Sources of educational inequality and redistributive behaviour: Experimental evidence*

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Abstract

This paper examines how beliefs about the relative importance of factors beyond an individual's control ("fixed" factors) affect support for policies or initiatives that reduce education-related inequalities. In an online survey on a demographically diverse US sample (N=2,000), we find that providing information about the extent and source of inequalities in 4-year college attendance: (1) strengthens participants' beliefs in the role of fixed factors in educational inequalities, (2) positively affects real donation behavior, and (3) increases stated support for pre-college policies aimed at students from low-income families but not post-college redistribution.

JEL codes: D63, D64, D83, I24.

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1 Introduction

> *"A four-year degree has become the key marker of social status... The elite can sometimes be smug about their accomplishments, attributing them to their own merit, and dismissive of those without degrees, who had their chance but blew it. The less educated are devalued or even disrespected, are encouraged to think of themselves as losers, and may feel that the system is rigged against them."*
>
> \- Case and Deaton (2021)

Education is an important determinant of economic outcomes, but access to education - particularly a 4-year college degree - is highly unequal. Studies of inequality in educational attainment reveal a puzzle: people recognize educational inequalities but have limited support for policies that tackle such inequalities. On the one hand, college attendance varies considerably with factors beyond an individual's control, particularly parental income and parental educational attainment.[1] In the US, 54% of children born into the top income quartile obtain a bachelor's degree, compared to only 9% of those in the lowest quartile (Bailey and Dynarski, 2011), and several studies have shown that family background matters even after conditioning on test scores and other measures of pre-college credentials (Chetty et al., 2020b; Dynarski et al., 2021). On the other hand, there is limited support for college admissions policies that account for parental income or education. Instead, the vast majority of Americans believe that high school grades and test scores should be the determining factors in college admissions (Pew Research Center, 2022), even though these vary substantially and systematically with parental income (Dixon-Román et al., 2013; CollegeBoard, 2022).[2]

This paper explores whether beliefs about the sources of educational inequalities could help explain this puzzle. We classify sources of educational inequalities on a spectrum of "fixed" (outside one's control) to "malleable" (within one's control), in recognition of the fact that many factors are not completely "fixed" or "malleable" but a mixture of both.[3] Inequalities that arise from fixed factors, such as race, gender, and parents' socioeconomic status, are generally perceived as "unfair" whereas inequalities that arise from malleable factors, such as hard work, are generally seen as "fair" (Benabou, 2000).[4] If people believe that the decision to attend college is primarily driven by malleable factors such as hard work, they would view existing college attendance inequalities as fair and thus be unwilling to support policies or initiatives that reduce such inequalities, especially those that account for fixed factors. We therefore ask: how do beliefs about the sources of educational inequalities causally affect support for related policies or initiatives?

To cleanly identify the causal link between beliefs about the sources of educational inequalities and our outcomes of interest, we conduct an online survey experiment on a demographically diverse sample of over 2,000 Americans. We exogenously vary the information that participants receive about the extent and source of educational inequality. The treatment group is given information from Chetty et al. (2020b) on how and why 4-year college attainment depends on parental income (a factor beyond an individual's control), while the control group is given unrelated information about college students.

After the information treatment, we measure participants' beliefs about the sources of educational inequality, by asking participants about the relative importance of "fixed" versus "malleable" factors in explaining differences in college attendance. We then examine the effect of the treatment on participants' support for policies and initiatives that reduce education-related inequalities. Specifically, we focus on the following outcomes: a behavioral measure (real donations of potential survey earnings to the National College Attainment Network, a national charity that tackles inequalities in college attainment), self-reported support for policies that address barriers to applying to or attending college ("ex-ante" policies), and a policy that addresses post-college economic inequalities ("ex-post" redistribution).

We explore two key mechanisms for potential treatment effects. First, information provision has a "direct effect" on beliefs about the sources of educational inequalities (existing literature that use information provision treatments focuses on this effect). Second, information provision may have an "indirect effect" on stereotypes about the less-educated (those without a 4-year college degree). An emerging literature in psychology finds that individuals with a college degree are evaluated much more positively than those without, even in contexts where this distinction is irrelevant (Kuppens et al., 2018).[5] Information about the importance of fixed factors in determining college attendance may weaken negative stereotypes about the less-educated as people who "had their chance but blew it" (Case and Deaton, 2021). We use a novel adaptation of the Implicit Association Test (IAT) to measure implicit stereotypes about the less-educated (on a positive-negative spectrum), and numerical scale questions to measure explicit attitudes towards the less-educated. The IAT is a widely used measure in psychology, and meta-studies have confirmed its reliability and validity in predicting real inter-group behaviour (Greenwald et al., 2009).

