Barely a ripple in the sea: EU COVID relief funds failed to change citizen attitudes

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Abstract

Political actors may opt to distribute public funds to bolster declining or stagnant public support. This applies to international organisations: European Union actors have adopted this approach as citizen support may be driven by rational economic evaluations. We test whether this is the case using the EU's *announcement* of the historic €750 billion stimulus bill to tackle the damage caused by the COVID pandemic. Did this promise of incoming financial stimulus increase trust and support for the EU? Results of a quasi-experiment facilitated by the coincidental timing of Eurobarometer fieldwork and the EU's announcement of final COVID-19 rescue deal indicate robust, precisely-estimated and well-powered null effects on individuals' trust in the EU, satisfaction with EU democracy, and on approval of the EU's handling of the pandemic. These effects are not substantially conditioned by individual or country characteristics, including domestic economic conditions and levels of euroscepticism. Our findings support previous literature highlighting the difficulties of the EU using public funds to garner support.

Keywords: COVID-19, European Union, financial stimulus, Next Generation EU, political trust, quasi-experiment, utilitarian support

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We did it: Europe is strong, Europe is robust, and above all, Europe is united.

- Charles Michel, European Council President, announces a deal on the EU COVID relief fund.
July 21st 2020.

1 Introduction

Political actors often actively leverage their discretionary spending power to their electoral advantage by seeking to buy political support (Hanretty, 2021; Hicken & Nathan, 2020). Donald Trump, when faced with a simultaneous collapse in domestic approval and economic growth, signed a historic \$900 billion stimulus package that sent a personal cheque into the home of every American. These cheques were stamped with the then-President's own signature in a clear attempt to electorally monopolise the redistributive policy (Hale, 2022). Like domestic executives and political incumbents, EU-level policymakers often leverage fiscal transfers and (re)distribution to their advantage (Schneider, 2018) under the assumption that doing so is likely to reap political benefits in the form of increased support among beneficiaries. Relying on data from 1977 to 2013, not only does Schneider (2018) demonstrate that states receive a significantly greater share of the EU budget when the subjected to elections in the near future (p.115), but that this increases share of the pie is *even* greater when the incumbent is subjected to electoral distress (pp.121-123). This non-random use of discretionary financial assignment is rational given that national governments operate in a supranational arena that citizens are increasingly more aware of, but Schneider (2018) also shows that these actions have political consequences: those states who gain a larger share of EU funds enjoy a significant increase in approval ratings for the domestic government (p.140).

On July 21st 2020, the EU's European Council agreed to a €750 billion package of relief funds (*'Next Generation EU'*) to mitigate the economic damage caused by the COVID-19 pandemic. The agreement was not only momentous in terms of its monetary value, but also because it was a signal of European cooperation and solidarity which broke with traditional EU policymaking (de la Porte & Jense, 2021; Jones, 2021). There

were clear *political* incentives behind the COVID-19 funds: beyond purely economic and COVID-related determinants of the funds' distribution, there is evidence that domestic anti-EU sentiment was an important predictor of which states were promised the larger slice of the financial pie (Armingeon et al., 2022). This, apparently politically motivated, distribution of funds, consistent with evidence of EU-level responsiveness to (low) public support in member states (Schneider, 2018), suggests that one of the ambitions of the *Next Generation EU* fund was to rally support for the polity. In this paper we ask: did the *announcement* EU's unprecedented financial stimulus package agreed on July 21st 2020 indeed improve citizens' trust and support for the EU?

We answer this question by leveraging a quasi-experimental research design involving respondents from twenty-three of the EU's twenty-seven member states. Empirically, and against our expectations of citizens' responsiveness to institutional policy output (Levitt & Snyder, 1997; Tavits, 2009), we present robust and precisely estimated null effects of the announcement of the COVID relief fund deal on various different measures of political support including trust in the supranational institution, satisfaction with EU democracy, and approval of the community's handling of the pandemic.

These findings speak to three interconnected lines of literature. First, it speaks to the scholarship on politically motivated spending plans pursuing remedial responses to political dissatisfaction and electoral malaise. Whilst research highlights the likely pursuit of these goals by both state governments (Dahlberg & Johansson, 2002; Hanretty, 2021; Hicken & Nathan, 2020) and the EU (Armingeon et al., 2022; Schneider, 2018), little work tests the efficacy of doing so. Second, our quasi-experimental study speaks to debates regarding the specific case of the *Next Generation EU* funds that has previous focused on the inter-state lines of conflict (de la Porte & Jense, 2021) or in understanding levels of public support for within-EU distribution and the effect of elite-level cues in shaping this support (Bauhr & Charron, 2022). Third, our results speak to the wider literature on the determinants of support and trust in supranational institutions like the EU. Dynamics in public opinion towards the EU have been explained via combination

of “distributional” conflicts (Eichenberg & Dalton, 1993; Vasilopoulou & Talving, 2020) and “identity” conflicts (Hooghe & Marks, 2009; McLaren, 2002), or between those whose support is determined by utilitarian calculus or identity-based factors. Substantively increasing the perceived benefits emerging from the EU, by signalling unprecedented incoming EU investment, does little to shift public trust, pointing towards the relatively limited role of utilitarian factors in determining contemporary trust in the polity.

2 Brussels to the rescue?

After a marathon four-night EU summit, EU Council President announced on the morning July 21st that the European Council of member states had reached an agreement for the EU recovery fund. In their joint press conference on the agreement on the COVID recovery package, Germany’s then-Chancellor, Angela Merkel, and French President, Emmanuel Macron, made the aim of the package quite clear: next to promoting a quick European recovery, it was to protect the future functionality of the Union through a show of solidarity with fellow EU members. The heads of government worried that diverging national performances that materialised during the pandemic would otherwise sow long-term division between member states and amongst citizens that would impede the functionality of the Union. In light of the decline of public support for the EU since the European sovereign debt crisis (Dustmann et al., 2017) and the subsequent substantial growth of eurosceptic parties (DeVries, 2018), these worries are arguably well founded. Long gone are the days where national governments could strike bargains in Brussels without taking into account the effects of their actions on domestic approval ratings (devries21; Hooghe & Marks, 2009).

