

# Rank-heterogeneous effects of political messages: Evidence from randomized survey experiments testing 59 video treatments

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## Abstract

Central to theories of political persuasion is treatment effect heterogeneity—the idea that people respond to political messages in different ways—so persuasion is easier when different messages are targeted to different audiences. The standard approach to testing for heterogeneity is to examine whether the effect of an individual message differs between subgroups of people (such as liberals versus conservatives). We describe the shortcomings of this approach, and propose an alternative: jointly examining *many* messages on the same political issue, and assessing whether the rank-order of their effects differs between subgroups (which we call “rank-heterogeneity”). Implementing this approach, we conduct two large-scale survey experiments spanning two policy issues, 59 message treatments, and over 40,000 American adults. Across experiments we find mixed evidence of rank-heterogeneity, suggesting that it depends upon the particular issue in question. However, in the case where we do observe strong evidence of rank-heterogeneity, its primary cause is consistent with the predictions of moral reframing theory, an influential account of heterogeneity in political persuasion. Alongside these implications for theory, our results have implications for political persuasion in practice.

**Keywords:** Persuasion, attitudes, heterogeneity, American politics, experiment

## Author note

We thank Jack Blumenau and Alex Coppock for feedback on a previous version of this manuscript. Any errors are ours. COI statement: we are founders of a research organization that conducts public opinion research.

## Introduction

What types of messages persuade people in politics? A fundamental assumption of many theories of persuasion is that the answer depends on the person receiving the message—because different types of people find different types of messages persuasive. As Hornikx and O’Keefe (2009) explain, “[for] centuries, students of persuasion have taken it to be a commonplace that, in order to be effective, persuasive messages should be adapted to the audience” (p.3). Indeed, major theories of political attitude formation posit that variables such as people’s existing attitudes (Lord, Ross, and Lepper 1979; Taber, Cann, and Kucsova 2009; Taber and Lodge 2006), moral values (Feinberg and Willer 2015, 2019) and political and social identities (Kahan 2016; Van Bavel and Pereira 2018), among many others, moderate the extent to which they change their attitudes in response to political messages. Notably, this assumption—that there exists reliable heterogeneity across people in the effects of messages—underpins the logic of targeting different types of messages to different types of people in order to maximize their persuasive impact (Teeney et al. 2021).

The prominence of academic theories of heterogeneity, combined with the apparent prevalence of message-targeting in the political advertising industry (Dobber, Fathaigh, and Borgesius 2019; Privacy International 2020), suggests that there exists substantial heterogeneity across people. In other words, the political message that best persuades one type of person is perhaps *generally unlikely* to be that which best persuades another, different type of person. While this could be the case, in this paper we argue that there is actually limited existing evidence (in the academic literature at least) to speak to this question. This lack of evidence has important implications, both for prominent theories of heterogeneity and for political persuasion in practice.

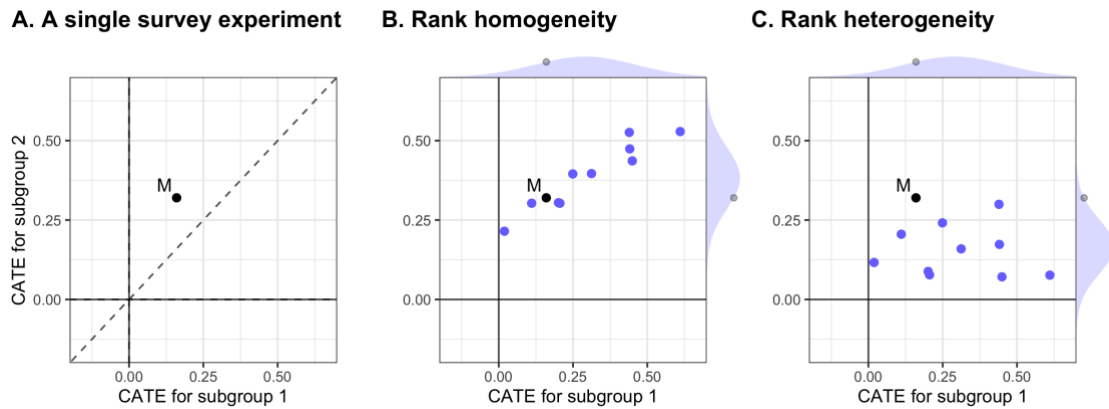
Our argument rests on a simple premise: the most common method of examining heterogeneity is ill-equipped to determine whether or not the political message that best persuades one type of person is that which also best persuades another type of person. Therefore, in this paper we propose an alternative method, and apply it in two large-scale survey experiments spanning two U.S. policy issues, 59 message treatments, and over 40,000 American adults. In doing so, we simultaneously provide a new and comprehensive test of *moral reframing theory*, an influential account of persuasive communication which claims that political messages cause greater attitude change when they are matched to people’s moral values (Feinberg and Willer 2019). In the next section, we lay out our argument in greater detail.

### **Limitations of the common operationalization of heterogeneity**

A common approach for examining heterogeneity in the effects of political messages is to conduct an experiment in which people are randomly exposed to a political message, and then to estimate whether the average effect of the message on people’s attitudes varies across those of different demographics, political identities, moral values, or any number of other covariates. Typically, the inferential threshold used to conclude heterogeneity in such cases is observing an interaction between treatment assignment and the relevant covariate—indicating that the average treatment effect of the political message differs between subgroups of the sample.

While the norm, this operationalization of heterogeneity cannot determine how often the political message that best persuades one type of person is that which also best persuades another type of person. To illustrate why, consider the following case. Imagine that a researcher conducts a survey experiment like that described above, and observes that the average treatment effect of a political message **M** is half as large

among people in subgroup 1 (e.g., liberals) than in subgroup 2 (conservatives). The difference between subgroups is statistically significant—evidence of heterogeneity by the standard interaction effect definition. This result is depicted in Figure 1A.



**Figure 1.** Illustrative example of conditional average treatment effects (CATEs) for a hypothetical set of experiments. The overall distribution of treatment effects for each subgroup is shown at the margins.

Now imagine that the researcher conducts ten further survey experiments, using a different type of political message as the treatment in each. In almost every experiment, they observe that the average treatment effect of the message is different in subgroup 1 versus subgroup 2. Sometimes the differences are larger, and other times they are smaller. However, because the researcher is diligent and rigorous, they recruited a large sample of respondents in each experiment and are thus able to statistically detect even small differences between subgroups. The results of these ten imaginary experiments are plotted in Figure 1B alongside the original experiment.

Figure 1B reveals a striking pattern: while there is clear evidence of heterogeneity in many of the individual experiments (an interaction effect), the

subgroup treatment effects are strongly correlated *across* experiments—such that the strongest and weakest treatments are essentially the same for subgroups 1 and 2. This pattern indicates that, despite responding differently to the individual messages, people in either group were nevertheless receptive to *similar* features of the messages in general—and this shared receptivity swamps the heterogeneity at the level of individual messages. Put differently, the pattern in Figure 1B reveals evidence of *homogeneity in the rank order of message effects*. Thus, we call this pattern rank-homogeneity. When considered relative to the overall distribution for each subgroup (shown at the margins of Figure 1B), the original message **M** is unremarkable; it is a somewhat weak treatment for people in subgroup 1, and a similarly weak treatment for people in subgroup 2. In other words, the message that best persuaded one type of person tended to be that which also best persuaded a different type of person.

Contrast this with the pattern depicted in Figure 1C. There, the subgroup treatment effects are *uncorrelated* across experiments, and so the rank ordering of treatment effects differs substantially between subgroups. If the effect of a message on one subgroup is large, this does not mean it is large for other subgroups, and may even suggest it is small among other subgroups. This is clear evidence of *heterogeneity in the rank order of treatment effects*—a pattern that we call *rank-heterogeneity*. Relative to the overall distribution for each subgroup, the original message **M** is a somewhat weak treatment for subgroup 1, yet it is an unusually strong treatment for subgroup 2. The message that best persuaded one type of person was generally unlikely to be that which best persuaded a different type of person.

Are we in a world more like that depicted in Figure 1B or 1C? That is, should we expect to see rank-homogeneity or rank-heterogeneity in message persuasiveness? Answering this question is important for understanding whether people are receptive to

similar or different features of messages in general. As a result, the answer to this question has important implications for political persuasion theory and practice.

### **Implications for persuasion theory and practice**

The persuasive advantage of targeting and micro-targeting political messages scales with rank-heterogeneity; if the message that best persuades one type of person is rarely that which best persuades a different type of person—i.e., there is high rank-heterogeneity—then targeting different messages to different people can result in greater returns to persuasion (compared with not targeting). Thus, the extent of rank-heterogeneity has practical implications for public messaging campaigns. For example, suppose that a campaign wants to persuade both liberals and conservatives to change their behavior. Should they use different messaging for each audience, or use the same messaging for both? If rank-heterogeneity is low, then there may be a single “best” message, and targeting will generate little benefit. Furthermore, in light of recent public concerns over the power of political micro-targeting to influence voters’ behavior (BBC 2018; Cadwalladr 2017; Scott 2018), better understanding the extent of rank-heterogeneity can shed empirical light on the validity of those concerns.

Understanding the extent of rank-heterogeneity also has implications for influential theories of political communication. For example, moral reframing theory is one such theory, claiming that political messages are most persuasive when they are framed to appeal to people’s moral values (for a review, see Feinberg and Willer 2019). More specifically, moral reframing theory is based on a more general theory of human moral psychology—known as moral *foundations* theory—which posits that there are five core moral value foundations (care/harm, fairness, loyalty, authority, and sanctity) that different types of people endorse to different degrees (Graham et al. 2013). Most

notably, liberals in the U.S. are claimed to endorse the “individualizing” foundations of care/harm and fairness more than conservatives, while putting lesser weight on the “binding” foundations of loyalty, authority, and sanctity (Graham, Haidt, and Nosek 2009). As applied in moral reframing theory, previous research has found evidence to suggest that messages based on the individualizing foundations are uniquely persuasive among liberals, while messages based on the binding foundations are uniquely persuasive among conservatives (Feinberg and Willer 2013, 2015, 2019).

However, a key question left open by previous work on moral reframing theory is the extent to which liberals and conservatives are receptive to different features of messages *in general*. On one view, the values of liberals and conservatives are sufficiently different that appealing to their distinct sets of values is all but essential when trying to successfully persuade them—it will rarely be the case that they find the same types of messages persuasive (characteristic of rank-heterogeneity). On another view, while liberals and conservatives may put different emphasis on some values, there is sufficient overlap among their values as a whole that they respond largely similarly to most types of messaging appeal (characteristic of rank-homogeneity). Previous work has rarely evaluated the persuasiveness of morally-framed appeals alongside other types of messaging appeals—such as appeals to scientific evidence, commonsense, or expert opinion—and thus cannot distinguish between these views.

## **Relevant previous research**

To estimate rank-heterogeneity (versus rank-homogeneity) and shed light on the foregoing questions, it is necessary to estimate the persuasive effects of many different messages for various subgroups of people, and examine the correlation between the estimates (i.e., Figure 1). This demands a very large sample of messages and people.

To our knowledge, Coppock (2016, 2022) has conducted the most relevant analysis of this type to date. He analyzed twenty-three existing experiments in which Americans were randomized to receive a political message. An important feature of his analysis was that the topics targeted by the messages varied across the experiments, as did various other features of the messages and experiments (e.g., message length, delivery format, experiment subject pools, etc.). Coppock created plots of the average treatment effects of the messages by different subgroups, and they most closely resemble Figure 1B; that is, he finds that the average effects of the messages are strongly positively correlated across subgroups. For example, he estimates a correlation coefficient of 0.82 for the treatment effects across messages for subgroups defined by ideology: when the effects of the messages were large for liberals, they very often also tended to be large for conservatives. Similarly-large correlations were reported for subgroups defined by partisanship, race, education, and gender. Contrary to influential theories of political attitude formation, and the apparent prevalence of message-targeting in the political advertising industry, these results suggest that the effects of political messages are best characterized by rank-*homogeneity*, not heterogeneity.

However, while insightful, the foregoing analysis is limited in two crucial ways. First, by virtue of being taken from existing experiments, the political messages targeted a variety of different policy issues and were delivered in a variety of different formats (e.g., text vignettes, long-form op-eds, videos, etc.). This plausibly inflates the correlation in message effects across subgroups, because attitudes on some issues are generally easier to change than others (e.g., issues that are lower salience), and because some message formats may be generally more engaging than others. This diminishes the theoretical implication of the observed rank-homogeneity, because theories of heterogeneity typically argue that the “match” between message content and subgroup



characteristic is what is most relevant—not the policy issue to which the message speaks, nor the format in which it is delivered. For example, moral reframing theory holds that messages must appeal to the values of the audience in order to persuade; when there is a discordance between message frame and audience values, the effect of the message is diminished. Thus, a strong test of rank-homogeneity is to hold fixed the policy issue and delivery format while varying only the message *content*.