Finally, to address concerns about experimenter demand effects (De Quidt et al., 2018), we conduct an obfuscated follow-up survey two weeks after the initial survey to measure the same beliefs, outcomes, and stereotypes about the less-educated.

There are two reasons why it is important to study the causal effect of correcting misperceptions about the sources of educational inequalities on support for policies and initiatives that reduce education-related inequalities. First, policies that address education-related inequalities need taxpayers' support. The notion of equality of opportunity is prevalent in the US. Nationally representative surveys find that almost three-quarters of Americans believe hard work, rather than luck or help from others, is more important for success (Pew Research Center, 2022) and this belief is especially common for educational attainment (Case and Deaton, 2021; Sandel, 2020). However, there is growing evidence that fixed factors, especially parental income, play a large role in determining educational attainment. In the US, low-income students are much less likely to attend college compared to high-income students, even controlling for other factors: around two-thirds of the variation in college enrollment rates can be attributed to parental income rather than differences in academic preparation, race, or geography (Chetty et al., 2020b).[6] Informing the public of the crucial role that fixed factors play in determining educational attainment could increase their willingness to support these policies, making it more likely that they would be implemented in practice.

Second, it is important to understand how to overcome barriers to social cohesion. One of the deepest ideological political divides in Western politics is between those with and without a college degree, with education being one of the strongest predictors of recent political events like Donald Trump's election and Brexit (Goodwin and Heath, 2016). This "BA-divide" is not solely explained by the relative economic discontent of the less-educated, but instead is partly due to the negative stereotypes that college graduates have of non-college graduates (Sandel, 2020; Case and Deaton, 2021). These stereotypes may arise from underestimating the role of fixed factors in determining educational outcomes. Besides correcting misperceptions, information provision can change these stereotypes. Understanding how information provision affects policy support and real behaviour is important for designing effective low-cost interventions.

Our paper has three main findings. First, we find that most participants underestimate the extent of inequality in college attendance. We elicit these beliefs by asking participants what percentage of college attendees grew up in a "low-income" and "high-income" household, where "low" and "high" refer respectively to the bottom and top quintiles of parental income. The main source of misperceptions comes from underestimating the percentage of college attendees from high-income households.

Second, informing participants about the extent and source of inequality strengthens their beliefs in the relative importance of fixed factors as the source of educational inequalities, and increases their support for equality-enhancing policies and initiatives. Treated participants increase beliefs in the relative importance of fixed factors by 0.53 standard deviations (SDs) ($p<0.01$) compared to non-treated participants. The information treatment affects real behavior, with treated participants donating more money from their potential survey earnings to a higher-education charity in both the main survey (0.29 SDs, $p<0.01$) and follow-up survey (0.24 SDs, $p<0.01$). The effects on real behaviour last for at least two weeks after the experiment, ruling out experimenter demand and salience effects. The information treatment is also effective in increasing support for ex-ante policies (0.22 SDs, $p<0.01$) but not ex-post redistribution.

Third, the observed treatment effects are largely due to the direct effect of information provision on beliefs about the sources of educational inequality (conscious information processing), rather than the indirect effect of changing stereotypes (heuristic decision-making). We find that most participants have negative implicit and explicit stereotypes about non-college graduates. Notably, these negative stereotypes are stronger among college graduates. Furthermore, explicit negative attitudes towards non-college graduates are stronger than those against Blacks and females, suggesting that these attitudes are more socially acceptable than racism or sexism. Despite the prevalence of negative stereotypes and attitudes, mediation analysis indicates that a negligible percentage (less than 1%) of the total treatment effect for each outcome is mediated by implicit stereotypes about the less-educated. Given the reliability of the IAT as a measure of stereotypes (Glover et al., 2017; Greenwald et al., 2009), we can therefore rule out stereotypes as an indirect mechanism for our results. The fact that the information treatment changes behavior and policy preferences primarily via beliefs suggests that it can be effective to appeal to participants' cognitive reasoning processes rather than their heuristic decision-making processes.