The COVID-19 rescue package signals a substantive paradigm shift for the community. Whilst the EU’s track record of providing financial assistance for member state economies has often been delivered alongside “conditionality”¹ (Blyth, 2013; Copelovitch et al.,

¹The COVID-19 relief funds actively avoided the term “conditionality” - laced with connotations of the EU’s *Memorandums of Understanding* with bailout recipients from the Eurozone crisis - although

2016) - that consequently undermine political support (Armingeon & Guthmann, 2014), public trust, and economic evaluations (Devine & Turnbull-Dugarte, 2021) - the COVID-19 rescue package was promoted as a growth-driving initiative. This distinctive policy move away from the EU's historical demand for domestic internal devaluations and fiscal restructuring of growth (Blyth, 2013) towards increased community-based solidarity, described as a "critical juncture" in European political economy, is demonstrated by two features of the COVID-19 relief funds. Firstly, the distribution of grants - amounting to €390 billion and 52% of the total rescue package - as opposed to additional debt commitments that would place increased fiscal burdens on member states signalled a step towards cross-national solidarity and a departure from the *loan-only* approach of the European Stability Mechanism. Second, advances towards debt mutualisation and the establishment of so-called *Eurobonds* have long been rejected by wealth(ier) Northern members and supported by poor(er) Southern members (Matthijs & McNamara, 2015). The deal agreed by the European Council in July 2020 was described by the then-Finance Minister of Germany (now German Chancellor), Olaf Scholz, as a "Hamiltonian moment" on the road towards Fiscal Union given that the deal included, if however time-sensitive, *Coronabonds* that would be part-financed by an unprecedented concession of EU revenue-gathering tax powers to the European Commission (Howarth & Quaglia, 2021).

This distribution of the *Next Generation EU* funds was not, however, to be distributed based purely on economic need or on the severity of COVID contagion. As explored by Armingeon et al. (2022), those states most at risk to rising euroscepticism, even when conditioning on the severity of the pandemic and economic conditions, were entitled to larger *Next Generation EU* funding per capita than those with less eurosceptic publics. This distribution, based on *political* as supposed to economic necessity, is consistent with opportunistic funding distributions from the EU observed elsewhere during the regular financial budgeting process (Schneider, 2018). As highlighted above, Schneider (2018)'s rich quantitative analysis of politically motivated manipulations in the EU's regular financial

some conditions were attached. These were, however, largely focused on driving the direction of spending towards investment projects aligned with climate change ambitions and digitisation (de la Porte & Jense, 2021).

settlement to allow states to “buy” public support suggests that EU-level decision-makers believe they can remedy disenchantment with the polity via spending pledges. Can they?

3 Theory: Can the EU buy support?

The idea that news of EU’s recovery plan would improve public support for the European Union rests on the backbone of utilitarian theories of support of EU support. For a long time, scholars have argued that EU public opinion depends on the perceived economic benefits resulting from integration. Empirically, scholars have found that support for the EU and deeper EU integration is related to economic benefits on an individual level, the perception of economic benefits on a national level and national macroeconomic conditions (Dabrowski et al., 2021; Eichenberg & Dalton, 1993; Gabel & Whitten, 1997; Hobolt & Wratil, 2015; Vasilopoulou & Talving, 2020).

The most direct economic benefits of membership are EU net financial transfers through the annual budget. EU financial transfers serve as a redistributive tool from richer to poorer members (Mattila, 2006). Some authors have argued that beyond promoting economic growth in poorer regions, financial transfers are also “a device to smooth the market integration process” (Carrubba, 1997), functioning as side payments in exchange for further steps on the road to “ever closer union”. Evidence that EU policymakers perceive transfers as having such effect comes from Schneider (2018) who provides robust empirical evidence that national governments with upcoming elections receive significantly larger EU budget shares (p.121). In other words, the EU’s purse strings become significantly more generous towards member state governments that are soon to face their domestic electorate. Moreover, Schneider (2018) also demonstrates that this significant and non-random incremental receipt in budget shares is greatest in the case of those member states whose populations are less supportive of the EU project. This EU-level responsiveness (Schneider, 2018) to domestic concerns and attitudes towards the community project is an example of EU elite’s belief and practice in the support-buying nature of supranational

public transfers, which assumes that signalling the availability and distribution of public funds will induce rational voters to increase their support for the distributor (Levitt & Snyder, 1997), in this case, the EU.²

The empirical evidence on whether this strategy is effective at increasing support for the European project remains ambiguous. Some authors have found that EU net transfers have no significant effect on public support for the EU (Eichenberg & Dalton, 1993; Hobolt, 2012), while others have found positive correlations (Carrubba, 1997; Hooghe & Marks, 2005; Karp et al., 2012). These contrasting findings might be explained by the fact that individual characteristics appear to condition the effect of net transfers on EU support. As Hobolt (2012) points out, many citizens may simply be unaware of the net transfers or unable to interpret its size in relation to the national budget or other countries' receipts. Chalmers and Dellmuth (2015) also find evidence that the effect of transfers on support is conditioned by an individual's level of political awareness, education and a European communal identity. They find no direct effect of transfers on support. These conditional effects reflect findings that support for the EU is generally higher amongst the more educated (Hakhverdian et al., 2015; Hobolt, 2012), who are more knowledgeable about the policy-making processes of the EU. For the majority of citizens, the EU remains relatively distant from their daily lives, a complex multilevel governance system which dilutes responsibility between individual institutions (Hobolt & Tilley, 2014). Knowledge about the EU compared to national systems is generally low (Sanders, 2003), although this has increased during the Eurozone crisis, in part, because of the EU's increased oversight and control over economically intervened-in member states (Ruiz-Rufino & Alonso, 2017). And, as Schneider (2018) also demonstrates, shifts in domestic approval ratings are indeed significantly influenced by state government's ability to "bring home the bacon" from EU (p.140) evincing that domestic voters are very much aware of the redistributive spending commitments that the EU signs up for.

²Domestic governments' attempts to leverage the economic distribution of public resources in a manner that systematically benefits their electoral changes - e.g., spending more in incumbent-led or marginal constituencies - by 'buying' electoral support, is not an uncommon practice (Dahlberg & Johansson, 2002; Hanretty, 2021; Tavits, 2009).