A second and related limitation of Coppock’s analysis is that the political messages were not designed to vary over dimensions that, according to theory, are most relevant for inducing subgroup heterogeneity. This is crucial because a lack of variation over theoretically-relevant message dimensions could also inflate the correlation in message effects across subgroups. For example, if the messages lack variation in the types of moral arguments that are theorized to appeal to liberals versus conservatives then it is perhaps unsurprising to observe minimal heterogeneity by ideology. Thus, a strong test of rank-homogeneity is to vary message content along dimensions identified by theory as most relevant for inducing subgroup heterogeneity.

In this paper, we conduct two large-scale randomized survey experiments designed to address the above limitations, comprising two policy issues, 59 message treatments and over 40,000 U.S. adults in total. Importantly, half of the messages are modelled explicitly on moral reframing theory. Our experiments thus provide (1) a stronger test of whether people are receptive to *similar* or *different* features of political messages in general, and (2) a new and comprehensive test of moral reframing theory.

## **Methods**

### **Policy issues**

Each of our two experiments focuses on attitudes towards a single policy issue. The policies are *Universal Basic Income* (UBI; Experiment 1) and the *U.S. Citizenship Act of 2021* (Experiment 2), an immigration reform bill introduced by Joe Biden on his first day in office. We chose these policy issues for several reasons. First, they are both somewhat low salience (at the time the experiments were conducted), meaning respondents would be less likely to have “made up their mind” on the issue. This increases our chance of detecting message effects. Second, both issues are relatively complex and thus amenable to a variety of different arguments—enabling us to develop and test a large diversity of different message treatments. Finally, the two issues are different in character: UBI may be seen primarily as an economic issue, while the U.S. Citizenship Act is more akin to a social issue because it relates to immigration policy. Studying both issues allows us to better assess the generalizability of our results.

### **Message treatments**

We created a large sample of message treatments for each experiment by first searching online news sources and articles for arguments relating to the policy in question. We subsequently used these arguments to write a set of messages supporting/opposing the policy, exemplifying different persuasive strategies of interest. In particular, as mentioned previously, we drew heavily upon moral foundations/reframing theory, developing half our messages using this framework. We modelled the remaining messages after a variety of other dimensions that commonly appear in theories of political persuasion—including appeals to religion, scientific/historical evidence, expert opinion, public opinion, commonsense reasoning, moral arguments based on

liberty, appeals to compromise, and ad-hominem attacks on those supporting/opposing the policy (Blumenau and Lauderdale 2021). All messages were developed to exemplify a single persuasive strategy, and were coded as such prior to testing. Finally, each message was then edited into a short video treatment comprising 1 or 2 background images in a slideshow, a voiceover with subtitles, and music that was common to all videos. We used video treatments to facilitate engagement.

To illustrate the treatments, Figure 2 shows a screenshot from a pro-UBI treatment (Experiment 1), whose argument was coded as appealing to the “care/harm” moral foundation. The full argument transcript of this treatment reads:

*The idea is seen by supporters as a way to live up to a compassionate ideal that society, as a first priority, should look out for its people’s survival. The COVID-19 pandemic has exposed vulnerabilities of huge populations, and without a social safety net, the social costs can be unbearably high. Universal Basic Income can provide citizens with the money to get necessities like food, to ensure that, at the very least, nobody should be made to go hungry or homeless after losing their jobs.*

The full video can be viewed at [https://mit.edu/~lbh/www/ubi\\_for-care.mp4](https://mit.edu/~lbh/www/ubi_for-care.mp4). The complete list of arguments used in each treatment is in Appendix sections 0.6 and 0.7.



**Figure 2.** Screenshot from pro-UBI message treatment (Experiment 1).

## Experiment design

To estimate the persuasive effect of our video treatments, in each experiment we recruited U.S. adults online via a survey platform that is a common supplier to the Lucid platform (Coppock and McClellan 2019). While our sample is thus a convenience sample and unrepresentative of the general U.S. population, this does not undermine the generalizability of our estimates: formal comparisons between treatment effects estimated using samples from Lucid or Mechanical Turk, versus those in national samples, indicate the two often correspond quite closely (Coppock 2019; Mullinix et al. 2015). Notably, our samples were approximately balanced on ideology and party identification (demographic characteristics of our sample are reported in Appendix section 0.1 that contains balance and differential attrition checks). Finally, studies conducted since the COVID-19 pandemic have found that online survey respondents are recently more diverse and representative than they were before, but

also substantially less attentive (Aronow et al. 2020). Thus, respondents were required to pass an attention check question before entering our experiments.

After answering an initial survey containing demographic covariates, respondents were randomly assigned to condition: either (1) a control group, (2) a single argument for or against the policy, or (3) two arguments: one for and one against the policy. Respondents in all conditions viewed a single video (that is, all videos contain a common introduction describing the policy, followed by zero, one, or two arguments). We included the two-sided treatment condition to improve the generalizability of our results—as the public may often be exposed to conflicting arguments on an issue—and to improve the robustness of our experiment to possible demand characteristics. An important point for our analysis is that respondents who received both a for- and against-argument were assigned each argument independently. Finally, after viewing the video, respondents were asked to rate their attitude towards the policy on a 7-point Likert scale. Below we briefly detail the specific policy description, set of treatment conditions, and outcome question used in each experiment.

### **Experiment 1: Universal Basic Income (UBI)**

Date fielded: 2021-01-02.

Sample size: A total of 17,418 U.S. adults were assigned to watch a video in the UBI experiment.

Treatment assignment: This experiment tested 10 arguments in favor of UBI and 10 arguments against it. Each respondent was assigned to see one (or no) argument in favor of the policy and independently assigned to see one (or no) argument against the policy. When two arguments were assigned to one respondent, their order in the video was randomized—producing a total of 221 possible conditions.

Video transcript: “*Universal Basic Income is a policy proposal that's recently gained public attention. But what does it mean? A Universal Basic Income would replace the current welfare system, so the government would cut all the existing means-tested programs like food stamps, or earned income tax credit and instead pay a fixed amount to everybody in the United States, with a monthly cheque of \$1000 for every citizen.* [TREATMENT ARGUMENTS]. *What do you think? Should the United States implement a Universal Basic Income?*”

Outcome variable: “Do you think the U.S. federal government should create a Universal Basic Income of \$1000 per month for every citizen?” (7-point Likert scale: [1] Definitely no - [4] Not sure - [7] Definitely yes).

## **Experiment 2: U.S. Citizenship Act**

Date fielded: 2021-07-08.

Sample size: A total of 26,472 U.S. adults were assigned to watch a video in the U.S. Citizenship Act experiment.

Treatment assignment: This experiment tested 26 arguments in favor of the U.S. Citizenship Act and 13 arguments against it. Each respondent was assigned to see one argument in favor of the policy, or to a control group. Then, among only those who were assigned a for-argument, a subset was also assigned an argument against the policy. When two arguments were assigned to one respondent, their order was randomized—producing a total of 703 possible conditions (including control, for-only, and for + against). Excluding the “against-only” condition from the design of Experiment 2 allowed us to test a wider variety of arguments (in the “for” direction)—and thus better characterize the variability between them—at the expense of adding some additional complexity to our analysis (as described in the Results section).

Video transcript: “*What is the U.S. Citizenship Act, and why is it important? The U.S. Citizenship Act is an immigration bill introduced to congress this year. Among other things, the bill would increase the yearly limit on visas; create pathways to citizenship for many undocumented immigrants; and invest money in smart border control technology. [TREATMENT ARGUMENTS]. What do you think? Should congress pass the U.S. Citizenship Act?*”

Outcome variable: “Do you support or oppose the US Citizenship Act?” (7-point Likert scale: [1] Strongly oppose - [4] Not sure - [7] Strongly support).

## Results

### Examining rank-heterogeneity

We begin by estimating the correlation between message effects across different subgroups of people in order to identify the extent of rank-heterogeneity (versus rank-homogeneity) in each experiment. Thus, we estimate the conditional average treatment effect (CATE) of each treatment for the demographic subgroups of age, gender, partisanship, and ideology, and we plot the estimates in a scatterplot.<sup>1</sup> The estimates are computed separately for each subgroup in a linear regression which includes both for- and against-treatments, and all 3 remaining demographic variables as covariates to improve precision (Gerber and Green 2012). Due to our two-sided treatment experiment design, the interpretation of the estimates differs slightly between experiments. For UBI, treatment effects are estimated in the context of a (randomly present or absent) counterargument. For the U.S. Citizenship Act, the effects of for-

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<sup>1</sup> In Appendix section 0.1, we present balance and differential attrition checks in which we (i) provide demographic characteristics of our sample and (ii) confirm that the effects we report are not likely to be driven by any differences in sample composition between treatment conditions.

arguments are estimated when those arguments are viewed alone, while the effects of against-arguments are estimated in the presence of a random for-argument.

Figures 3A and 3B show the scatterplots of the estimates for UBI and the U.S. Citizenship Act, respectively. All the estimates are standardized.

To a first approximation, we find limited evidence of rank-heterogeneity of message effects by age or by gender in either experiment. On the contrary, the effects of the messages appear relatively well correlated for respondents older than 40 vs. younger, and for male vs. female respondents. These correlations are all the more impressive given that the individual message effect estimates contain substantial noise, which will tend to attenuate the correlation towards zero.<sup>2</sup> Furthermore, for UBI we find evidence of only moderate rank-heterogeneity by partisanship and ideology: broadly speaking, message effects remain positively correlated for Democrats vs. Republicans, and for liberals vs. conservatives. This occurs despite the fact that our message treatments were designed explicitly to include features that theory says should strongly distinguish these subgroups. Notably, we also observe no evidence of backfire: the for-arguments generally increased support among all subgroups, while the against-arguments generally decreased support among all subgroups on average.

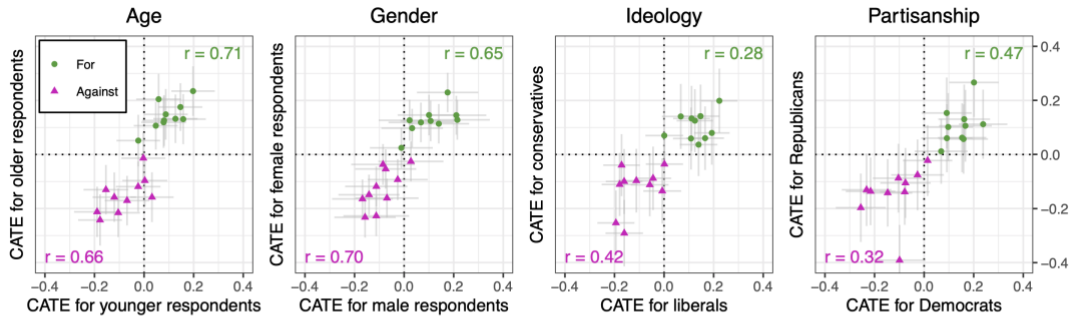
In sum, the results from our UBI experiment appear to be best characterized by a pattern of rank-*homogeneity*, most similar to the pattern depicted in Figure 1B. In contrast to theories of heterogeneity, various different types of people—including Democrats and Republicans, as well as liberals and conservatives—were receptive to broadly similar features of messages regarding the issue of Universal Basic Income.

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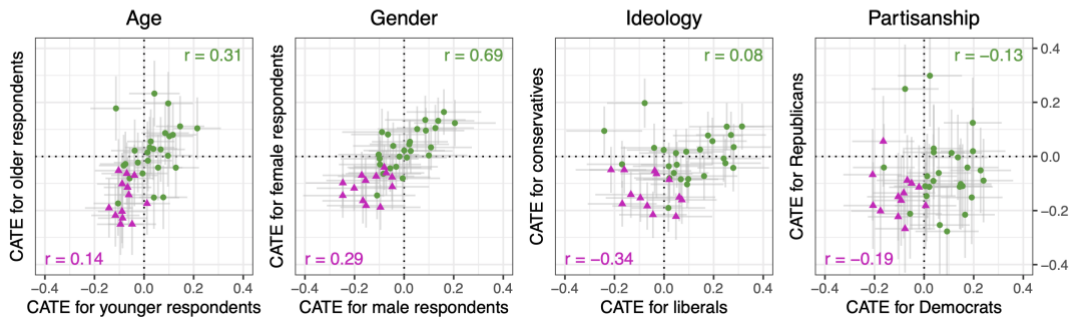
<sup>2</sup> Disattenuated correlations are reported in Appendix section 0.4.



