0.04; 0.01]
Cutting spending x T1	-0.01 [-0.05; 0.02]	0.02 [-0.01; 0.06]
Cutting spending x T2	-0.01 [-0.04; 0.03]	0.00 [-0.03; 0.04]
Increasing taxes	-0.04* [-0.07; -0.02]	-0.04* [-0.06; -0.01]
Increasing taxes x T1	0.01 [-0.03; 0.04]	0.03 [-0.00; 0.07]
Increasing taxes x T2	0.02 [-0.02; 0.05]	0.03 [-0.00; 0.06]
Defence	0.15* [0.10; 0.19]	0.21* [0.16; 0.26]
Defence x T1	-0.06 [-0.12; 0.01]	-0.09* [-0.16; -0.02]
Defence x T2	-0.07* [-0.13; -0.01]	-0.04 [-0.10; 0.02]

Environment	0.08*	0.08*
	[0.04; 0.13]	[0.04; 0.13]
Environment x T1	-0.04	-0.05
	[-0.10; 0.02]	[-0.11; 0.01]
Environment x T2	-0.07*	-0.05
	[-0.13; -0.01]	[-0.11; 0.01]
Foreign aid	0.18*	0.19*
	[0.14; 0.22]	[0.14; 0.24]
Foreign aid x T1	-0.09*	-0.08*
	[-0.15; -0.03]	[-0.14; -0.01]
Foreign aid x T2	-0.06	-0.06
	[-0.12; 0.00]	[-0.12; 0.00]
Research development	0.09*	0.11*
	[0.05; 0.13]	[0.07; 0.15]
Research development x T1	-0.05	-0.02
	[-0.11; 0.01]	[-0.08; 0.04]
Research development x T2	-0.02	-0.05
	[-0.08; 0.03]	[-0.11; 0.01]
Pensions	0.04*	0.07*
	[0.00; 0.09]	[0.02; 0.11]
Pensions x T1	-0.03	-0.04
	[-0.09; 0.03]	[-0.11; 0.02]
Pensions x T2	-0.02	-0.03
	[-0.08; 0.04]	[-0.09; 0.03]
Social security	-0.00	-0.01
	[-0.04; 0.04]	[-0.05; 0.04]
Social security x T1	-0.03	-0.02
	[-0.09; 0.03]	[-0.09; 0.04]
Social security x T2	-0.04	-0.03
	[-0.09; 0.02]	[-0.09; 0.03]
Public transport	0.10*	0.11*
	[0.06; 0.14]	[0.06; 0.15]
Public transport x T1	-0.05	-0.02
	[-0.11; 0.00]	[-0.08; 0.04]
Public transport x T2	-0.04	-0.03
	[-0.09; 0.02]	[-0.09; 0.03]
T1	0.04	-0.01
	[-0.01; 0.10]	[-0.07; 0.05]
T2	0.04	0.03
	[-0.02; 0.09]	[-0.03; 0.09]
R ²	0.09	0.14
Adj. R ²	0.09	0.13
Num. obs.	8501	8679
RMSE	0.27	0.28
N Clusters	1060	1079

* Null hypothesis value outside the confidence interval.