Related to net transfers, the relationship between the EU Cohesion Policy and public support has also received considerable scholarly attention. Country-level studies on the ability of Cohesion Policy receipts' ability to shape public support for the EU have, however, produced mixed results. Becker et al. (2017) shows that EU Cohesion Policy did not influence the levels of EU support in British constituencies before Brexit, while Crescenzi et al. (2020) show that EU structural funds only increase citizens' support for the EU if they go hand in hand with an improvement of local labour market conditions. Bachtrögl and Oberhofer (2018) find that an increase in the average employment growth effect of EU Cohesion Policy on firms within a region in France significantly reduced the vote share of the eurosceptic Marine Le Pen. Likewise, Albanese et al. (2022) present evidence that transfers through the EU Cohesion Policy lead to reduced voting for populist parties in Italy. Although the country-level findings have not produced any clear trends, the majority of cross-country analyses have shown that the EU Structural Policy does improve public attitudes towards the EU (Borin et al., 2015; Rodríguez-Pose & Dijkstra, 2021). Once again this effect may vary depending on individual characteristics such as education and income which increase the effect of Structural Funds on support and also depend on whether citizens directly benefit from the funds, for instance through cash transfers or only indirectly through infrastructural investments (Osterloh, 2011).

The COVID relief *Next Generation EU* fund passed in July 2020 can be understood as a more extensive, one-off payment similar to the EU Cohesion Policy. Citizens in countries in receipt of these payments, especially those countries most negatively affected by the COVID-induced economic shock, should see significant increases in public support for the EU after the *announcement* of the EU's ambitious stimulus spending. Our concrete case of the *Next Generation EU* COVID relief funds adds to the literature on EU Cohesion Policy for two reasons. First, we seek to understand the effect of the public and widely reported-on EU-level *commitment* to redistribute funds rather than their actual receipt. Given the high-intensity media coverage of the announcement, we view the communication of incoming funds as significant given that it is clearly attributable to the EU itself, whereas any eventual regional developments that are indeed funded by the fiscal

transfer may, as a result of elite-level action (Hobolt & Tilley, 2014), be perceived as domestic as opposed to EU successes. Thus the COVID relief funds were a highly salient announcement at a time when citizens were looking towards political elites for economic remedies, and present a unique case where the effects of *pledges* to provide fiscal boosts can be assessed. Moreover, the prioritisation of growth via the distribution of grants *and* loans, as opposed to the conditionality loans of the Eurozone crises which *reduced* political support (Devine & Turnbull-Dugarte, 2021), clearly signals the expectation of a positive effect.

While the literature indicates that the signal of incoming economic benefits resulting from the *Next Generation EU* fund announcement are likely to improve public support, the inconsistency of the existing findings on EU transfers and Cohesion Policy spending gives reason to doubt that economic factors are the only factors influencing public support for the EU. Other factors commonly related to public support such as nationalism and social identity (Halikiopoulou et al., 2012; Hooghe & Marks, 2005; McLaren, 2002) will also likely interact with the announcement of the relief fund. Since individuals with lower levels of attachment to their nation and higher levels of attachment to the over-arching European community tend to have higher levels of support for the EU (Carey, 2002; Hobolt, 2012; McLaren, 2002), we can expect these citizens' increase in support for the EU to be greater following the announcement of the *Next Generation EU* fund.

Hypotheses

We present two hypotheses. First, building on the assumption that individuals' trust and support in institutions and actors can react rationally and thermostatically to information signals regarding pledges of incoming economic transfers (Levitt & Snyder, 1997; Tavits, 2009), as well as concrete literature on the role of economic calculus in determining support for the EU and concrete EU policies (Eichenberg & Dalton, 1993; Hobolt & Wratil, 2015; Vasilopoulou & Talving, 2020), our first expectation is that respondents exposed to news about the *Next Generation EU* fund will become more supportive of the

EU.

H1: *Eurobarometer respondents interviewed immediately after the announcement of the EU coronavirus bailout funds (July 21st 2020) will be more supportive of the EU than those interviewed immediately before*

Second, we identify theoretical argument to expect between states asymmetries in the effects of the funding pledges. The economic shocks that resulted from the state-mandated lockdown measures triggered by the high rates of contagion of the COVID-19 virus have not been symmetrical for the members states of the European community. As a result individuals in those countries experiencing poor(er) economic performance vis-à-vis than their EU peers are more likely to response positively to the announcement of the rescue package given that the necessity, and resulting economic benefit, of the funds will be greater(er). There was also notable cross-state variation in elite-level preferences regarding what the deal should look like (Bauhr & Charron, 2022) with elites in some wealthier members states – e.g., the so-called *Frugal Four* – negatively cuing citizens on the desirability of within-EU fiscal transfers. Relatedly, signals of EU-level outputs may have asymmetric effects on populations with varying levels of existing euroscepticism: positive EU-level outputs may exhibit a larger alleviating effect on trust in the supranational polity in more eurosceptic states especially given, as Armingeon et al. (2022) demonstrates, that that the distribution of the *NGEU* was more charitable to eurosceptic states. Our second hypothesis, therefore, anticipates country-level³ asymmetries:

H2: (a) *The increased support for the EU among the treatment group will be greater among those respondents who live in EU member states whose economy was most negatively affected by the coronavirus pandemic.* (b) *The increased support for the EU among the treatment group will be greater among those respondents who live in EU member states with higher levels of euroscepticism.*

³Individual-level asymmetries based on education and political interest (Turnbull-Dugarte, 2020), pre-existing attachment to the supranational polity (Chalmers & Dellmuth, 2015), as well as ideological left-right placement (a proxy in the absence of partisanship which may be a better test elite cues (Bauhr & Charron, 2022; Stoeckel & Kuhn, 2018)) are also considered and are reported in the appendix material.

4 Research Design

To test these hypotheses, we use a quasi-experimental identification strategy leveraging the discontinuity during survey fieldwork approach detailed by Muñoz et al. (2020). Fieldwork for Eurobarometer 93.1 took place between July 9th and August 26th 2020. The last-minute announcement of the deal resulting in the €750bn COVID relief package occurred on July 21st, providing us with naturally occurring discontinuity with *as good as random* assignment to one of two treatment conditions: i) control (respondents interviewed before agreed upon details of EU relief funds were announced), and ii) treatment (respondents interviewed after the agreed upon details of the rescue package were announced).