### A. Universal Basic Income experiment



### B. US Citizenship Act experiment



**Figure 3.** Standardized conditional average treatment effects (CATEs) of all message treatments among demographic subgroups in both experiments. Error bars are 95% CI.

However, the results from our experiment on the U.S. Citizenship Act paint a strikingly different picture (Figure 3B). In particular, far from the moderate positive correlations observed in the UBI experiment, here the correlations between message effects are substantially weaker for subgroups of age, and typically *negative* in the case of ideology and partisanship subgroups. Thus, the messages with the largest persuasive effects for liberals/Democrats were often among those with the weakest effects for conservatives/Republicans. The results of this experiment are therefore best characterized by rank-*heterogeneity*, most similar to that depicted in Figure 1C.

The lack of positively-correlated message effects among partisans and ideologues in the U.S. Citizenship Act experiment is consistent with theories which hold that, at least under some conditions, different types of people are indeed receptive

to different features of political messages in general. Given this result, a central question of interest is whether the rank-heterogeneity we observe among liberals and conservatives in particular is explained by their differential receptiveness to messages that were based upon the moral foundations—as predicted by moral reframing theory.

Examining Figure 3B offers suggestive evidence that this may be the case. In the upper left quadrant of the Ideology panel, two for-arguments stand out as having unusually large treatment effects on conservatives, yet lower-than-average effects on liberals. These arguments were based on the moral foundations of *sanctity* and *authority*. The fact that they rank substantially more persuasive among conservatives is consistent with moral reframing theory, because both are classified as “binding” foundations; those theorized to be particularly persuasive among conservatives. Nevertheless, properly testing the theory’s prediction requires testing (1) whether this pattern holds for every single morally-framed argument, and (2) whether it holds *averaging across* all such arguments. In the next section, we examine these questions.

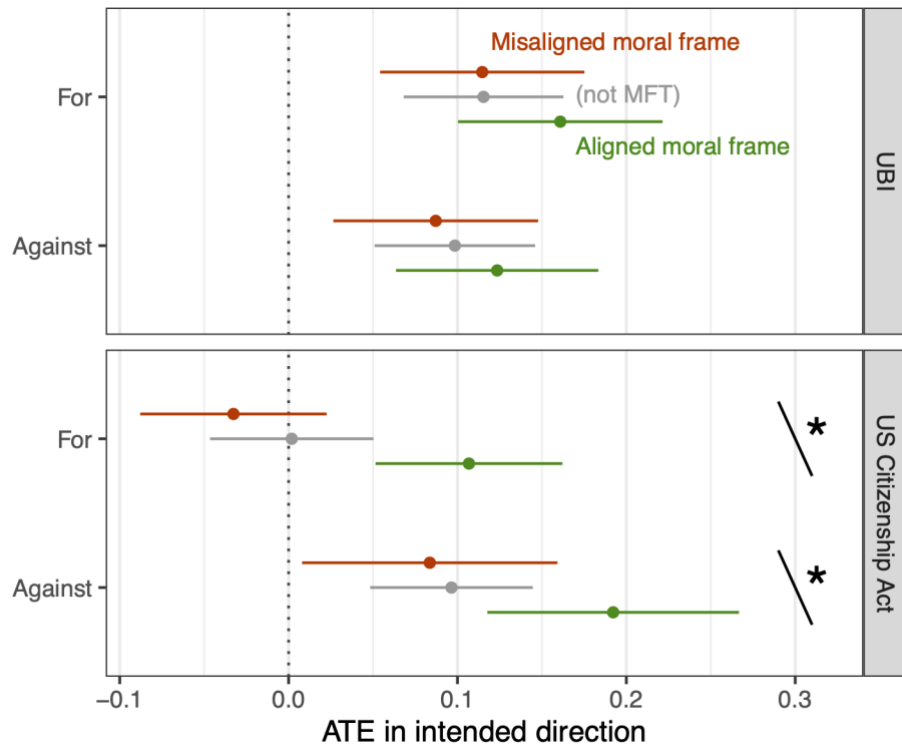
### **Explaining rank-heterogeneity by testing moral reframing theory**

We conduct two analyses to examine whether rank-heterogeneity among liberals and conservatives is explained by their differential receptiveness to messages that were based upon the moral foundations, as predicted by moral reframing theory. First, we use OLS to estimate the average treatment effect of messages that contain moral frames that are aligned vs. misaligned with respondents’ self-reported ideology. Recall that, according to the theory, “individualizing” frames are aligned for liberals, whereas “binding” frames are aligned for conservatives. This analysis provides a basic overall picture of whether aligned messages are more persuasive on average, collapsing across all the relevant (i.e., morally framed) messages. Second, we fit a multilevel linear

regression (Gelman and Hill 2006) in order to quantitatively examine the effect of *individual* messages. This allows us to ask whether *all* the morally-framed messages conform to the predictions of moral reframing theory, or whether only some do.

The estimates from our first analysis—the OLS regressions—are shown in Figure 4. The estimates are broadly consistent with the theory: on average, messages that used moral frames aligned with respondents’ ideology were qualitatively more persuasive—in both experiments and in both directions (*for* and *against*). However, the pattern is much more pronounced and only statistically significant at the .05 level in the U.S. Citizenship Act experiment. This difference between the experiments implies that the greater rank-heterogeneity we see in the U.S. Citizenship Act experiment may indeed be partly due to the fact that liberals and conservatives were differentially persuaded by the moral frames on average, as predicted by the theory.

To investigate whether this is true of every individual morally-framed message in our set, we proceed to fit a multilevel linear regression model to the data. This is preferable to using OLS regression to estimate the individual treatment effects, because some amount of the variability in the individual treatment effect estimates is due to sampling variability rather than true underlying variation in the treatment effects. The multilevel model takes this into account, thus providing estimates of the effect of each individual message that are more accurate on average (McElreath 2020, chapter 13).



**Figure 4.** Average treatment effects of messages, disaggregated by whether the moral frame is aligned or misaligned with the respondent's ideology (moderates excluded). OLS regressions were fitted separately on liberals and conservatives and then the estimates were averaged, to ensure that ideology and frame were orthogonal. Asterisks indicate a (95%) significant difference between aligned and misaligned frames.

The informal description of the multilevel model is that it estimates respondents' policy support using fixed effects for (i) whether they were treated (by either a *for*- or *against*-argument) vs. in the control group, (ii) their self-reported ideology, and (iii) a linear interaction between (i) and (ii). We specify random effects for the individual treatments, allowing (i) and (iii) to vary across them. Since it would be incoherent to pool the *for*- and *against*-treatments together, we include separate fixed and random effects for each of these treatment types. The formal specification of the model is:

$$\begin{aligned}
Y_i &= \text{Normal}(\mu_i, \sigma) \\
\mu_i &= \alpha + \beta_1 \text{ideo}_i + \beta_{2j[i]} \text{for}_i + \beta_{3k[i]} \text{against}_i \\
&\quad + \beta_{4j[i]} (\text{for} \times \text{ideo})_i + \beta_{5k[i]} (\text{against} \times \text{ideo})_i \\
\begin{bmatrix} \beta_{2j} \\ \beta_{4j} \end{bmatrix} &\sim \text{MVNormal} \left( \begin{bmatrix} \beta_2 \\ \beta_4 \end{bmatrix}, \Sigma_1 \right) \\
\begin{bmatrix} \beta_{3k} \\ \beta_{5k} \end{bmatrix} &\sim \text{MVNormal} \left( \begin{bmatrix} \beta_3 \\ \beta_5 \end{bmatrix}, \Sigma_2 \right)
\end{aligned}$$

Where  $Y_i$  is policy support (standardized), subscript  $i$  indexes observations,  $j$  indexes the for-treatments,  $k$  indexes the against-treatments, and  $\Sigma$  is a covariance matrix. The variable *ideo* is scaled to have zero mean and unit range so that the interaction coefficients are interpretable as the difference in treatment effects between “very conservative” and “very liberal” respondents; the *for* and *against* variables are dummy variables for whether the respondent received a for- or against-treatment, respectively. We fit the model in a Bayesian framework, and specify weakly-informative prior distributions over all model parameters—allowing the data to “speak for itself.” (Prior distributions and model diagnostics are in Appendix section 0.2).

Table 1 shows a summary of the model results. The results confirm that the average treatment effects were in the expected direction: in both experiments, the *for* dummy is positive and the *against* dummy is negative. Furthermore, there was substantial variability across treatments in both experiments—shown by the models’ estimate of the standard deviation (SD) in the treatment effects. For example, the average treatment effect of the treatments in favor of UBI was 0.11, with an estimated standard deviation of 0.06 across the treatments. Using the mean  $\pm$  2SDs as a heuristic,

this implies that we should expect the true effect of the messages in favor of UBI to range from zero ( $0.11 - 2 \times 0.06 = -0.01$ ) to larger than 0.20 ( $0.11 + 2 \times 0.06 = 0.23$ ).

Moving onto the interaction between the treatments and ideology, there is some evidence of interaction effects on average across treatments, but only one of these average effects is significant at the 0.05 level: *For x Ideology* in the U.S. Citizenship Act experiment. Notably, however, all of the average interaction effects are negatively signed. This tells us that for-treatments tended to produce the greatest persuasive effects among liberals, while against-treatments tended to produce the greatest persuasive effects among conservatives. This highlights the potential pitfalls of considering only individual interaction effects when drawing conclusions about heterogeneity—as we outlined in the introduction. That is, a for-treatment with a larger persuasive effect among liberals vs. conservatives may still be an unusually weak treatment among liberals, and an unusually strong treatment among conservatives.

**Table 1.** Summary of results from multilevel models.

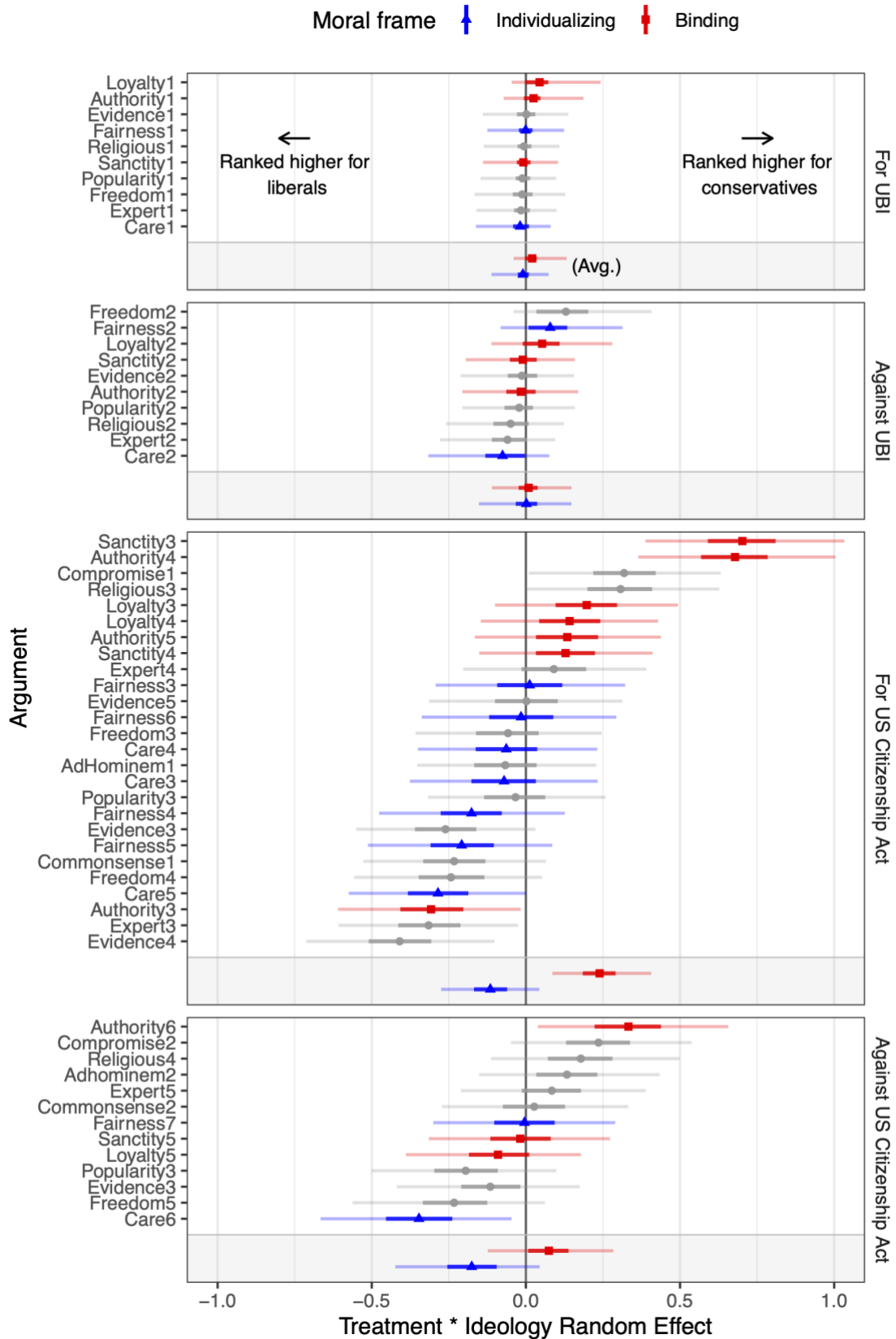
Term	UBI	U.S. Citizenship Act
Ideology	-0.72 (0.06)	-0.68 (0.05)
For	0.11 (0.03)	0.03 (0.02)
Against	-0.12 (0.03)	-0.12 (0.02)
For x Ideology	-0.12 (0.07)	-0.21 (0.09)
Against x Ideology	-0.11 (0.08)	-0.12 (0.10)
SD (For)	0.06 (0.02)	0.07 (0.01)
SD (Against)	0.08 (0.03)	0.03 (0.02)
SD (For x Ideology)	0.06 (0.05)	0.34 (0.06)
SD (Against x Ideology)	0.12 (0.08)	0.27 (0.09)
Cor (For, For x Ideology)	0.03 (0.55)	-0.10 (0.24)
Cor (Against, Against x Ideology)	0.24 (0.46)	0.20 (0.48)