Table C13: Heterogeneous effects with economic vulnerability

	Not vulnerable	Vulnerable
(Intercept)	0.37* [0.32; 0.43]	0.37* [0.34; 0.40]
Companies	0.09* [0.05; 0.12]	0.07* [0.05; 0.09]
Companies x T1	-0.01 [-0.07; 0.05]	-0.00 [-0.03; 0.03]
Companies x T2	-0.05* [-0.11; -0.00]	-0.02 [-0.05; 0.01]
Older people	-0.08* [-0.12; -0.04]	-0.13* [-0.15; -0.11]
Older people x T1	-0.00 [-0.06; 0.06]	0.02 [-0.01; 0.05]
Older people x T2	-0.03 [-0.08; 0.03]	0.01 [-0.02; 0.04]
Wealthy	0.12* [0.08; 0.16]	0.08* [0.06; 0.10]
Wealthy x T1	-0.00 [-0.06; 0.06]	0.02 [-0.02; 0.05]
Wealthy x T2	-0.05 [-0.10; 0.01]	0.01 [-0.02; 0.04]
Cutting spending	-0.02 [-0.05; 0.01]	-0.01 [-0.02; 0.01]
Cutting spending x T1	0.04 [-0.01; 0.09]	0.00 [-0.02; 0.03]
Cutting spending x T2	0.01 [-0.03; 0.05]	-0.00 [-0.03; 0.02]
Increasing taxes	-0.04* [-0.07; -0.01]	-0.04* [-0.06; -0.02]
Increasing taxes x T1	0.04 [-0.01; 0.08]	0.02 [-0.00; 0.05]
Increasing taxes x T2	0.03 [-0.01; 0.07]	0.03* [0.00; 0.05]
Defence	0.14* [0.08; 0.20]	0.18* [0.15; 0.22]
Defence x T1	-0.00 [-0.08; 0.08]	-0.07* [-0.12; -0.03]
Defence x T2	-0.08 [-0.16; 0.00]	-0.06* [-0.10; -0.01]

Environment	0.07*	0.08*
	[0.01; 0.13]	[0.05; 0.12]
Environment x T1	-0.04	-0.06*
	[-0.12; 0.05]	[-0.10; -0.01]
Environment x T2	-0.08	-0.06*
	[-0.16; 0.01]	[-0.10; -0.02]
Foreign aid	0.20*	0.19*
	[0.14; 0.26]	[0.15; 0.22]
Foreign aid x T1	-0.05	-0.08*
	[-0.13; 0.03]	[-0.13; -0.03]
Foreign aid x T2	-0.12*	-0.06*
	[-0.21; -0.04]	[-0.10; -0.01]
Research development	0.13*	0.10*
	[0.07; 0.19]	[0.07; 0.13]
Research development x T1	-0.03	-0.04
	[-0.11; 0.06]	[-0.08; 0.01]
Research development x T2	-0.10*	-0.04
	[-0.18; -0.02]	[-0.08; 0.00]
Pensions	0.03	0.06*
	[-0.03; 0.09]	[0.03; 0.09]
Pensions x T1	0.05	-0.04
	[-0.03; 0.14]	[-0.09; 0.00]
Pensions x T2	-0.05	-0.03
	[-0.14; 0.03]	[-0.08; 0.01]
Social security	0.01	-0.00
	[-0.05; 0.07]	[-0.03; 0.03]
Social security x T1	0.01	-0.03
	[-0.08; 0.09]	[-0.07; 0.02]
Social security x T2	-0.11*	-0.03
	[-0.18; -0.03]	[-0.08; 0.01]
Public transport	0.10*	0.11*
	[0.04; 0.15]	[0.08; 0.14]
Public transport x T1	-0.02	-0.04
	[-0.09; 0.06]	[-0.08; 0.00]
Public transport x T2	-0.06	-0.04
	[-0.14; 0.02]	[-0.08; 0.01]
T1	-0.02	0.02
	[-0.10; 0.06]	[-0.02; 0.06]
T2	0.10*	0.04
	[0.02; 0.18]	[-0.00; 0.08]
R ²	0.10	0.11
Adj. R ²	0.09	0.11
Num. obs.	5077	16396
RMSE	0.27	0.27
N Clusters	635	2042

* Null hypothesis value outside the confidence interval.

D Fiscal preferences expressed in the ANES 2016

Tables D1 and D2 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, while the last item (defense cuts) runs from 1 to 7. The highest values represent most support for the relevant proposal.

Table D1: 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 D2: 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.

E Support for fiscal policy packages

Tables E1, E2 and E3 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 E1 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 E2 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 E3 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 E1: 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 E2: 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 E3: 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

F October wave

Table F1: October wave within-subject aggregate results

	October Preferences of Respondents in T1/T2 in April
(Intercept)	0.39* [0.37; 0.42]
Companies	0.06* [0.04; 0.08]
Older people	-0.18* [-0.20; -0.16]
Wealthy	0.11* [0.09; 0.13]
Cutting spending	-0.01 [-0.02; 0.01]
Increasing taxes	-0.03* [-0.05; -0.02]
Defence	0.18* [0.15; 0.22]
Environment	0.05* [0.03; 0.08]
Foreign aid	0.18* [0.15; 0.21]
Research development	0.10* [0.07; 0.12]
Pensions	0.03* [0.00; 0.06]
Social security	-0.04* [-0.07; -0.01]
Public transport	0.09* [0.06; 0.12]
R ²	0.17
Adj. R ²	0.17
Num. obs.	7171
RMSE	0.29
N Clusters	795

* 0 outside the confidence interval.