This identification strategy assumes that respondent allocation to either side of the treatment threshold coincides with a significant discontinuity in the information respondents are exposed to, with the treated being exposed to more information than those before the announcement. Individuals in the control group had expectations about a future EU-level response to the COVID-19 crisis, but only those after the announcement could know about the details of the agree upon deal or of the specific components (such as the balance of payable and non-repayable funds). Observing popular interest in the EU deal over time using Google Trends data (visualised in appendix figure 6), we observe a significant and positive discontinuity in interest that coincides with the threshold date. Moreover, as we detail, the announcement did update respondents' *economic expectations*, suggesting that, along with interest, there was relevant attitudinal updating.

The quasi-experimental design we leverage facilitates a high level of ecological validity. Our identification strategy relies on naturally occurring exposure to political information signals occurring in the field without the potential for the behaviour-priming bias of researcher-imposed manipulation that may occur in synthetic treatments in survey experiments. In other words, those respondents assigned to treatment are exposed to *organic* stimuli - i.e., exposure to news and/or public discussion occurring naturally within

a respondents typical media diet.

Outcome

We consider a rich catalogue of measures of political support. Our primary outcome variable of interest is trust in the EU. The Eurobarometer records trust dichotomously with respondents asked⁴ to indicate if they tend to trust (1) or not (0) different institutions. In addition to modelling the effects the *Next Generation EU* on trust with the EU - an outcome we view to be indicative of concrete support for the EU as a polity - we also model effects of treatment on satisfaction with EU democracy, as well as a battery of alternative outcome measures such as DeVries (2018)'s EU differential⁵ and economic expectations.

Treatment

Treatment, based on the naturally occurring random assignment, is operationalised as:

$T_i = 0$ (control) if respondent i interviewed before Tuesday July 21st 2020

$T_i = 1$ (treatment) if respondent i interviewed on or after Tuesday July 21st 2020

The member state-level distribution of treatment assignment - in those member states with individuals on either side of the treatment threshold - is summarised in Table 8 and the distribution of respondents across the full fieldwork period is summarised in Figure 9. Of the total sample, 59.2% ($N = 14563$) are assigned to the control condition, with 40.8% ($N=10046$) assigned to treatment.

⁴Question wording: "I would like to ask you a question about how much trust you have in certain institutions. For each of the following institutions, please tell me if you tend to trust it [1] or tend not to trust it [0]."

⁵The EU differential measures support for the EU *relative* to support for the domestic political system, with higher (lower) differential values indicating that citizens have a greater (lesser) opinion of the EU than their national government. Following DeVries (2018), we define the EU differential as $Trust_{EU} - Trust_{MemberState}$.

Moderators

To test **H2** we estimate the moderating effect of two country-level indicators: economic performance (a), and country-level euroscepticism (b). We operationalise economic performance via two distinct approaches. First, we dichotomise member states into those with above average and below average GDP growth (see DeVries (2018) for a similar dichotomous approach). Second, we operationalise economic performance in relation to the economic shock triggered by the events of COVID-19. Specifically we create a measure which indicates the deviation in GDP growth between 2020 and the mean level from the three-year period (2017-2019) before the pandemic hit Europe in 2020. Using this metric, we identify Malta and Spain as those countries whose GDP growth suffered the largest divergence from the pre-COVID mean equal to 14.2 and 13.6, respectively. We operationalise country-level euroscepticism as the vote share of eurosceptic parties in the European Parliament elections of 2019 (one year prior to the events of COVID). Data on party classifications as eurosceptic and corresponding vote shares is sourced from the 2019 iteration of the Chapel Hill Expert Survey (Jolly et al., 2022).

Model

The model specification for testing **H1** is summarised in the basic linear Model 1) in which $\beta_1 Treatment_i$ is the effect of treatment assignment (ITT), γX_i is a vector of individual-level covariates, θ_c are country fixed-effects, and ϵ_i is the error term:

$$Y_i = \alpha + \beta_1 Treatment_i + \gamma X_i + \theta_c + \epsilon_i \quad (1)$$

$\beta_1 Treatment_i$ indicates the treatment effect after considering potential pre-treatment covariates. The vector of controls, γX_i , are limited to those covariates that we can con-

fidently assume are not causally posterior to treatment assignment and include: gender, age, education and political interest. To test of **H2**, we replicate 1 to include a multiplicative interaction term between $\beta_1 Treatment_i$ and a country’s economic shock from COVID-19 ($\beta_2 CovidShock_c$) and euroscepticism ($\beta_3 Euroscepticism_{c,t-1}$).

5 Results

Before presenting the results of our modelling approach, we visualise the mean level of trust, by treatment condition, in each member states included in our analysis. As reported in the choropleth map visualised in Figure 1, the mean country-level trust in the EU shows little descriptive evidence of any treatment-induced variation in trust in the supranational polity. A descriptive comparison visualising DeVries (2018)’s EU differential report similar levels of stability on either side of the treatment threshold (see appendix Figure 5). So did the announcement of the EU’s unprecedented COVID relief funds really have *no* significant effect on political support for the EU?

The results for the modelled effect of the announcement of COVID relief funds on trust in the EU (H1) are shown in Figure 2, which shows mean level of trust by treatment condition for the full sample and for those whose economy was below or above average (H2a). The results indicate that there was not a significant effect of the relief funds on trust in the EU, rejecting H1. Whilst, on average, the modelled ITT for the full sample is negative (i.e., in the opposite direction from that theorised), it is of minimal magnitude ($\beta=-.007$) and insignificant ($p=.43$). There is an even stronger null for countries that experienced worse than average GDP growth in 2020 ($\beta=.001$) and the modelled effect is negative ($\beta=-.016$), yet still insignificant ($p=.07$), for those that had a ‘better’ than average pandemic. Bearing in mind that the outcome is binary, an ITT of $-.016$ represents an approximately 1.75% decrease in trust, a trivial change. There is no difference between member state economies with above and below average economic performance, rejecting H2.

Figure 1: Member state trust in the EU by treatment condition

These initial results suggest that, on average, the as good as random exposure to the positive news story regarding the EU’s COVID-19 relief played no significant role in shifting Europeans’ levels of trust in the EU. In Figure 3 we demonstrate that this null effect is not conditioned by the severity of the economic shock facing member states as a result of COVID-19, nor by the levels of pre-existing euroscepticism. Given the announced *Next Generation EU* funding would asymmetrically benefit countries with higher euroscepticism (Armingeon et al., 2022), these publics may be more primed to react to news of the incoming pledged transfers.