Our findings contribute to the literature in three ways. First, we build on studies that examine how providing information about the extent of educational inequalities causally affects policy preferences (Lergetporer et al., 2020; Alesina et al., 2018b) and real behaviour (Grewenig et al., 2022). These studies are part of a broader literature on the causal impacts of information provision about the extent of inequality (Hvidberg et al., 2020; Kuziemko et al., 2015). Rather than focus on correcting misperceptions about the *extent* of inequality, as existing papers do, we also seek to correct misperceptions about the *sources* of inequality (fixed vs malleable factors). Our information treatment affects real behavior for at least two weeks after the experiment, which suggests that "cheap" interventions could be an effective way to increase actual (and not just stated) support for inequality-reducing policies.

Second, we build on the literature that examines the causal link between beliefs about the sources of income inequality and preferences for redistribution. This relationship has been documented with survey data (Fong, 2001; Alesina and La Ferrara, 2005) and in controlled experiments that exogenously vary the source of income inequality between workers ("effort" or "luck" (Almås et al., 2020; Andre, 2021; Cappelen et al., 2022)).[7] These studies all focus on broad beliefs about the sources of economic inequality rather than inequalities in educational attainment specifically. By focusing on this key source of income differences, our study can assess support for ex-ante policies as well as the ex-post redistributive measures that the existing literature centers on. We also build on this literature by documenting the public's awareness and beliefs about the sources of educational inequalities specifically.

Third, our investigation of stereotypes as a mechanism for treatment effects adds to studies that measure implicit stereotypes and relate them to economic outcomes (Beaman et al., 2009; Glover et al., 2017; Carlana, 2019; Alesina et al., 2018a). We construct a novel adaptation of the IAT and find that most participants have implicit negative stereotypes against the less-educated, which is consistent with emerging literature in psychology (Kuppens et al., 2018). In contrast to the existing literature that uses explicit measures of attitudes towards specific groups in society, our implicit measure is likely to be less prone to social desirability bias. To explore stereotypes as a mechanism, we conduct causal mediation analysis (Keele et al., 2015) to estimate the size of the "indirect effect" (information provision affects stereotypes used to make heuristic decisions) compared to the "direct effect" (information provision affects the knowledge that individuals use to make conscious cognitive decisions). In contrast to much of the previous literature, we can therefore explain *why* our information treatment works, not just *whether* it works. In our context, the size of the indirect effect is negligible but the longer-lasting treatment effects we observe for real behavior are promising as they suggest that support for inequality reduction may not require people to overcome their negative stereotypes about the less-educated.

The rest of this paper is structured as follows. Section 2 provides details of the survey design. Section 3 discusses data collection and descriptive statistics. Section 4 discusses the effect of information provision on beliefs about the sources of educational inequalities and the causal effects of shifting beliefs in the relative role of fixed and malleable factors. Section 5 explores how implicit stereotypes about the less-educated vary across participants and examines the effects of the information treatment on stereotypes. Finally, Section 6 concludes and explores further avenues of research.

2 Survey design

We first measure beliefs about socioeconomic inequalities in college attendance, and randomly provide half of the participants with the correct information about the extent and sources of this inequality. We then measure participants' beliefs in the relative importance of fixed (vs malleable) factors in determining college attendance, support for policies to address education-related inequalities, donations to education-related charities, and stereotypes about non-college graduates. For the full description of the experiment design and survey instructions, see Appendices E-G. Figure A1 summarizes the structure of our main survey.

2.1 Information treatment

To create exogenous variation in beliefs about the role of fixed factors in college attendance, we conduct a randomized information treatment. Treatment group participants are provided with information on the extent and source of inequalities in college attendance. Control group participants are provided with unrelated information.

The information we present the treatment group is from Chetty et al. (2020b), who use de-identified administrative data that links college-related outcomes with parental household income. We chose this study because it considers the influence of an unambiguously fixed factor (parental income when the child was 17 years old) on college attendance, while accounting for important malleable factors (e.g. SAT/ACT scores). This "ceteris-paribus-type" analysis allows us to comment on both the extent and sources of educational inequalities, conditional on malleable factors.[8] Although Chetty et al. (2020b) present statistics for various college types and length of degree program, we focus on 4-year college degrees from all college types, as the returns to education are commonly measured by comparing the wages of 4-year college-degree-holders with high-school-diploma holders (Fortin, 2006). Furthermore, a specific focus on 4-year college degrees would avoid measurement error that arises from differences in perceptions between 2-year and 4-year college degrees.

We use a mixture of figures and text to present the extent and source of educational inequalities. To show the *extent* of educational inequalities, we show treatment participants the infographic presented in Figure 1, which is based on statistics from Table VI of Chetty et al. (2020b).[9] The infographic shows that out of 100 randomly selected 4-year college attendees, 8 grew up in a low-income household and 37 grew up in a high-income household. We also provide an interpretation of these numbers: there are roughly 5 times more high-income than low-income college attendees.