*Note.* Standard errors are in parentheses. SD = standard deviation; Cor = correlation.

Now we move onto our main quantity of interest: the models' estimate of the standard deviation in interaction effects across treatments:  $SD(For\ x\ Ideology)$  and  $SD(Against\ x\ Ideology)$ . This quantity tells us how variable the effects of individual treatments were for liberals vs. conservatives. In the U.S. Citizenship Act experiment, this variability is substantial —  $SD(For\ x\ Ideology) = 0.34$  — swamping the average interaction effects observed in that experiment. For example, despite the for-treatments being more persuasive among liberals on average ( $For\ x\ Ideology = -0.21$ ), some individual for-treatments were nevertheless substantially more persuasive among conservatives (i.e., the mean + 2SD =  $-0.21 + 2*0.34 = 0.47$ ). This was not the case for UBI. Consistent with Figure 3A, in the UBI experiment the variability was much more muted; interaction effects, where they existed, were mostly predictable based only on the direction of the treatment rather than its content.

Our key question is whether the variability in treatment effects across liberals and conservatives coheres with the predictions of moral reframing theory. In other words, is it true that (i) every single treatment that uses a “binding” frame is more persuasive among conservatives than liberals, and (ii) every single treatment that uses an “individualizing” frame is more effective among liberals than conservatives?

To answer this question, we examine the model-estimated interaction effects for each of the 59 individual treatments tested in our two experiments (Figure 5). The interpretation of the effects is: the difference in each treatment's effect size between “very liberal” and “very conservative” respondents, after adjusting for the mean effect size of each subgroup. Positive values indicate that the treatment was ranked more persuasive among conservatives than among liberals, while negative values indicate the treatment was ranked more persuasive among liberals than among conservatives.



**Figure 5.** Interaction effects for each argument (treatment) from the multilevel models. Coefficients are interpretable as the difference in treatment effect size between “very liberal” and “very conservative” respondents, after adjusting for the mean effect size of each subgroup. Error bars are 95% and 50% credible intervals.



Treatments that appealed to the “binding” moral foundations are colored red, while those that appealed to the “individualizing” foundations are colored blue (treatments that made other types of appeal are in grey).

The estimates in Figure 5 provide mixed support for moral reframing theory. In particular, examining the treatments from the U.S. Citizenship Act experiment shows that there are some treatments that clearly map onto the predictions of the theory. For example, “binding” treatments *Sanctity3* and *Authority4* ranked substantially more persuasive among conservatives than liberals, while an “individualizing” treatment like *Care5* ranked more persuasive among liberals than conservatives.<sup>3</sup> Furthermore, averaging across all treatments, the pattern predicted by theory is generally borne out. However, a number of treatments depart from the theory’s predictions. For example, *Authority3* actually ranked higher among liberals than conservatives, and several other treatments ranked similarly among both sets of respondents—such as *Sanctity5* and *Loyalty5*—despite theory implying they should rank higher among conservatives.

We thus draw two main conclusions from the results reported in this section. First, the greater rank-heterogeneity observed in the U.S. Citizenship Act (vs. UBI) experiment does indeed map onto the predictions of moral reframing theory, *on average across treatments*. Second, however, we observe a number of cases where the effects of individual treatments clearly depart from the theory’s predictions. This qualifies support for the theory, and highlights an important implication of our results for generalizability—which we return to in our concluding comments below.

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<sup>3</sup> Recall that the treatment texts can be viewed in Appendix sections 0.6 and 0.7.

## Conclusion

In this paper, we aimed to advance understanding of the extent to which people are receptive to similar or different features of political messages in general. To that end, we conducted two large-scale experiments to study how correlated message effects are between different subgroups of U.S. adults, across a large and diverse set of messages on the same issue. A strong positive correlation suggests that people are primarily susceptible to similar features of messages when updating their attitudes, and this shared susceptibility swamps any differences in how they respond to individual messages. We call this pattern *rank-homogeneity*. In contrast, a weak, null or negative correlation indicates that people are primarily susceptible to distinct features of messages when updating their attitudes. We call this pattern *rank-heterogeneity*.

Does rank-homogeneity or rank-heterogeneity better characterize people's response to political messages? The results of our experiments provide nuanced and revealing evidence on this question. In the UBI experiment, message effects were moderately to strongly positively correlated across various demographic subgroups, including partisanship and ideology. This is significant because the messages were designed explicitly and on the basis of theory to induce heterogeneity among these subgroups. Thus, the results offer solid evidence of rank-homogeneity. By contrast, the U.S. Citizenship Act experiment revealed weak negative correlations among these subgroups—thus, the most persuasive messages for conservatives were among the least persuasive for liberals, and vice versa. This offers strong evidence of rank-heterogeneity. Furthermore, we found evidence of a message-level cause of this heterogeneity, consistent with moral reframing theory: messages ranking more persuasive among liberals typically employed “individualizing” frames, whereas those ranking more persuasive among conservatives typically employed “binding” frames.

Our results have several implications for persuasion theory and practice.

First, the contrasting pattern of results between our experiments could be due to a number of factors. However, the clearest and perhaps most obvious candidate is the different policy issues that were targeted (UBI vs. U.S. Citizenship Act). A growing body of evidence suggests that the effects of political messages are prone to considerable variation across policy issues (Blumenau and Lauderdale 2021; Clifford, Leeper, and Rainey 2021; Tappin 2020). Our results are consistent with this evidence, and further demonstrate the importance of including multiple policy issues or topics in studies on political communication and persuasion. This is important for building cumulative and generalizable knowledge, and for avoiding a published literature that consists of many studies that canvas a small number of policy issues and reach different, mutually exclusive conclusions due to an idiosyncratic sample of issues (Yarkoni 2020). Of course, in a design like ours, the resources required to conduct such studies can sometimes be substantial. The field may thus need to hasten its shift toward larger scale collaborations to meet these requirements (e.g., Moshontz et al. 2018).

A related implication of our results arises from the variability in treatment effects observed for the U.S. Citizenship Act issue. Specifically, despite the predictions of moral reframing theory being supported *on average across treatments*, we observed a number of cases where the effects of individual treatments clearly departed from the theory's predictions (e.g., an appeal to authority ranking more persuasive among liberals than conservatives). The implication is that, when theories make predictions about the effects of a *latent* treatment—such as “appeals to authority will be more persuasive among conservatives than liberals”—the effects of particular *instantiations* of that treatment may still depart from the theory's prediction. The upshot is that tests of theory will be more generalizable when they rely on multiple instantiations of the

latent treatment, as we did here (see also Blumenau and Lauderdale 2021). Thus, our results offer stronger evidence in favor of moral reframing theory than previous studies, which tend to rely on just one or two treatments from a single moral foundation.

Our results are broadly consistent with the idea that U.S. conservatives and liberals hold different patterns of moral values (Graham et al. 2013), explaining why they are receptive to different moral arguments on some issues. However, we hasten to add that there are other explanations of the mechanism here. For example, another possibility is that moral arguments provide information about which groups (e.g., political parties) support the policy in question. Perhaps for-arguments that appeal to “binding” values are more diagnostic of Republican than Democratic Party support, explaining why conservatives are more receptive to such arguments.<sup>4</sup> However, we leave it to future research to more rigorously investigate the mechanism(s) underlying moral reframing’s impact on rank-heterogeneity.

Our results additionally offer insights for political communication in practice: for some political issues and demographic subgroups, targeting messages to different subgroups may not be worthwhile because the rank-ordering of message effectiveness is similar. However, as our results also show, clearly this may not always be the case. Thus, perhaps the clearest practical implication of our results is that it is worthwhile to test the heterogeneous effectiveness of messages before disseminating them.

Finally, our results directly extend the analyses conducted by Coppock (2016, 2022). Recall that those analyses identified very strong positive correlations across subgroups—powerful evidence of rank-homogeneity—even for subgroups defined by partisanship and ideology. While our results qualitatively agree with those findings in

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<sup>4</sup> In Appendix section 0.3, we report some evidence of this possibility from a smaller-scale follow up study: heterogeneity was somewhat attenuated when people were told the positions of the parties on the U.S. Citizenship Act.

many cases, we did not consistently replicate such strong correlations. Our findings thus lend credibility to a concern described in the introduction, that the very strong correlations reported by Coppock may be somewhat inflated by variation in policy issues/message format across experiments, rather than the content of the messages themselves. That said, there are other possible explanations. The discrepancy may reflect a difference in the emphasis of the treatments: our experiments explicitly focused on arguments that theory suggests should induce subgroup heterogeneity, an emphasis that is arguably less pronounced in the treatments analyzed by Coppock. Furthermore, we focused on lower-salience policy issues for which many people may be unaware of party positions. It is possible that the heterogeneity we observe is due to respondents interpreting the arguments as implicit cues as to which parties support the policy. Therefore, future research should more thoroughly test whether and to what extent rank-heterogeneity is robust to the presence of explicit party cues, as well as how common it is among treatments (e.g., ads) produced by actual political campaigns.

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# Appendix

## 0.1 Balance and attrition checks

In this section we test for any differences in sample composition between treatment groups that could have substantially altered our estimates of treatment effects. Such differences may arise either from a failure to properly randomize respondents, or because some treatments cause fewer, or different, respondents to drop out of the study with missing outcomes. The primary concern is that, if imbalance between treatment groups is correlated with respondents potential outcomes (after conditioning on observed covariates) it can substantially bias the estimated causal effects of treatments, and therefore compromise the validity of a randomized study. As our study emphasized the heterogeneous effects of moral frames by respondent ideology, we conduct specific checks to more strictly test whether imbalance may have driven these heterogeneity estimates in particular, in addition to standard checks of demographic balance between treatment groups.

In our study design, all respondents: (1) provided demographic covariates, then (2) viewed an identical 20-25s description of the policy under consideration, then (3) viewed (in treatment but not control groups) one or two policy arguments, then (4) provided their outcome (policy support). The *attrition rate* is therefore the proportion of respondents who left the study during stages (3) or (4), after viewing the initial policy description but before providing outcomes. The attrition rate was 2.6% in the U.S. Citizenship Act experiment, and we analyze predictors of this attrition to provide additional evidence beyond our overall balance checks below. Unfortunately, in the Universal Basic Income experiment, data collection failed to distinguish between respondents who dropped out during stages (2) and (3), and so we are unable to measure the true attrition rate in the UBI experiment. Instead, we conduct tests for differential attrition among *all respondents who provided pre-treatment covariates* (1), based on the treatment that they would have received after viewing the policy description. Missingness is therefore larger in this group (9.3%), and our tests of differential attrition are somewhat more noisy.