Looking at the moderating effect of the economic shock of COVID-19, whilst it is clear that in those states hit hardest (economically) by the events of the pandemic, trust in the EU is significantly lower (note the significant negative slope: $\beta = -.02$)⁶, there is

⁶Substantively, a 1 percentage-point *increase* in deviation in GDP growth caused by COVID is associated with a 2 percentage-point *decrease* in the probability that an individual tends to trust the EU.

(Null) effects of EU's Covid-19 rescue package announcement

Trust in the EU (0-1)

Controlling for age, education, sex, political interest and country fixed effects
Data from the Eurobarometer. Weights (EU27) applied.

Figure 2: Estimated null effects

no significant variation in the relationship for treated and untreated respondents. Our distinct empirical approaches of modelling H2 provide the same evidence in support of the same conclusion: regardless of the economic performance of the member state, individuals' exposure to news regarding the unprecedented levels of financial assistance had no influence of substance or significance on individuals' levels of trust in the EU.

5.1 Support beyond trust

In 4 we consider the effects on a full range of viable outcomes. We select satisfaction with EU democracy, DeVries (2018)'s EU differential and satisfaction with the EU coronavirus measures (left-hand panel), which is directly related to treatment, and two indicators for economic expectations, at the national and European levels (right-hand panel). The results in Figure 4 show that there are no significant positive effects for satisfaction with the EU or the EU's COVID-19 policies - indeed, if anything, there is a negative effect

Figure 3: (Un)conditionality of null effects

- but that there are positive effects for economic expectations depending on how the country had fared economically up until that point.

For those countries that had below average GDP growth, announcement of the funds is significantly related to improved economic expectations at both the national and EU levels; for those that had fared relatively better, the effect was almost a mirror opposite. That said, with a maximum effect of 0.075, this represents only a 2.5% change on the 3-point response scale.

Overall, our conclusion is that the announcement about COVID relief funds failed to shift support for the EU across any plausible measure. In the Appendix, our robustness tests include randomisation inference, changes to bandwidth dates (restricting the sample to $\pm$ a number of days either side of treatment), and placebo dates. In all cases, there is no evidence in the data of an effect of the announcement of the COVID relief funds on trust in the EU. Indeed, our randomisation inference suggests that the effect on countries that had above average GDP performance is less significant than our main estimates ($p = 0.105$ rather than $p = 0.089$). We also tested whether there were differential effects on key individual-level characteristics: education, attachment to the EU, political interest, and left-right placement. These are chosen since they may signal the likelihood of ‘treatment compliance’ (education, interest) and differential responsibility attribution (attachment

Figure 4: Null ITT effects on alternative outcome measures

and ideology). These results are shown in appendix Figure 14. The quasi-experimental design we rely present two potential threats to identification: i) *conditional ignorability*, and ii) *excludability*. We reject these threats via a series of empirical tests of covariate balance, item non-response, attrition, pre-existing temporal trends, and parallel treatment, available in the Appendix.

6 Conclusion

The EU’s COVID-19 financial relief fund, finalised and announced on July 21st 2020, is the largest supranational transfer of financial resources in the polity’s history. It represents a significant step towards the EU’s ambition of “ever closer union” via fiscal solidarity and debt mutualisation. We followed existing utilitarian theories of public support for the EU by hypothesising that such a large, mostly unconditional, fiscal transfer would

increase public support for the EU. We tested this through a quasi-experimental design in which the surprise announcement of the COVID relief funds occurred during survey fieldwork, randomly sorting respondents into treatment and control groups.

Against our prior expectations and that of the dominant literature - and, not least, the intent of the EU (Armingeon et al., 2022) - we find no evidence that the EU announcement of its COVID-19 relief funds resulted in increased support for the international organisation. We find almost no moderation effects depending on individual variables such as education, EU identity, the severity of the crisis for the respondents' country, or the national level of euroscepticism. These results are consistent with Hobolt (2012) and Becker et al. (2017) who find little evidence that the EU can "buy" support. Whilst Hobolt (2012) suggests the null effect of net transfers may be a product of individuals simply not being aware of these fiscal transfers, we do not view this as a valid explanation for the null effects we find in the case of the widely reported and politically salient announcement of the EU's deal on COVID-19 relief funds. Individuals *did* update their economic expectations in response to the funds, which suggests that the information treatment on the incoming stimulus did update individuals' attitudes in a theoretical congruent direction, but this did not, in turn, translate into increased support for the EU. It is important to note that the treatment effect we estimate is of the *announcement* of incoming EU funds and the *promise* of cross-national redistribution. Importantly, however, similar quasi-experimental work has demonstrated that *announcement's* of EU-level fiscal intervention can indeed shape preferences: Devine and Turnbull-Dugarte (2021) show, for example, that EU economic intervention in Portugal at the height of the Eurozone crisis had a negative causal impact on both economic evaluations and political support. Future work may well seek to assess the potential trust-inducing role of these EU funds when local investment actually arrives. In line with the empirical findings we present here, and the literature of the (limited) efficacy of EU spending (Hobolt, 2012) and Cohesion funds in driving EU support (Becker et al., 2017), our expectation, however, is that the *Next Generation EU* spending receipts will do little to rally support around the supranational polity.

This is quite problematic for the EU's ability to drive public support. The COVID relief funds were perhaps a most likely case: a high profile, substantial transfer of funds, but one that was barely a ripple in the sea for citizens' attitudes. If the EU cannot drive support through such transfers, EU support is likely instead reliant on 'input' factors (Føllesdal, 2006; Moravcsik, 2002) and concerns about sovereignty (Halikiopoulou et al., 2012), identity-based determinants of support (Hooghe & Marks, 2005; McLaren, 2002) and the political strategies of domestic elites (DeVries & Edwards, 2009). Yet the EU is unlikely to be able to demonstrate its democratic qualities in the near future, and political elites have strong incentives to shift blame and claim credit to the detriment of the EU (Hobolt & Tilley, 2014). The EU faces incentives to rally support among a disenchanted public. Unfortunately, attempting to rally support by leveraging discretionary spending has, so far, failed to be the silver bullet against low support that EU may have hoped for.