To show the *source* of educational inequalities, we present some text explaining why parental income limits the opportunities of low-income students to attend college, even after controlling for differences in academic preparedness for college. This explanation includes concrete examples of why low-income students who have good pre-college credentials might face barriers to attending college, such as not being able to pay tuition fees.[10] We purposely use "ceteris paribus" statements that hold academic preparedness fixed because participants' beliefs about the actual distribution of college attendance may partly depend on their beliefs about differences in ability or effort across quintiles.[11]

In the control group, participants receive information that is unrelated to educational inequalities. In particular, participants are told the average employment rate of college attendees at age 30 and the different types of colleges that this cohort attended, with statistics also taken from Chetty et al. (2020b).[12] We check participants' comprehension of the information they received. Overall, 97% of all participants passed the comprehension check, with roughly the same percentage in control and treatment groups.

Overall, our information treatment shows: (1) how unequal college attendance is across parental income groups, (2) how college attendance depends on fixed factors, even conditional on malleable factors such as pre-college academic credentials, and so (3) the observed inequality in educational outcomes is, at least partly, unfair. Since our treatment aims to compare beliefs about college attendance with the actual distribution (rather than with a hypothetical distribution where college attendance only depends on malleable factors), our information treatment does not take a stance on what the distribution of college attendance "should" be across parental income quintiles.[13]

Before the treatment, we ask both groups of participants a beliefs elicitation question, so that we can conduct heterogeneity analysis by prior beliefs. We ask participants to report how many, out of 100 random individuals who attended a 4-year college, grew up in a "low-income", "below-middle", "middle-income", "above-middle", and "high-income" household, where each of these income groups corresponds to a quintile of the parental income distribution as specified by Chetty et al. (2020b).[14] To reduce experimenter demand effects and avoid priming, we do not explicitly ask about beliefs about the sources of educational inequalities before the treatment. Instead, we elicit pre-treatment beliefs about the extent of educational inequality, which we show is correlated with beliefs about the sources of educational inequality. We also ask all participants to rate their confidence in their answers for the low-income group and high-income group, on a 1-5 scale.

2.2 Post-treatment beliefs about the sources of educational inequality

After the information provision, we measure beliefs about the sources of educational inequality by asking participants about the relative importance of "fixed" versus "malleable" factors in explaining differences in college attendance. The exact wording we use is: "Consider two groups of individuals. All individuals in group 1 attended college. All individuals in group 2 did not attend college. How important are the following factors in explaining this difference in college attendance between groups 1 and 2? 1) Fixed factors: Factors that are fixed at birth (e.g. whether they are born into a rich or poor household); 2) Malleable factors: Factors that are not fixed at birth (e.g. their mindset towards hard work)."

Participants use a slider to indicate the relative importance of these two factors. In line with previous literature, we treat the role of "fixed" and "malleable" factors as independent as it is difficult to empirically infer the potential correlation or endogeneity between the two (for example, the extent to which a disadvantaged family background may discourage students from exerting effort). There is also experimental evidence that people neglect this interplay between choices and circumstances when evaluating outcomes (Andre, 2021).

This method of measuring post-treatment beliefs has several advantages. First, we reduce experimenter demand effects, since this point in the survey is the first time we explicitly ask about beliefs in the sources of educational inequality. Second, it is consistent with existing conceptualizations of the sources of inequality. For example, Benabou (2000) mathematically decomposes the variation in outcomes that is due to variation in "intrinsic" qualities (defined as those that an individual can change), and factors that an individual cannot change. Third, the question avoids being prescriptive about what factors determine "deservingness". The question wording suggests that the development of malleable factors may be within an individual's control and therefore they may be "responsible" for them but is not prescriptive about what counts as malleable factors.[15]

Since definitions of "fixed" and "malleable" may vary across participants, we ask participants to indicate, on a 0-100 scale, the extent to which they think the following factors are within an individual's control: how hardworking an individual is, an individual's ambition, SAT scores, the job an individual has, intelligence, social connections, the neighborhood an individual is born in, an individual's race. These factors are taken from the International Social Survey Program and have been used by sociologists to measure beliefs in determinants of success, broadly defined (Mijs, 2021). Appendix Figure A2 shows participants have similar beliefs about the "malleability" of each factor, with neighborhood and race being (correctly) perceived as factors that an individual does not have control over, while hard work and ambition are seen as factors that an individual has control over.[16]

2.3 Main survey outcomes

We assess the impact of our information treatment on the following outcomes. To minimize order effects, we randomize the order in which participants answer these questions.