Below, we present standard balance checks along respondent demographics, additional checks along the dimensions most correlated with respondent outcome, and also estimate any imbalance caused by attrition specifically. To preview our results, we do find evidence of some differential attrition between treatment and control groups: respondents were more likely to drop out of the study when assigned to view a policy argument than not, which we suggest is likely due to the additional length of the videos. Fortunately, while the treatment effect on attrition is significant, its magnitude is very small (only detectable due to the unusually large scale of our experiments). Furthermore, this attrition is essentially uncorrelated with respondent demographics that predict outcome, and is not detectable in our overall balance checks. Overall, we do not find evidence that imbalance between treatment groups substantially affected our estimates of average treatment effects, or of heterogeneity by moral frames.

### 0.1.1 Overall imbalance

In Tables 1 and 2, we assess balance along various demographics among all respondents analysed in our main analysis (that is, excluding those with missing outcomes). For each experiment we conduct four regressions, testing whether respondents in the *For* or *Against* conditions differed in age, gender, ideology, or party identification. Despite the high precision afforded by our large sample size, the omnibus test is null for each of these regressions, suggesting that any demographic imbalance between treatment groups was at most substantively small.

Since the primary concern of imbalance is along dimensions that correlate with respondent outcomes, we next provide a more targeted test of imbalance that utilises these dimensions in particular. For this we assess balance along an ‘aggregate’ covariate which is a weighted sum of all pre-treatment covariates, choosing the weights to produce maximal correlation with respondent outcomes. Specifically, weights were set by fitting an OLS regression model to outcomes of control group respondents, and then scaled so that the resulting aggregate has unit variance.

Figures 1 and 2 each show the result of six regressions conducted to test for imbalance along this aggregate covariate. We first assess differences between treatment groups among all respondents who provided outcomes (top-left) and again find no significant effect. A benefit of this analysis is its interpretability: for example, in the UBI experiment, the standardized average treatment effect of *For* arguments was estimated to be 0.11 (Main text, Table 1). Compare this to the Figure 1 estimate for the standardized effect of *For* treatments on the aggregate pre-treatment covariate. This effect is not only much smaller than 0.11 (it is null and precisely estimated) but is also in the opposing direction. This provides stronger evidence that our main effect estimates were not driven by an imbalance of potential outcomes between treatment groups.

We then conduct two further regressions (center-left, bottom-left) to assess whether imbalance may have contributed to the heterogeneous effects by moral frame described in our main analysis. For these, we restrict to only respondents who received a *For* (/Against) treatment with a moral frame and estimate the effect of the frame type (Binding vs. Individualizing) interacted with respondent ideology. Across all tests, no coefficients involving treatments were significantly different from zero, suggesting that respondents’ potential outcomes *within ideological subgroups* were also balanced between treatment groups.

### 0.1.2 Imbalance due to attrition

Next, we conduct tests of imbalance caused by differential attrition specifically. Broadly speaking, attrition can produce imbalance through mechanisms: treatments may cause *different* respondents to drop out, or they may cause respondents to drop out at a different rate. We consider these possibilities in turn.

On the right of Figures 1 and 2 we conduct the same set of balance checks for the aggregate covariate described above, but restricted only to respondents with *missing outcomes*. This allows us to assess whether the composition of missing respondents differs between treatment groups, which could potentially cause imbalance. In these tests, no coefficients involving treatments were found to be significantly different from zero, suggesting that this mechanism

did not play a substantial role in driving our treatment effect estimates.

Tables 3-10 consider the extent to which different *rates* of attrition between treatment groups may impact our treatment effect estimates. Tables 3 and 7 estimate the increase in attrition due to respondents receiving a For or Against argument in each experiment. Among these, the largest effect is in arguments *Against* UBI, which increased attrition by 1.8% on average. In absolute terms this is a relatively small difference: if we consider the extreme adversarial case in which all 1.8% of these additional missing respondents had maximal potential outcomes (1.32 on a standardized scale), this would shift standardized ATE estimates only by 0.024 (a small portion of the overall ATE, estimated as -0.12 in Table 1). Furthermore, this extreme case is unrealistic: as Table 4 shows, missing respondents do not appear substantially more supportive of the policy, but if anything are estimated to be (very slightly) *less* supportive than average. These results strongly suggest that differences in attrition rates are unlikely to substantially affected our estimates of average treatment effects.

Finally, Tables 5, 6, 9 and 10 show regressions restricted to respondents who received For (/Against) arguments based on moral frames, and include interaction with ideology. These regressions test whether Binding (vs. Individualizing) frames affected attrition overall, as well as whether they affected attrition differentially between liberals and conservatives. Again, none of these tests were significant, suggesting that differences in attrition rates were not a substantial driver of our findings of treatment effect heterogeneity.

## Universal Basic Income experiment: Balance checks

	Age (years)			Female (vs. Male)			Ideology (1-7)			Rep (vs. Dem)		
	Effect	stderr	pval	Effect	stderr	pval	Effect	stderr	pval	Effect	stderr	pval
<i>(Intercept)</i>	41.728	(0.234)	0.000	0.713	(0.007)	0.000	3.388	(0.024)	0.000	0.417	(0.010)	0.000
<i>For</i>	0.172	(0.241)	0.477	0.009	(0.007)	0.200	0.039	(0.025)	0.117	0.021	(0.010)	0.040
<i>Against</i>	0.049	(0.241)	0.838	0.008	(0.007)	0.271	0.019	(0.025)	0.439	0.010	(0.010)	0.346
<i>F – test</i>			0.761			0.240			0.218			0.077

Table 1: Balance checks for individual demographics. Each set of columns shows a single regression, restricted to respondents who provided outcomes.

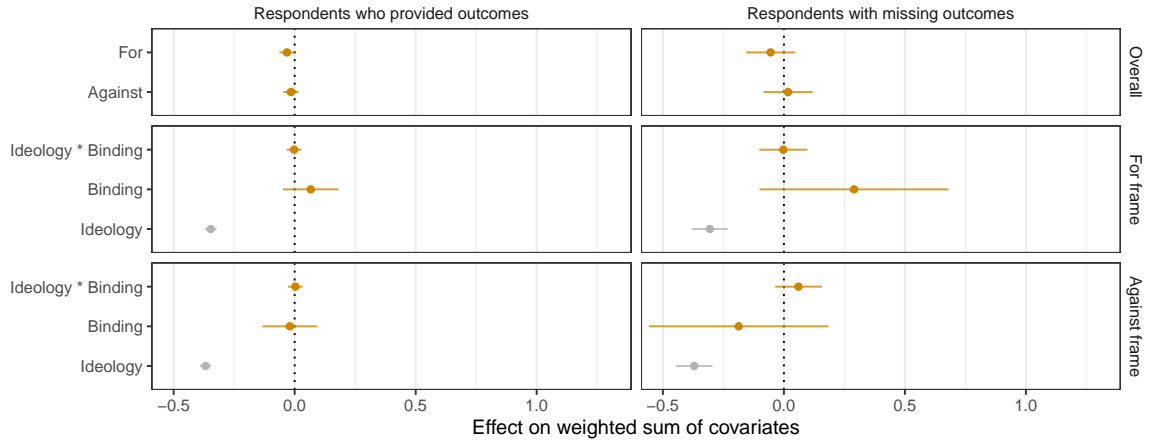


Figure 1: Balance checks for the aggregate of pre-treatment covariates. Each subplot shows a regression (intercepts not shown) restricted either to respondents who provided outcomes (left) or those with missing outcomes (right). The aggregate is a weighted sum of covariates, with unit variance and maximal correlation to control-group outcomes.

## U.S. Citizenship Act experiment: Balance checks

	Age (years)			Female (vs. Male)			Ideology (1-7)			Rep (vs. Dem)		
	Effect	stderr	pval	Effect	stderr	pval	Effect	stderr	pval	Effect	stderr	pval
<i>(Intercept)</i>	39.826	(0.174)	0.000	0.739	(0.005)	0.000	3.406	(0.017)	0.000	0.426	(0.007)	0.000
<i>For</i>	-0.047	(0.239)	0.844	0.013	(0.007)	0.049	0.033	(0.023)	0.148	0.019	(0.010)	0.057
<i>Against</i>	0.031	(0.233)	0.896	-0.010	(0.007)	0.135	-0.040	(0.022)	0.076	-0.020	(0.010)	0.043
<i>F – test</i>			0.980			0.118			0.164			0.074

Table 2: Balance checks for individual demographics. Each set of columns shows a single regression, restricted to respondents who provided outcomes.

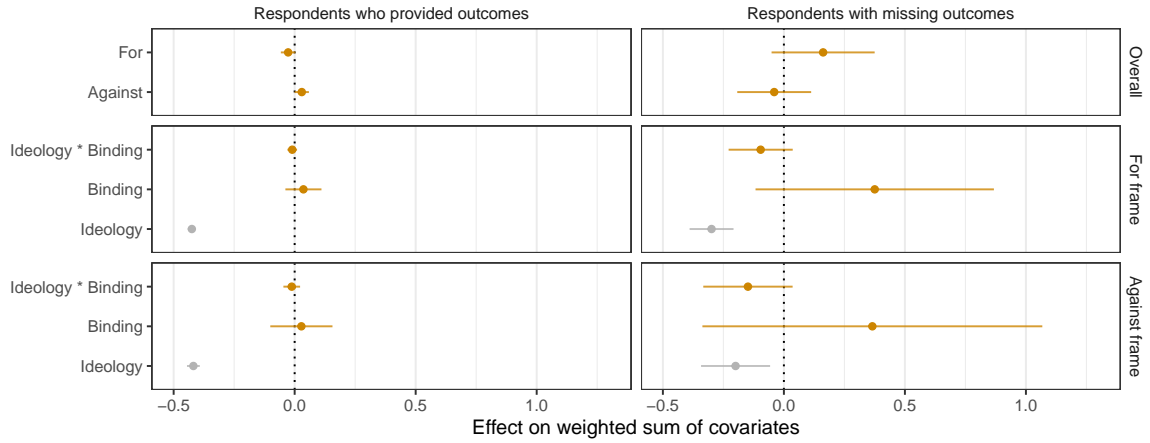


Figure 2: Balance checks for the aggregate of pre-treatment covariates. Each subplot shows a regression (intercepts not shown) restricted either to respondents who provided outcomes (left) or those with missing outcomes (right). The aggregate is a weighted sum of covariates, with unit variance and maximal correlation to control-group outcomes.

### A. Universal Basic Income experiment: Attrition checks

	Effect on <i>Missing</i>	pval
<i>(Intercept)</i>	0.075 (0.004)	0.000
<i>For</i>	0.012 (0.004)	0.007
<i>Against</i>	0.018 (0.004)	0.000

Table 3: Attrition main effects

	Effect on <i>covariate</i>	pval
<i>(Intercept)</i>	0.010 (0.008)	0.204
<i>Missing</i>	-0.109 (0.026)	0.000

Table 4: Aggregate traits of missing respondents

	Effect on <i>Missing</i>	pval
<i>(Intercept)</i>	0.071 (0.016)	0.000
<i>Binding</i>	0.012 (0.021)	0.584
<i>Ideology</i>	0.009 (0.004)	0.043
<i>Ideology * Binding</i>	-0.003 (0.006)	0.565

Table 5: Attrition interaction, *for* frames

	Effect on <i>Missing</i>	pval
<i>(Intercept)</i>	0.082 (0.017)	0.000
<i>Binding</i>	0.021 (0.022)	0.337
<i>Ideology</i>	0.005 (0.004)	0.220
<i>Ideology * Binding</i>	-0.003 (0.006)	0.648

Table 6: Attrition interaction, *against* frames

### B. US Citizenship Act experiment: Attrition checks

	Effect on <i>Missing</i>	pval
<i>(Intercept)</i>	0.013 (0.002)	0.000
<i>For</i>	0.016 (0.002)	0.000
<i>Against</i>	0.007 (0.002)	0.004

Table 7: Attrition main effects

	Effect on <i>covariate</i>	pval
<i>(Intercept)</i>	0.000 (0.006)	0.967
<i>Missing</i>	-0.010 (0.038)	0.796

Table 8: Aggregate traits of missing respondents

	Effect on <i>Missing</i>	pval
<i>(Intercept)</i>	0.022 (0.006)	0.000
<i>Binding</i>	0.012 (0.009)	0.179
<i>Ideology</i>	0.003 (0.002)	0.118
<i>Ideology * Binding</i>	-0.004 (0.002)	0.108

Table 9: Attrition interaction, *for* frames

	Effect on <i>Missing</i>	pval
<i>(Intercept)</i>	0.033 (0.012)	0.008
<i>Binding</i>	-0.002 (0.016)	0.884
<i>Ideology</i>	0.001 (0.003)	0.671
<i>Ideology * Binding</i>	0.000 (0.004)	0.928