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Appendix

Can the EU garner political support by
announcing financial stimulus? No

A Variable operationalisation and summary statistics

Variable	Operationalisation
Education	Derived from variable ‘d8r1’. We code those leaving full-time education before 18 and with no full-time education as as having ‘Low/No’, 19-21 years as ‘Medium’ and more than 22 years as ‘High’. ‘Still studying’ is categorised as such. Refusal and DKs are dropped.
Interest	Derived from variables ‘polintr_*’. We coded polintr_1 == 1 as ‘High’, polintr_2 == 1 as ‘Med’, polintr_3 == 1 as ‘Low’ and polintr_4 == 1 as ‘Not at all’.
Age	Identical to ‘d11’. A continuous variable measuring ‘exact age’ of respondent.
Sex	Derived from ‘d10’. We coded 1 as ‘Man’ and 2 as ‘Woman’. Those declaring ‘none of the above’, non-binary, or not recognised were dropped (N = 4).
Trust institutions	Derived from ‘qa6a_*’ where ‘tend to trust’ == 1 and ‘tend not to trust’ == 0.
Trust differential	trust EU minus trust national government. If positive, means respondent trusts EU more
EU measures	derived from ‘qa21a_2’ where 1 == ‘Very satisfied’, 2 == ‘fairly satisfied’, 3 == ‘Rather not satisfied’ and 4 == ‘Not at all satisfied’
Economic expectations	Derived from ‘qa2a_3’ (national) and ‘qa2a_7’ (EU) where 1 == ‘Better’, 2 == ‘Worse’ and 3 == ‘Same’.
Attachment to EU	derived from ‘qc1a_3’ where 1 == ‘Very’, 2 == ‘Fairly’, 3 == ‘Not very’ and 4 == ‘Not at all’ attached
Democratic satisfaction	derived from ‘sd18b’ (EU) and ‘sd18a’ (national), where 1 == ‘Very’, 2 == ‘Fairly’, 3 == ‘Not very’ and 4 == ‘Not at all’ satisfied.

Table 1: Operationalisation of variables from the Eurobarometer

Table 2: Summary statistics (Categorical)

		N	%
Education	Low/No	4589	18.6
	Med	9102	37.0
	High	2271	9.2
	Still studying	1445	5.9
Sex	Man	11149	45.3
	Woman	13460	54.7
Political interest	Not at all	3605	14.6
	Low	4025	16.4
	Med	12244	49.8
	High	4735	19.2
EU Covid measures	Not at all satisfied	2277	9.3
	Rather not satisfied	7202	29.3
	Fairly satisfied	10087	41.0
	Very satisfied	2046	8.3
Economic expectations	Worse	11185	45.5
	Same	7381	30.0
	Better	4649	18.9
EU economic expectations	Worse	9824	39.9
	Same	8056	32.7
	Better	4012	16.3
Attachment to EU	Not at all attached	2646	10.8
	Not very attached	7461	30.3
	Fairly attached	9811	39.9
	Very attached	3818	15.5
Satisfaction with democracy	Not at all satisfied	2698	11.0
	Not very satisfied	6750	27.4
	Fairly satisfied	11997	48.8
	Very satisfied	2480	10.1
Satisfaction with EU democracy	Not at all satisfied	2035	8.3
	Not very satisfied	7582	30.8
	Fairly satisfied	12403	50.4
	Very satisfied	1172	4.8

Table 3: Summary statistics (Continuous)

	Unique (#)	Missing (%)	Mean	SD	Min	Median	Max
Trust in EU	3	11	0.5	0.5	0.0	1.0	1.0
Trust in Govt	3	6	0.5	0.5	0.0	0.0	1.0
Trust in UN	3	15	0.5	0.5	0.0	1.0	1.0
Trust in Politicians	3	5	0.7	0.4	0.0	1.0	1.0
Trust differential	4	13	0.1	0.5	-1.0	0.0	1.0
Age	83	0	51.4	18.0	15.0	53.0	99.0

Figure 5: Member state EU differential (DeVries, 2018) by treatment condition

B Google trends data

Figure 6: Popular salience of EU deal

C Full tables

Table 4: Main regression output for trust in the EU

	All countries	Below average GDP	Above average GDP
COVID funds	-0.007 0.009 (0.433)	0.001 0.016 (0.931)	-0.016 0.009 (0.070)
Observations	14583	4387	10196
R2	0.080	0.074	0.058
R2 Adj.	0.078	0.071	0.055
AIC	29879.5	7874.8	21185.7
BIC	30129.9	7977.0	21380.9
Log.Lik.	-14906.752	-3921.397	-10565.860
Std.Errors	by: isocentry	by: isocentry	by: isocentry

Table 5: Models with individual-level interactions with relief funds announcement

	Political interest			Education			EU attachment		
	All countries	Below average GDP	Above average GDP	All countries	Below average GDP	Above average GDP	All countries	Below average GDP	Above average GDP
Intervention	0.026 (0.020)	0.003 (0.030)	0.071* (0.030)	-0.041** (0.015)	-0.012 (0.027)	-0.047* (0.019)	0.009 (0.021)	0.061+ (0.035)	-0.040 (0.028)
Intervention * Interest (Low)	-0.051+ (0.028)	-0.026 (0.046)	-0.091* (0.038)						
Intervention * Interest (Med)	-0.048* (0.023)	-0.009 (0.039)	-0.112*** (0.033)						
Intervention * Interest (High)	-0.005 (0.028)	0.051 (0.052)	-0.059 (0.037)						
Intervention * Education (High)				0.074** (0.027)	0.020 (0.048)	0.080* (0.034)			
Intervention * Education (Med)				0.029 (0.019)	-0.003 (0.037)	0.026 (0.023)			
Intervention * Education (Still studying)				0.112*** (0.030)	0.113* (0.056)	0.091* (0.037)			
Intervention * EU Attachment (Fairly)							-0.002 (0.024)	-0.067 (0.042)	0.045 (0.031)
Intervention * EU Attachment (Not very)							-0.010 (0.025)	-0.092* (0.043)	0.066* (0.032)
Intervention * EU Attachment (Very)							0.031 (0.029)	0.101+ (0.057)	0.026 (0.036)
Observations	14583	4387	10196	14583	4387	10196	14425	4343	10082

Country-clustered robust standard errors

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

D Threats to identification

The quasi-experimental design presents two distinct threats to identification. The first threat is *conditional ignorability*. Conditional ignorability assumes i) exogenous assignment to treatment independent of respondents' potential outcomes and individual-level covariates, and ii) independent rates of non-response. We reject this through two tests. First we compare the covariate balance between the control and treatment groups across a vector of observable covariates. Assignment is unrelated to sex, medium and high levels of political interest, and age. It is related, however, to low education and low political interest. Despite this, these are not to worryingly levels (see Figure 11), and we address them through appropriate matching methods detailed in the appendix.