Real behavior. To maximize the external validity of our results, aside from self-reported support, we collect a behavioral measure: real donations to the National College Attainment Network (NCAN, `https://www.ncan.org`), a reputable charity whose primary mission is to tackle inequalities in educational attainment at the tertiary level. One of the NCAN's core activities is to provide information sessions and funding for cities to help high-school students complete their Free Application for Federal Student Aid (FAFSA), which enables students to access funding for postsecondary education, including the Pell Grant (a federal need-based award that does not need to be repaid).[17] Participants are told they have been automatically enrolled in a lottery for $100 and, if they win, they can choose to donate some (or all or none) of their winnings to the NCAN.[18] Participants then indicate how much of their winnings they would like to donate.

We chose the amount $100 because of its relatively large size compared to the participants' payment for completing the survey (winning the lottery would increase their survey earnings by more than sixteen times), while still being an amount that individuals are likely to encounter in daily expenditure decisions (compared to larger amounts such as $500 or $1,000). Given the size and scale of NCAN's operations, $100 would also constitute a meaningful donation.[19] The personal consequences involved in the donation decision - giving up potential survey earnings to make a donation - ensures external validity.

Ex-ante policies. Participants are asked about their extent of support for a policy that reduces financial barriers to college attendance. Each participant is randomly given one of these two policies: (1) Expanding the Pell Grant, the federal government's financial aid program for low-income students who need help to pay for college costs (vs spending the federal budget on something else); (2) Automatic exemptions of college application fees for low-income students (vs charging every applicant the same to cover administrative costs of reviewing applications, which is the status quo).

We provide participants with a brief description of the policy, information about its benefits, and information about the alternative option (stated above). Providing information about the alternative options ensures that participants are aware of policy trade-offs and the possible fiscal implications of supporting these policies. We choose to make these trade-offs salient to improve the external validity of our stated policy support results, as such trade-offs feature prominently in real-world policy debates.

Ex-post redistribution. For the ex-post policy question, we ask participants about their desired earnings ratio for a typical college graduate and non-college graduate. To benchmark responses, participants are given the current average earnings ratio (100:173), calculated using data from the Bureau of Labor Statistics.[20] We then ask them whether this difference is too small, just right, or too large. Participants who answer "too small" or "too large" are asked to provide a number for their desired earnings ratio, which in effect constitutes a redistribution of income between college graduates and non-college graduates.

We chose to focus on policies related to redistribution of income rather than other endowments resulting from a college degree (such as wealth) as income taxes comprise the largest percentage of US government revenue by far.[21] Also, much of the literature on economic inequality and policy preferences focuses on income tax policies or income redistribution.[22]

2.4 Alternative mechanisms

We explore two mechanisms through which information provision can affect behavior: beliefs about the sources of educational inequalities, and stereotypes about the less-educated. The conceptual underpinning of these mechanisms is the dual process theory of human reasoning, which is used in the psychology literature to distinguish between two types of thinking: fast and heuristic-based ("System 1"; stereotypes) vs slower and consciously controlled ("System 2"; beliefs derived from information) (Evans, 2008; Kahneman, 2011).

We collect measures of beliefs in the sources of educational inequality as described in Section 2.2. To measure heuristic-based reasoning (stereotypes about the less-educated), we construct a novel adaptation of the Implicit Association Test (IAT). The IAT is a computer-based tool developed by psychologists (Greenwald et al., 1998) and recently adopted by economists to measure gender and racial discrimination (Alesina et al., 2018a; Carlana, 2019). The IAT aims to measure stereotypes that participants may not be explicitly aware of, or are unwilling to express. There is evidence that the IAT is difficult to manipulate (Glover et al., 2017; Greenwald et al., 2009).

In our IAT, participants rapidly categorize words according to two sets of criteria: positive/negative valence and college/non-college graduate. For the positive/negative list, we use the same words as those in the standard IAT (for example, "happy" as a positive word and "gloom" as a negative word). For the education-related list, we create our own list of words, each consisting of a name that either does or does not include an education-related abbreviation (BSc, J.D., MBA, MSc, M.D., PhD). Appendix B provides the further details of our IAT, including the full list of words used.