Table 10: Attrition interaction, *against* frames

## 0.2 Multilevel regression model specification

Our multilevel regression model was fit using BRMS (Bürkner, 2017) using the following specification:

$$\begin{aligned} PolicySupport \sim & 1 + Ideology + ... \\ & For + For : Ideology + (0 + For + For : Ideology | i) + ... \\ & Against + Against : Ideology + (0 + Against + Against : Ideology | j) \end{aligned}$$

...where *For* (*Against*) is a dummy variable indicating whether the respondent saw any For (Against) treatment, and *i* (*j*) is a categorical variable indicating the specific For (Against) treatment they saw. We use default, weakly-informative priors over all latents, corresponding to the following hierarchical Bayesian regression model:

$$\begin{aligned} a &\sim \text{StudentT}(3, M, 2.5) && \text{Intercept} \\ \alpha &\sim \text{ImproperUniform}(\mathcal{R}) && \text{Slope on Ideology} \\ m_{(F)}, \mu_{(F)}, m_{(A)}, \mu_{(A)} &\sim \text{ImproperUniform}(\mathcal{R}) && \text{Mean effect/interaction} \\ s, s_{(F)}, \sigma_{(F)}, s_{(A)}, \sigma_{(A)} &\sim \text{StudentT}(3, 0, 2.5) && \text{Standard deviations} \\ \rho_{(F)}, \rho_{(A)} &\sim \text{Uniform}(-1, 1) && \text{Correlations} \\ (b_i, \beta_i) &\sim \text{MVN}(0, [[s_{(F)}^2, \rho_{(F)} s_{(F)} \sigma_{(F)}], [\rho_{(F)} s_{(F)} \sigma_{(F)}, \sigma_{(F)}^2]]) && \text{For random effect} \\ (b_j, \beta_j) &\sim \text{MVN}(0, [[s_{(A)}^2, \rho_{(A)} s_{(A)} \sigma_{(A)}], [\rho_{(A)} s_{(A)} \sigma_{(A)}, \sigma_{(A)}^2]]) && \text{Against random effect} \\ \epsilon &\sim \text{Normal}(0, s^2) && \text{Residuals} \\ PolicySupport = &a + For \cdot (m_{(F)} + b_i) + \cdot (m_{(A)} + b_j) + ... \\ &Ideology \cdot \left( \alpha + For \cdot (\mu_{(F)} + \beta_i) + \right. \\ &\quad \left. Against \cdot (\mu_{(A)} + \beta_j) \right) + \epsilon \end{aligned}$$

Before fitting, policy support was scaled by the control group mean and variance (to produce standardized effect sizes) and *Ideology* was scaled to have zero mean and unit range (so that heterogeneous effects are interpretable as ‘very conservative vs. very liberal’). The intercept prior was centered on the data median ( $M$ , as default in BRMS). The posterior was estimated using 4 parallel chains with 1000 post-warmup samples per chain. MCMC diagnostics suggest that the posterior was explored successfully: the Gelman-Rubin statistic ( $\hat{R}$ ) was less than 1.01 and the effective sample size was over 700 for all parameters.

### 0.3 Party cues experiment

In our U.S. Citizenship Act experiment, we observed substantial rank-heterogeneity between liberals and conservatives. One explanation for this difference is that the treatments contain arguments which emphasize values and preferences on which liberals and conservatives differ, therefore producing a different persuasive effect between these groups (‘moral reframing’). However, an alternative explanation is that participants may not be responding to the substantive arguments *per se*, but rather to what these arguments implicitly communicate about which groups support/oppose the policy (e.g. based on the specific words). To help distinguish these accounts, we conducted a smaller-scale follow-up experiment (N=10,025 U.S. adults) in which we additionally manipulated participants’ knowledge about who supports the policy, by including an explicit party cue.

For this experiment we used a restricted set of eight arguments, chosen as those with the largest interaction effects (estimated by our initial experiment). The experiment design was identical to the initial experiment, except that all participants were additionally assigned to either the *cue* or *no cue* condition. In the *cue* condition, participants were informed about partisan support for the bill, immediately before watching the video, and then again immediately before providing their response. The exact text of this cue was:

*“The U.S. Citizenship Act was proposed by President Joe Biden on his first day in office, and was later introduced to congress by Democrats Linda Sanchez and Bob Menendez.”*

We analyze their responses by conducting OLS regressions separately for the *cue* and *no cue* conditions, using the following regression specification for both:

$$\text{policy\_support} \sim \text{ideology} + \text{t\_aligned} + \text{t\_misaligned},$$

For this regression,  $\text{t\_aligned} = 1$  indicates that the participant saw an argument aligned with their ideology, and  $\text{t\_misaligned} = 1$  indicates that the participants saw an argument that misaligned with their ideology (see Table 11). The difference between coefficients on these terms therefore estimates the extent to which ideology moderates treatment effects in the expected direction, averaged over all eight treatments.

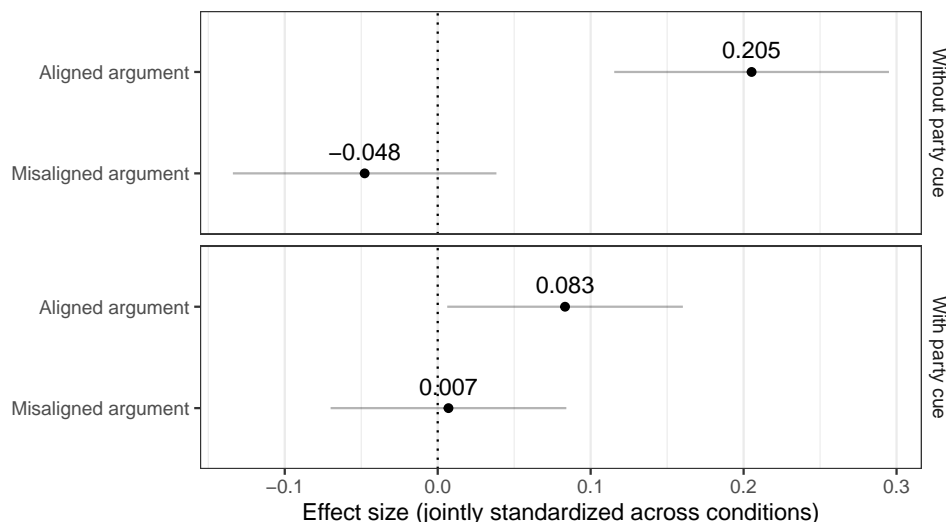


Figure 3: Regression coefficients from party cues experiment.



Results of this analysis are shown in Figure 3. Overall, we observe a substantial difference between the two conditions. In the absence of party cues, treatment effects were estimated to be 0.205 among participant’s whose ideology matched the treatment; but when party cues were present, these effects were estimated as only 0.083. For participants whose ideology clashed with the treatment, effects did not differ significantly from zero in either condition, and in fact were estimated to be slightly negative (-0.048, corresponding to backlash) in the *no cue* condition.

It is notable that for this subset of treatments, which were selected based on their highly heterogeneous effects between conservatives and liberals, the scale of this group difference decreased substantially when party cues were present (difference = 0.076) vs absent (0.253). This result suggests that implicit group cues were responsible for much of the observed rank-heterogeneity in our initial experiment.

Nonetheless, the fact that we continue to see differences by ideology, even when participants are made already explicitly aware of party positions, suggests that implicit group cues in the treatments are not the sole mechanism driving heterogeneity. Indeed, we find that the (overall) average treatment effect across all participants reduces from 0.073 [0.011, 0.135] in the absence of party cues, to 0.045 [-0.010, 0.100] in the presence of party cues. Thus, even if the heterogeneity in our initial experiment was driven *purely* by a mechanism other than implicit group cues, the introduction of explicit party cues would still somewhat reduce the magnitude of this heterogeneity (by virtue of diminishing the overall treatment effect).

To convincingly distinguish mechanisms of heterogeneity, we suggest that future work consider other designs which experimentally control the impact of implicit group cues without diluting overall treatment effects. This could include creating treatments which vary linguistically, or which vary the person to whom an argument is attributed (e.g. a liberal or conservative voter), while fixing or independently manipulating the substantive argument.

Argument	<i>con</i>	<i>lib</i>
<b>Sanctity3</b> ( <i>Supporting argument for conservatives</i> )	1	0
<b>Authority4</b> ( <i>Supporting argument for conservatives</i> )	1	0
<b>Evidence4</b> ( <i>Supporting argument for liberals</i> )	0	1
<b>Expert4</b> ( <i>Supporting argument for liberals</i> )	0	1
<b>Authority6</b> ( <i>Opposing argument for conservatives</i> )	-1	0
<b>Compromise2</b> ( <i>Opposing argument for conservatives</i> )	-1	0
<b>Care6</b> ( <i>Opposing argument for liberals</i> )	0	-1
<b>Popularity3</b> ( <i>Opposing argument for liberals</i> )	0	-1

Table 11: U.S Citizenship Act arguments used in party cues experiment, labeled based on the direct of effects (for=1, against=-1) as well as whether they were stronger among liberals or conservatives (as estimated by the initial experiment). Regression inputs are defined based on the viewing participant’s **ideology** (very liberal=0, very conservative=1) as follows:

$$\begin{aligned} \mathbf{t\_aligned} &= \mathbf{con} * \mathbf{ideology} + \mathbf{lib} * (1 - \mathbf{ideology}) \\ \mathbf{t\_misaligned} &= \mathbf{lib} * \mathbf{ideology} + \mathbf{con} * (1 - \mathbf{ideology}) \end{aligned}$$

For participants who saw multiple arguments, we assume that treatment effects are additive and so calculate **t\_aligned** and **t\_misaligned** by summing over all arguments included.

## 0.4 Estimating unattenuated correlation between CATEs

The scatterplots in Figure 3 (main text) presented raw correlations for CATEs between exclusive demographic subgroups. However, these correlations are substantially attenuated by noise in the individual CATE estimates. Here we estimate the unattenuated correlation using a simple Bayesian model.

To maximise precision, we consider the correlation of all arguments in a single experiment together (both *for* and *against*). However, we reverse the signs of the CATEs on all *against* arguments so that, like the *for* arguments, they represent treatment effects in the ‘intended’ direction. This avoids inflating the correlation due to mixing arguments with different persuasive goals.

We then fit a model in which all CATEs are assumed to come from a bivariate normal distribution, where the means and standard deviations in each dimension are unknown with a flat (improper) prior, and the correlation between subgroups is also unknown. For observations, we use the same point estimates and (co-)variances presented in Figure 3 (main text) to define a Normal likelihood. We then use Stan to find the maximum (marginal-) likelihood estimate of the correlation between subgroups.

The estimates produced by this analysis are shown in Figure 4 below, and succinctly highlight the differences between experiments and between demographics under consideration. Looking at gender, for example, the correlation between CATEs calculated for men vs. women is statistically indistinguishable from 1 in both experiments, replicating the strong correlations found by Coppock (2016, 2022). By contrast, the correlations between CATEs calculated for liberals vs. conservatives, or for Democrats vs. Republicans, are estimated to be *negative* in the U.S. Citizenship Act experiment, indicative of substantial rank-heterogeneity.