Table F2: October wave between-subject aggregate results

	Treated in April	Not treated in April
(Intercept)	0.39* [0.37; 0.42]	0.38* [0.34; 0.42]
Companies	0.06* [0.04; 0.08]	0.08* [0.04; 0.11]
Older people	-0.18* [-0.20; -0.16]	-0.18* [-0.21; -0.15]
Wealthy	0.11* [0.09; 0.13]	0.09* [0.06; 0.12]
Cutting spending	-0.01 [-0.02; 0.01]	-0.01 [-0.03; 0.01]
Increasing taxes	-0.03* [-0.05; -0.02]	-0.04* [-0.07; -0.02]
Defence	0.18* [0.15; 0.22]	0.18* [0.14; 0.22]
Environment	0.05* [0.03; 0.08]	0.08* [0.04; 0.12]
Foreign aid	0.18* [0.15; 0.21]	0.18* [0.13; 0.22]
Research development	0.10* [0.07; 0.12]	0.12* [0.08; 0.16]
Pensions	0.03* [0.00; 0.06]	0.03 [-0.01; 0.07]
Social security	-0.04* [-0.07; -0.01]	-0.01 [-0.05; 0.03]
Public transport	0.09* [0.06; 0.12]	0.13* [0.10; 0.17]
R ²	0.17	0.16
Adj. R ²	0.17	0.16
Num. obs.	7171	3754
RMSE	0.29	0.30
N Clusters	795	407

* Null hypothesis value outside the confidence interval.

Table F3: October wave party results for those who were treated in April

	October Aggregate	October Democrat	October Republican
(Intercept)	0.39* [0.37; 0.42]	0.37* [0.33; 0.40]	0.44* [0.38; 0.50]
Companies	0.06* [0.04; 0.08]	0.10* [0.07; 0.13]	0.01 [−0.03; 0.06]
Older people	−0.18* [−0.20; −0.16]	−0.19* [−0.22; −0.16]	−0.16* [−0.21; −0.11]
Wealthy	0.11* [0.09; 0.13]	0.13* [0.10; 0.16]	0.05* [0.01; 0.10]
Cutting spending	−0.01 [−0.02; 0.01]	−0.01 [−0.03; 0.01]	0.01 [−0.03; 0.04]
Increasing taxes	−0.03* [−0.05; −0.02]	−0.02 [−0.04; 0.00]	−0.07* [−0.10; −0.03]
Defence	0.18* [0.15; 0.22]	0.24* [0.20; 0.29]	0.06 [−0.01; 0.13]
Environment	0.05* [0.03; 0.08]	0.07* [0.04; 0.11]	0.06 [−0.01; 0.13]
Foreign aid	0.18* [0.15; 0.21]	0.21* [0.17; 0.25]	0.12* [0.06; 0.18]
Research development	0.10* [0.07; 0.12]	0.11* [0.07; 0.15]	0.10* [0.04; 0.15]
Pensions	0.03* [0.00; 0.06]	0.07* [0.03; 0.10]	−0.02 [−0.08; 0.04]
Social security	−0.04* [−0.07; −0.01]	−0.04 [−0.07; 0.00]	−0.06 [−0.12; 0.00]
Public transport	0.09* [0.06; 0.12]	0.12* [0.08; 0.16]	0.04 [−0.02; 0.10]
R ²	0.17	0.23	0.11
Adj. R ²	0.17	0.22	0.11
Num. obs.	7171	3856	1615
RMSE	0.29	0.29	0.29
N Clusters	795	431	174

* Null hypothesis value outside the confidence interval.