Second, we model rates of non-response on the primary outcome measures dependent on treatment assignment. Attrition is not dependent on treatment assignment so we can be confident that the potential exposure to stimuli relating to the EU response funds is not driving or depressing missingness in the outcome measures.

The second threat to identification is *excludability*. The excludability criteria assumes that observed effects are not the result of unobserved confounders. In the case of the "unexpected event" design (Muñoz et al., 2020), there are two potential confounders: pre-existing temporal trends and parallel treatment.

To address the potential presence of pre-existing temporal trends, we carry out two tests. First, we model alternative dates and bandwidths of treatment (in other words, reducing the sample to $\pm$ a certain number of days from treatment). Our *placebo* dates are the median date of the control group and median date of treated group, and alternative bandwidths are $\pm$ 10, 5, and 15 days. Appendix Table 7 shows that these sensitivity tests confirm our reported null results in all conditions. Second, we conduct randomisation inference tests on the effect of the Funds news - see visualisation in Figure 7. The results of 2000 permutations of randomly allocated treatment dates, consistent with our claims, indicate the null results we report for the full sample and below-average performing

Table 6: Randomisation inference coefficients

	All	Below	Above	All (RI)	Below (RI)	Above (RI)
Treatment	-0.007 (0.441)	0.001 (0.934)	-0.016 (0.089)	-0.007 (0.406)	0.001 (0.917)	-0.016 (0.105)
Num.Reps				2000	2000	2000

p-values shown in parentheses.

countries are robust and that the negative results for citizens in above-average performing states may be spurious. In considering parallel treatment, we demonstrate that the null effects replicate across a number of placebo items which we assume to be theoretically independent of treatment (e.g. trust in the police or the UN) as reported in Figure 12.

D.1 Randomisation inference, bandwidth changes and placebo tests

We conduct randomisation inference tests on the effect of the Funds on trust in the EU, with 2000 permutations. This randomises the treatment date, not just the ‘real’ treatment date we have selected. This is visualised in figure 7 for the three country samples, with the vertical red line indicating the estimated treatment effect in the main text. Table 6 displays treatment effects and p-values from the main estimations and the randomisation inference estimations for the three country groups. Results are consistent with a null effect, with p-values substantially above 0.05. The randomisation tests indicate lower significance for the treatment within above-average GDP countries than our main estimation does.

In a similar vein, we also provide alternative dates and bandwidths of treatment (in other words, reducing the sample to $\pm$ a certain number of days from treatment). This is for all countries. Our selected dates are the median date of the control group and median date of treated group, and alternative bandwidths are $\pm$ 10, 5, and 15 days. Table 7 shows that there are null results in all conditions.

Table 7: Models with placebo and bandwidth changes

	Median (Control)	Median (Treated)	Bandwidth: 10 days	Bandwidth: 5 days	Bandwidth: 15 days
Median (Control)	0.012 (0.012)				
Median (Treated)		-0.027 (0.014)			
Treatment			0.001 (0.009)	-0.012 (0.010)	-0.006 (0.008)
Observations	14583	14583	13007	6986	14306

Figure 8 also shows the effect of the announcement on trust in government, police and the United Nations alongside the EU. This indicates that the effect of the announcement on trust in the EU is equivalent in size and significance to the ‘effect’ on trust in the police and UN, two outcomes that we would not expect to be influenced by the COVID relief funds.

Figure 7: Randomisation inference tests for the effect of announcement of COVID funds on trust in the EU

Figure 8: Effect of announcement on trust measures including police and United Nations

D.2 Balance and matching

Figure 9 shows the sample size by country and fieldwork date. This shows that the bulk of fieldwork was occurring around the announcement date (day 18). The only fieldwork occurring after day 30 was in Ireland.

Figure 9: Sample distribution over fieldwork period

Our initial exploration of covariate balance between the treatment and control (figure

Table 8: N of treated and control by country

Country		Control	Treated	All
AT	N	646	364	1010
	% row	64.0	36.0	100.0
BE	N	708	300	1008
	% row	70.2	29.8	100.0
BG	N	721	336	1057
	% row	68.2	31.8	100.0
CY	N	610	395	1005
	% row	60.7	39.3	100.0
CZ	N	775	234	1009
	% row	76.8	23.2	100.0
DE	N	745	769	1514
	% row	49.2	50.8	100.0
DK	N	465	525	990
	% row	47.0	53.0	100.0
ES	N	865	147	1012
	% row	85.5	14.5	100.0
FI	N	461	567	1028
	% row	44.8	55.2	100.0
FR	N	736	270	1006
	% row	73.2	26.8	100.0
HR	N	540	490	1030
	% row	52.4	47.6	100.0
HU	N	422	637	1059
	% row	39.8	60.2	100.0
IE	N	168	837	1005
	% row	16.7	83.3	100.0
IT	N	669	358	1027
	% row	65.1	34.9	100.0
LT	N	561	444	1005
	% row	55.8	44.2	100.0
LV	N	583	413	996
	% row	58.5	41.5	100.0
MT	N	337	165	502
	% row	67.1	32.9	100.0
NL	N	799	205	1004
	% row	79.6	20.4	100.0
PL	N	739	292	1031
	% row	71.7	28.3	100.0
PT	N	712	344	1056
	% row	67.4	32.6	100.0
RO	N	499	613	1112
	% row	44.9	55.1	100.0
SE	N	729	325	1054
	% row	69.2	30.8	100.0
SI	N	819	193	1012
	% row	80.9	19.1	100.0
SK	N	^{xv} 254	823	1077
	% row	23.6	76.4	100.0
All	N	14563	10046	24609
	% row	59.2	40.8	100.0

10 and table 9) shows imbalance across the predictors, except for sex. This is the case even five days either side of the announcement (table 10). To address this, we turn to matching methods. Matching methods are designed to reduce or eliminate covariate imbalance between control and treatment groups, allowing a more accurate assessment of the effect of a binary treatment. We do this using *MatchIt* in R.