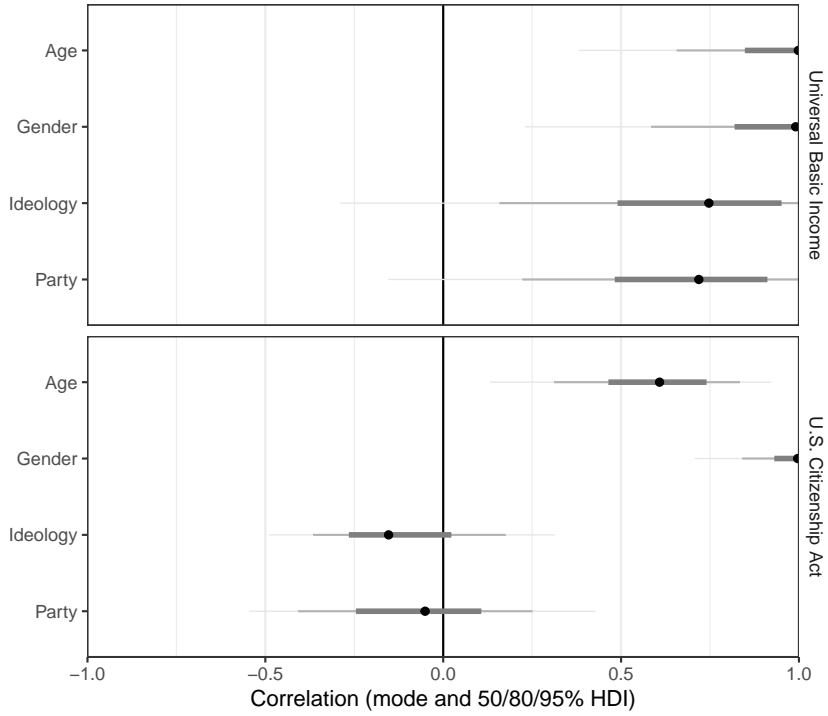


Figure 4: Estimates of unattenuated correlation between CATEs, including both *for* and *against* arguments (in the intended direction of each). Point estimates correspond to the maximum marginal-likelihood estimate, and error bars show 50%, 80% and 95% high density intervals. Note that this estimand is not the *sample correlation* across the specific set of treatments we tested (20 regarding UBI or 39 regarding the U.S. Citizenship Act), but rather the *population correlation* across all hypothetical treatments drawn from the same distribution. Error bars thus reflect some additional uncertainty regarding the representativeness of our specific treatments, and so are wider for the Universal Basic Income experiment which tested fewer distinct treatments.

## 0.5 Rank-heterogeneity estimand for unequal-variances

Our main analysis of rank-heterogeneity in Figure 5 (main text) showed the random effects of treatment-ideology interactions for each treatment, as estimated by our Bayesian model. As random effects, these coefficients adjust for subgroup differences in the average effect size (e.g., a treatment that is 0.15 more effective than average on conservatives and 0.05 more effective than average on liberals would have an interaction random effect of 0.1). A limitation of this analysis as a measure of ‘rank-heterogeneity’ is that it does not adjust for differences in the *spread* of effects between subgroups. For example, if the distribution of effect sizes were much wider for liberals, then a treatment that is +0.1 more effective than average for both conservatives and liberals may still rank much higher in the overall distribution of treatments for conservatives than it does for liberals.

In Figure 5 below we consider a different estimand, the ‘Difference in Z-scored CATEs between liberals and conservatives’. For both subgroups, we estimate the Z-score of each treatment with respect to all other treatments (that is, how many standard deviations each treatment effect is above the mean for that subgroup), and show posterior estimates of the difference in this quantity between conservatives and liberals. In general we prefer this approach as a general measure of *rank-order heterogeneity* as it accounts for subgroup differences in the spread of effects. However, as we find substantively similar results from both methods, we report interaction random effects in the main text, which are more commonplace and so readily interpretable.

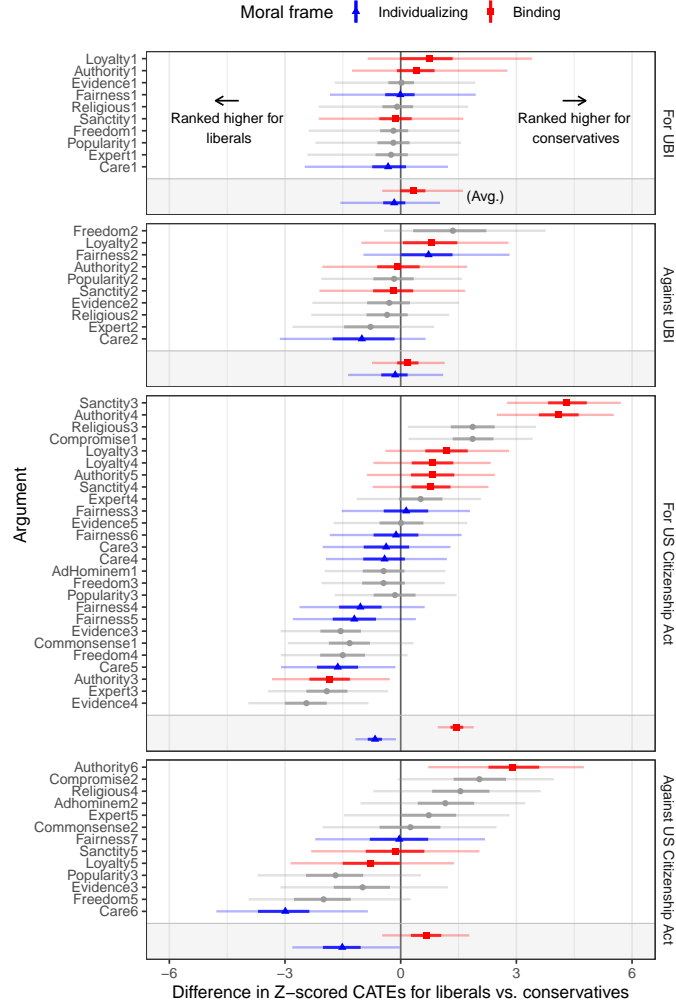


Figure 5: Difference in Z-scored CATEs between liberals and conservatives estimated by the Bayesian multilevel model. Error bars show 50% and 95% Bayesian percentile intervals.

## 0.6 List of arguments tested in UBI experiment

**Authority1** The idea is seen by supporters as a way to implement welfare without encouraging lawlessness or disrespect for authority. The argument is summarised by the Kialo debate forum: “Because a Universal Basic Income is unconditional, benefit fraud will likely decrease. The need to forge documents or misrepresent the truth in order to obtain benefits will become obsolete. There is almost no room for corruption.” Others argue that a Universal Basic Income would increase people’s respect for the Federal government, at a time when so many are losing their jobs due to the pandemic, reducing the risk of rioting and other disruptions to the peace.

**Care1** The idea is seen by supporters as a way to live up to a compassionate ideal that society, as a first priority, should look out for its people’s survival. The COVID-19 pandemic has exposed vulnerabilities of huge populations, and without a social safety net, the social costs can be unbearably high. Universal Basic Income can provide citizens with the money to get necessities like food, to ensure that, at the very least, nobody should be made to go hungry or homeless after losing their jobs.

**Popularity1** Supporters say that the idea is popular with the American public. For example, in a survey conducted by The Hill in August 2020, the majority of registered voters said they thought the government should introduce a Universal Basic Income, including 56% of independent voters. As more and more people hear of the idea, its popularity may increase even further.

**Evidence1** Supporters argue that there is already strong evidence in favor of Universal Basic Income. One U.S. state, Alaska, already has a small-scale version of the policy, and it has been extremely successful. Research has shown that this fund has significantly reduced poverty, as well as health problems such as childhood obesity. Importantly, these benefits came without causing any decrease in Alaska’s employment rate. This evidence suggests that a Universal Basic Income would be beneficial to American citizens, and to the economy, if implemented nationally.

**Expert1** The idea is supported by many expert economists, such as Chris Hughes, a Co-founder of Facebook and Senior Advisor at the Roosevelt Institute. “Automation and globalization have destroyed the employment market. It’s created a lot of part-time, contract, and temporary jobs, but those positions aren’t enough to provide a decent standard of living.” Like many other economists, Hughes believes that Universal Basic Income would be a wise policy for the U.S.

**Fairness1** The idea is seen by supporters as a way to ensure people are paid more fairly for their work. Universal Basic Income would allow workers to negotiate a fair pay and working conditions with their employers, rather than coming from a position of desperation, as many workers hesitate to speak up for fear of losing their family’s livelihoods. It would also provide a fair income to many people doing unpaid work, like parents who stay at home to look after their children, or caregivers providing help for their family members.

**Freedom1** The idea is seen by supporters as a way to give people freedom to make their own choices. Michael Tanner at the Cato institute says: “Universal Basic Income is far less paternalistic than traditional welfare, which often treats the poor like 10-year-olds receiving an allowance. Rather than provide them with cash — which is what poor people actually need — we dole out a variety of specialized benefits, such as food, housing and health care.

Government decides how the poor should budget and spend their money — not the poor themselves.” Michael believes that the poor would be better off with Universal Basic Income, so that they could decide themselves how to spend their money.

**Loyalty1** The idea is seen by supporters as a way to strengthen the nation. In an article for the News Times, Michael Gasparrini writes: “If we truly seek to see our country united and we value the principles of the American Dream, then we must provide families plagued by the misfortunes of COVID-19 with the bootstraps by which to pull themselves up. A Universal Basic Income, presents itself not only as these bootstraps, but as a middle ground rooted in fundamental American values with the ability to unite us. Not only does Universal Basic Income share a symbiotic relationship with capitalism, but it also embodies our belief in the value of community”

**Religious1** The idea is supported by many religious leaders and scholars. In a recent letter from the chief pastor of the worldwide Catholic Church, Pope Francis argues that Universal Basic Income should be implemented to make the world fairer in a post COVID-19 society. The pope endorsed an “unconditional lump-sum payment to all citizens” in the context of his regular calls for an economically and environmentally more equitable post-Covid world.

**Sanctity1** The idea is seen by supporters as a way to restore virtue and sanctity to people’s work. Many people, such as teachers and nurses, choose jobs out of a sense of duty but are forced to leave due to low wages. A Universal Basic Income would enable these people to follow their vocation. Many others work jobs they find demeaning, or which they know to be environmentally damaging, in order to earn enough for a decent life. A Universal Basic Income may free these people, to find a vocation chosen for more pure reasons.

**Authority2** Opponents worry that a Universal Basic Income would cause more people to enter the country illegally. They argue that paying money to all citizens would create the incentive for people in other countries to have their children here in the United States, even if it meant entering illegally, so that their children would then be eligible to claim for Universal Basic Income as adults.

**Care2** Opponents say that Universal Basic Income is cruel, because it would pay everybody the same amount, rather than prioritising the needs of the most vulnerable. Universal Basic Income would likely mean cutting welfare, food stamps, and the Earned Income Tax Credit, redistributing funds away from those that need them more desperately than others.

**Popularity2** Opponents say that the idea is unpopular with the American public. For example, in a survey conducted by Pew Research in August 2020, the majority of people said they would oppose the federal government providing a Universal Basic Income. Fewer than one in four people strongly agreed with the policy.

**Evidence2** Opponents argue that there is already strong evidence against Universal Basic Income. The U.S. federal government ran an experiment from 1968 to 1980 to test a similar policy called “Negative Income Tax”, but the results were disappointing. The program resulted in a drop in working hours, falling as much as 43% for single men not responsible for a family. Stints of unemployment were also prolonged, meaning that after someone lost a job it took them longer to begin a new one. This evidence suggests that a Universal Basic Income could be damaging to U.S. employment.

**Expert2** Universal Basic Income has drawn opposition from many expert economists, such as Daron Acemoglu, the Killian Professor of Economics at MIT. “One should always be

wary of simple solutions to complex problems, and Universal Basic Income is no exception. Much of the enthusiasm for Universal Basic Income is based on a misreading of employment trends in advanced economies.” Like many other economists, Acemoglu believes that Universal Basic Income would be an unwise policy for the U.S.

**Fairness2** Opponents say that Universal Basic Income is unfair, because it would allow somebody to keep being paid without ever having a job, or even searching for one. As Brian Lamacraft writes in an article on medium.com: “I know that when I was a young person, if I was given money for doing nothing, I would be spending it on beer, pizza, movies, and concerts. I would not be thinking of going to my local job office looking for a job. Why should I look for a job when I’ve already been given money from the government? I have no incentive to go looking for a job because I already have my basic needs looked after.”

**Freedom2** As with other systems of welfare, opponents see Universal Basic Income as an infringement on people’s liberty, as it would be funded by taxes on the wealthy. As one contributor to the Kialo debate forum puts it: “Anybody has the moral right to keep what they have worked for and legally obtained. A Universal Basic Income robs people of their labor and private property - money - and gives it to those who haven’t earned it.” They argue that a Universal Basic Income hands more power to a centralized government structure, eliminating individual agency and freedom.

**Loyalty2** Opponents say that Universal Basic Income runs counter to American values. Writing in the Daily Signal, Vijay Menon says: “70 percent of Americans agree that they would enjoy working in a paying job, even if they didn’t need the money. Work is a central pillar of human well-being and happiness.” Like many other Americans, Vijay believes that Universal Basic Income is inconsistent with the American values of dignity and self-sufficiency.

**Religious2** Universal Basic Income has drawn opposition from many religious leaders and scholars such as Professor Sam Brunson, a member of The Church of Jesus Christ of Latter-day Saints. “Few Christians could get behind a stipend program that hurts the people who need it most. Most religions teach that we have a particular duty to the poorest and most disadvantaged” He argues that the Church should oppose the policy on religious grounds.

**Sanctity2** Opponents argue that Universal Basic Income could stain the virtue and sanctity of people’s work. Professor David Cloutier writes “Some worry that Universal Basic Income undermines their belief in the dignity of labor. They think there’s something deeply important and even sacred about working and creating, as vital parts of a flourishing human life.” Writing on the Kialo debate forum, one contributor agrees: “One of Ghandi’s 7 sins is wealth without work. The reason for this is simple: that our lives are enriched by purpose and accomplishment. Helping someone over a hump is one thing, but paying charity over time reduces the quality of life, which is inhuman.”

## 0.7 List of arguments tested in U.S. Citizenship Act experiment

**AdHominem1** Supporters of the bill argue that offering citizenship to people who arrived here as children is just sensible policy, and is only opposed for purely political reasons. “Some politicians are intent on denying these people citizenship in order to keep down the number of hispanic voters, because that’s what they think will most help them win elections. It’s



short-sighted, self-serving, and irresponsible.”

**Authority3** Supporters of the bill argue that granting citizenship to DREAMers, who arrived in the U.S. as children, is required by the 14th amendment of the constitution, and we must respect and obey that. “DREAMers have an immutable characteristic - their country of origin - and live under the same conditions and jurisdictions as American citizens. Therefore, they must be granted the same legal protections under the constitution.”

**Authority4** Supporters of the bill argue that it would help crack down on criminal organizations. “The Act would help law enforcement to prosecute people involved in smuggling, trafficking, and exploitation of migrants. It would also expand the U.S. anti-gang task forces in Central America, and increase sanctions against foreign narcotics traffickers.”

**Authority5** Supporters of the bill argue that many immigrants are just trying to do right by their parents. “Like most Americans, immigrants want to honor their parents and make them proud, especially if their parents have sacrificed a lot to raise them. Coming to America and making a success of themselves is a way for immigrants to honor their parents and to make good on the sacrifices they have made.”

**Care3** Supporters of the bill argue that it would create a more caring and compassionate immigration policy. “Immigrants are simply seeking a better life for themselves and for their families, and most of them already contribute a lot to the nation. They are real and decent people; friends, loved ones and neighbors, and they deserve a chance to become citizens of the USA.”

**Care4** Supporters of the bill argue that it would help protect undocumented immigrants from exploitation by employers. “Millions of undocumented immigrants work in the US, often working in unacceptable conditions for low pay, and living in constant fear of deportation. Giving these people a path to citizenship would help to give those people more protections and rights.”

**Care5** Supporters of the bill argue that breaking up families by deportation is inhumane. “When one member of a family is undocumented and others are not, the Act would help them to stay together. No one deserves to be separated from their family; being able to stay together with the ones that you love is a right that all families deserve, American-born or immigrant.”

**Commonsense1** Supporters of the bill argue that creating a pathway to citizenship is simply common sense. “It would be unrealistic to deport all of the 11 million undocumented immigrants in America, whose lives, jobs, and housing are already deeply intertwined with the U.S. economy. Allowing those immigrants to one day become citizens is the only sensible option.”

**Compromise1** Supporters of the bill argue that it is a fair compromise which is progressive, but moderate. “While the bill provides different paths forward for immigrants who want to become citizens, it would still take eight years for an undocumented immigrant to become a permanent citizen, during which time they would not have rights to government benefits or employment as do those who are in the U.S. legally.”

**Popularity3** The major provisions of the bill are very popular among the American public. For example, a recent poll by Ipsos found that 7 out of 10 Americans would support offering a path to citizenship to undocumented immigrants living with Temporary Legal Status. This included strong majorities of Republican, independent, and Democratic voters.

**Evidence3** Supporters of the bill argue that immigrants help to drive innovation. More

than three-quarters of patents generated at top American universities involve a migrant inventor. Migrants are also twice as likely to start a business as locals. In fact, around half of Silicon Valley startups, including Google, LinkedIn, Tesla and Stripe, were co-founded by immigrants. This evidence suggests that the U.S. Citizenship Act would benefit the U.S. economy.

**Evidence4** Supporters of the bill argue that immigration helps to safeguard Social Security. In 2010, undocumented individuals paid \$13 billion into retirement accounts, and only received \$1 billion in return. Over the years, immigrants have contributed up to \$300 billion to the Social Security Trust Fund. Without the contributions of immigrants going into the system, it is estimated that full benefits would run out in the year 2037. This evidence suggests that the U.S. Citizenship Act can help protect Social Security for the future.

**Evidence5** Supporters of the bill argue that immigration helps to protect against an aging workforce. Since the year 2000, the U.S. prime working age population - between 24 and 65 years old - increased by more than 23.6 million people. Immigration was vital to this robust growth, with almost half of the total growth coming from immigrants. This evidence suggests that the U.S. Citizenship Act could protect the U.S. economy as its population grows older.

**Expert3** Many expert economists are in favor of providing undocumented immigrants a path to citizenship, such as Robert Lynch, Professor of Economics at Washington College. “As our study demonstrates, a road map to citizenship for the unauthorized will bring about significant economic gains in terms of growth, earnings, tax revenues, and jobs. The sooner we provide legal status and citizenship, the greater the economic benefits are for the nation.”

**Expert4** The bill is supported by many expert economists, who argue that increasing the number of high-skilled immigrant workers is vital to U.S. growth. “Frankly, there is little debate to be had. The evidence overwhelmingly indicates that high-skilled immigrants do not steal American jobs. Employer demand for H-1B workers far outstrips supply; there were 236,000 applications for just 85,000 available visas in 2016.”

**Fairness3** Supporters of the bill argue that DREAMers were brought to the U.S. as minors, and it would be unfair to deport them. “DREAMers were children when they were brought to the U.S. and are therefore victims of crime; they are not adults who committed crimes willingly. They should not be punished for the actions of those who brought them here.”

**Fairness4** Supporters of the bill argue that many undocumented immigrants deserve to be granted a path to citizenship because they have contributed greatly during the COVID-19 pandemic. “An estimated five million undocumented workers are serving in essential roles as front-line workers during the pandemic, from healthcare to transportation to agriculture to food services and delivery. Their work carried great personal risk of contracting the disease and the Act would fairly recognize their contributions to the U.S.”

**Fairness5** Supporters of the bill argue that undocumented immigrants deserve to have a pathway to citizenship because they are already contributing to society. “According to the Congressional Budget Office, 50% to 75% of undocumented immigrants already pay federal, state, and local taxes. Many choose to do this, hoping that it will help them become citizens in the future.”

**Fairness6** Supporters of the bill argue that the United States created the conditions that led immigrants to seek out new lives here. “International economic policy enacted by the

U.S. has caused the displacement of many people, such as Mexican farmers. These people should have a right to better their lives with the aid of the country ultimately responsible for their migration in the first place.”

**Freedom3** Supporters of the bill argue that freedom to work is a natural right, and employers ought to be able to enter into contracts with any employees they please. “The government doesn’t own the country, and political borders are just lines on a map. Treating law-abiding people like criminals, simply because they didn’t meet the bureaucratic requirements of migration, abrogates Americans’ natural right to freely associate and make contracts.”

**Freedom4** Supporters of the bill argue that freedom of movement is a natural right and is undermined by too much government interference. “The idea that immigration needs to be “authorized” by the government flies in the face of that freedom. Immigrants who come to America seeking the opportunity to work, and pursue happiness, ought to be able to stay to pursue those opportunities.”

**Loyalty3** Supporters of the bill argue that the United States is founded on immigrants, and has only grown stronger over the years through strong immigration programs. “Just as the Statue of Liberty declares on her plaque, “Give me your tired, your poor, your huddled masses yearning to breathe free...”, the U.S. Citizenship Act is a continuation of this principle, and the people protected under the Act are a staple of this country.”

**Loyalty4** Supporters of the bill argue that it will help America continue to prosper in its role as a world leader. Bob Menendez, the chairman of the Senate Foreign Relations Committee, says the bill will modernize our immigration system, and ensure America remains a powerhouse for innovation and a beacon of hope to refugees around the world.

**Religious3** Many people have voiced support for the Act on religious grounds. “Christian belief, which holds to a high view of every person’s intrinsic worth, should motivate us to assist those who are in need. There are direct biblical commands telling God’s people to welcome and honor the strangers and immigrants in their midst.”

**Sanctity3** Supporters of the bill argue that smart improvements to our border security are necessary. “We need to do more to keep our country clean and secure, for example to prevent the flow of dangerous drugs into the United States. Drugs are a daily plague on the American population, and large quantities of drugs are imported across the U.S. border every year.”

**Sanctity4** Supporters of the bill argue that a founding principle of America is opportunity. “America is the land of opportunity. If we don’t at least provide immigrants the opportunity to make a better life for themselves, we are polluting the American character, allowing our history and traditions to be tainted by short-sightedness and our decency to be corrupted by a moral rot.”

**Adhominem2** Opponents argue that the bill is a dishonest proposal by self-interested politicians. “Some politicians only want to offer citizenship to illegal immigrants in order to hand them a vote. Those politicians know that an amnesty will benefit them electorally. Meanwhile, ordinary people will lose out.”

**Authority6** Opponents of the bill argue that illegal immigrants have disrespected the rule of law, and so they need to be punished, rather than rewarded. “Providing an amnesty to people who came here illegally would seriously undermine the Rule of Law in the United States. The law is there for a good reason, and it is important that it is respected and

upheld.”

**Care6** Opponents of the bill argue that, in the long term, it is more humanitarian to reduce immigration to the U.S. as too much immigration can lead to a “brain-drain” on other countries where the most highly-skilled workers leave. “By staying in their homeland, immigrants would be better able to grow and support it, which will help develop their own countries more in the long run.”

**Commonsense2** Opponents of the bill argue that it would be short-sighted to grant citizenship to illegal immigrants, as it would set a bad precedent to future generations. “If we reward illegal immigration, it is simply common sense that we will get even more of it in the future. We have to take a hard stance now in order to prevent future migration crises from happening.”

**Compromise2** Opponents say that the bill is extreme, and does not represent the political middle-ground. The Heritage Foundation, arguing against the Act, writes “The U.S. Citizenship Act of 2021 is the most radical piece of immigration legislation ever introduced in America and seeks to reward illegal aliens at the expense of American citizens.”

**Popularity3** Many provisions of the bill are unpopular with the American public. For example, a recent poll by Rasmussen Reports found that fewer than 4 out of 10 Americans would support giving lifetime work permits to most of the estimated 12 million illegal residents.

**Evidence3** Opponents of the bill argue that being soft on immigration will lead to more crime. “Undocumented immigrants are at least 142% more likely to be convicted of a crime than other residents of Arizona. If undocumented immigrants committed crime nationally as they do in Arizona, in 2016 they would have been responsible for over 1000 more murders and 26,900 burglaries.”

**Expert5** Many expert economists are opposed to any increases in U.S. immigration, such as Sir Paul Collier, director of the International Growth Center. He worries that immigrants, bringing institutions of their homelands, will do harm to the U.S. “Migrants are essentially escaping from countries with dysfunctional social models. The cultures of poor societies, along with their institutions and organizations, stand suspected of being the primary cause of their poverty.”

**Fairness7** Opponents of the bill argue that providing an amnesty to illegal immigrants is grossly unfair to people who have stayed in their home country and are waiting to enter the U.S. through the legal process. “By entering a country illegally and gaining benefits, illegal immigrants “cut the line” in front of those who deserve to be here first.”

**Freedom5** Opponents say that the bill is far too large, and is an overreach of the federal government into local matters. “States at the U.S. border should have more freedom to make their own decisions about immigration policies which have an outsized impact on their communities. The rest of the U.S. is less directly impacted by illegal immigration and so the communities local to this problem should have more of a say in the policy.”

**Loyalty5** Opponents say that the bill hurts ordinary Americans. They argue that giving the benefits of citizenship to illegal immigrants would put a strain on the resources for those who are legally in the U.S. “Natural-born citizens deserve preferential treatment for American jobs, healthcare, education, welfare, and other services.”

**Religious4** Many people criticize the bill on religious grounds. Luis Cardenas, the chaplain for Grand View University, argues that illegal immigrants have committed a moral

violation and should be required to pay restitution before seeking citizenship. “The Apostle Paul makes clear that God has established government to maintain order. Breaking the law is a crime, and governments are right to enforce the law against criminals, foreign or domestic.”

**Sanctity5** Opponents of the bill argue that we need tougher limits on immigration, now more than ever, because of the coronavirus pandemic. “While many countries are failing to contain the virus, the U.S. should be limiting immigration to prevent an influx of new and more contagious strains of the virus into this country.”