Figure 10: Balance model plot

Importantly, the assessment of covariate imbalance using this package does not indicate

Table 9: Models predicting treatment assignment

	All countries	Below average	Above average
Education (Med)	-0.018* (0.009)	-0.049** (0.016)	0.007 (0.011)
Education (High)	0.006 (0.013)	-0.002 (0.022)	0.009 (0.016)
Education (Studying)	-0.081*** (0.016)	-0.095*** (0.027)	-0.077*** (0.020)
Woman	-0.001 (0.007)	0.005 (0.013)	-0.006 (0.009)
Interest (Low)	-0.058*** (0.012)	-0.072*** (0.018)	-0.030+ (0.017)
Interest (Medium)	-0.043*** (0.010)	-0.057*** (0.016)	-0.017 (0.015)
Interest (High)	-0.043*** (0.013)	-0.023 (0.023)	-0.040* (0.017)
Age	0.000* (0.000)	-0.001* (0.000)	0.000 (0.000)
Num.Obs.	16225	4863	11362
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$			

serious imbalance. The standardised mean difference (SMD) can be used to indicate whether there is imbalance, in which values closer to zero are better, and lower than 0.05 or 0.1 are acceptable. In our demographics, the largest SMD is 0.079 (political interest). Of more importance is covariate balance in countries: these indicate large imbalances for some countries (e.g 0.4 for Spain). As such, we interpret the imbalances indicated in figure 10 and tables 9 and 10 as outcomes of country inclusion rather than demographics per se.

To mitigate against this problem, we use full matching, which matches every unit to at least one control or treated unit, with the propensity score estimated using logistic regression. No observations were lost during matching. After matching, only one country (Belgium) had a higher than acceptable SMD (-0.17). The SMDs are displayed in figure 11 which compares matched and unmatched data, with a vertical line indicating 0.05 (the acceptable level of mean difference).

In table 11, we show the estimation of the effect of COVID funds on this matched data.

Table 10: Models predicting treatment assignment, changed bandwidth

	5 days	10 days	15 days
Education (Med)	-0.003 (0.014)	-0.016 (0.010)	-0.019* (0.009)
Education (High)	0.013 (0.020)	0.009 (0.013)	0.007 (0.013)
Education (Studying)	-0.059* (0.025)	-0.073*** (0.017)	-0.085*** (0.016)
Woman	0.010 (0.011)	0.001 (0.008)	-0.001 (0.007)
Interest (Low)	-0.078*** (0.018)	-0.053*** (0.012)	-0.059*** (0.012)
Interest (Medium)	-0.058*** (0.016)	-0.041*** (0.011)	-0.043*** (0.010)
Interest (High)	-0.044* (0.020)	-0.039** (0.014)	-0.046*** (0.013)
Age	0.000 (0.000)	0.000 (0.000)	0.000* (0.000)
Num.Obs.	7781	14447	15937
+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001			

The coefficient is negatively signed at -0.025 and significant at the $p = 0.1$ level. This is larger than our initial estimate (-0.006), but still relatively trivial. Given relatively low significant (i.e. > 0.05) and the still small effect, we do not think this changes our core conclusion; however, it does indicate that if there is an effect, it is likely negative in direction.

Table 11: Model on matched data

	Model 1
COVID funds	-0.025+ (0.015)
Education (Med)	0.059* (0.024)
Education (High)	0.100*** (0.029)
Education (Studying)	0.219*** (0.033)
Woman	0.010 (0.017)
Interest (Low)	0.028 (0.029)
Interest (Medium)	0.084** (0.027)
Interest (High)	0.097** (0.030)
Age	-0.001 (0.001)
Num.Obs.	11269
+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Figure 11: ‘Love plot’ model for matched data

D.3 Attrition

In Table 12 we assess the potential influences of treatment assignment on item non-response. Our test, which models the propensity of an individual to respond "Don't know" or not respond to the Eurobarometer survey item on trust in the EU as a function of treatment, shows treatment had no effect on response rates to the core outcome measure.

Table 12: Models predicting non-response on EU trust measure

	All countries	Below average GDP	Above average GDP
Treatment	0.005 (0.005)	0.004 (0.010)	0.005 (0.007)
Num.Obs.	16225	4863	11362
R2	0.031	0.030	0.032
R2 Adj.	0.029	0.027	0.030
AIC	18202.3	4949.9	11170.0
BIC	18456.2	5053.7	11368.1
RMSE	0.29	0.29	0.29

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

D.4 Placebo tests

Figure 12: Placebo outcome measures

D.5 Regression discontinuity (RD)

We conduct a range of RD design (RDD) tests which identifies the local effect of the announcement around the cut-off date. We do this using the *rdlocrand* package in R, which uses randomisation inference, as in Appendix A. We do this for the sample of all countries.

We set the window to ± 10 and 5 days and 1000 replications. The results are in table 13. They show null results ($\beta = [-0.01, 0.01]$, $p = [0.25, 0.93]$) for both window selections and test statistics.

window	method	estimate	pval
± 10 days	Diff in means	0.01	0.25
	Kolmogorov-Smirnov	0.01	0.90
± 5 days	Diff in means	-0.01	0.28
	Kolmogorov-Smirnov	0.01	0.93

Table 13: Regression discontinuity with randomisation inference

A descriptive representation of the (lack of) discontinuity is provided in Figure 13 which displays the average daily mean level of trust in the EU before and after the EU relief funds were announced. The Figure also stratifies the total sample into those state with below and above average performing economies.

E Individual-level moderation effects

Overall, the results show relatively muted interaction effects, except for those that are most attached to the EU in countries with below average GDP growth, amongst whom the announcement increased trust by 16 percentage-points. However, this is not statistically different from those who are not at all attached to the EU in those countries. Similarly for education, the announcement had a small negative effect (0.05) amongst those with low or no education in countries that had above average GDP growth. There were no statistically different results regarding political interest.

Figure 13: Regression discontinuity visualisation

Altogether, these results indicate that the announcement did not have a significant trust-boosting effect. There is some evidence that those who were most attached to the EU had a more positive reaction compared to those who are very attached to the EU, particularly in the hardest-hit countries. There is also some evidence that those with low or no education were more negative compared to those with high education, though those effects are quite small.

Figure 14: Individual-level moderation tests

Figure 15: Individual-level ideological moderation tests