Our IAT consists of seven categorization tasks, including practice tasks. During each categorization task, the participant sees a word in the middle of the screen. The top left and right of the screen show categories that the word can be grouped into (e.g. "college grad" on one side and "non-college grad" on the other side). Participants must categorize these words to the left or right-hand side of the screen as quickly as possible, and we record their response times (in milliseconds) for each word.

Our measure of implicit stereotypes about the less-educated is the participants' score on the IAT, known as the d-score. The d-score is the normalized difference in average response times between categorization tasks that use "stereotypical" pairs of concepts (in this case, college/good, non-college/bad) and tasks that use "non-stereotypical" pairs of concepts (Greenwald et al., 2003). The intuition behind this measure is that participants with a stronger association between two concepts find that sorting task easier and complete it faster. Larger d-scores (in absolute value) indicate stronger implicit stereotypes about the less-educated, where *positive* values indicate *negative* stereotypes about the less-educated. For interpretability, we standardize the d-scores by dividing raw IAT scores by the pooled standard deviation in our sample.

To provide a point of comparison for our novel implicit measure, we also measure explicit attitudes towards the less-educated using a "feeling thermometer" question, where participants are given a 0-100 scale and asked how "cold" (=0) vs "warm" (=100) they feel towards the following groups: college graduates, non-college graduates, males, females, White Americans, and Black Americans.[23] Following the psychology literature (Kuppens et al., 2018), we measure the strength of negative attitudes towards non-college graduates as the difference between reported feelings towards college graduates vs non-college graduates. To provide a benchmark for comparing attitudes toward the less-educated, we calculate analogous measures for males/females and White/Black people.

An important point to note is that both the implicit and explicit measures indicate general preferences to associate with or view particular groups favorably in a social context, rather than preferences over characteristics needed for a specific economic context. In particular, these measures do not capture labor market preferences to hire educated candidates (who may be more qualified to do a certain job) over less-educated candidates.

2.5 Other characteristics

We ask participants information about their socioeconomic background (e.g. age, gender, income, education) and questions to assess a participant's own "fixed factors", including their parent or guardian's highest educational attainment. We also collect information on whether participants consider particular socio-demographic factors (such as ethnicity or education level) to be important for their identity, a version of the locus of control measure (Cobb-Clark and Schurer, 2013), and a numeracy test (Lipkus et al., 2001).

2.6 Follow-up survey

While we designed our main survey to minimize concerns about experimenter demand effects, we further mitigate such concerns by conducting a follow-up survey two weeks later, in which no additional information is provided. We chose a two-week gap between the main and follow-up surveys to balance the trade-off between testing for demand or salience effects and ensuring a reasonable recontact rate. After asking the obfuscation questions, we measure beliefs about the sources of inequality in college attendance using the same question as the main survey, but use different questions to measure our main outcomes of interest. Appendix A contains further information on how we obfuscate the questions in the follow-up survey.

3 Sample and summary statistics

3.1 Data collection and sample characteristics

We recruited a demographically diverse sample of participants using Prolific, a reputable survey company used primarily by researchers for surveys and experiments. Compared to in-person data collection methods or similar platforms such as MTurk, Prolific has been shown to deliver higher or comparable data quality (Peer et al., 2017). To be eligible, participants had to be aged 25 and over (so they would likely have finished formal education and are now in the labor force), and normally reside in the US.

The main survey was conducted in July 2022, and the follow-up survey was conducted roughly two weeks later. Of the 2,069 participants contacted by Prolific, 2,008 consented to begin the survey and completed the full questionnaire. 1,674 participants completed the follow-up survey, which corresponds to a recontact rate of 83.4%. All study participants were paid $8 per hour (in line with Prolific's principle of "ethical rewards") for their participation, and were also told that they could win monetary bonuses for some survey questions.

Appendix Table A1 compares the characteristics of our main sample with equivalent statistics from the American Community Survey (ACS) and the General Social Survey (GSS). Since younger and more-educated participants are over-represented in our sample, in all our regression analyses, we apply weights to match those of the ACS, so that our weighted main sample is representative of the US population in terms of gender, age, racial-ethnic group, and educational attainment (columns 2-4).[24]

Our sample is balanced across treatment and control groups for a set of key characteristics and pre-treatment beliefs about inequality in educational attainment (Appendix Table A2), and our follow-up sample achieves balance across treatment and control groups for the same variables (Appendix Table A3). Appendix Table A4 shows that none of the key participant characteristics (gender, age, ethnicity, education, political affiliation, income, region of residence) jointly or individually predict assignment into the treatment group, though older participants and college graduates were more likely to complete the follow-up survey. To account for potential bias arising from this differential attrition, we re-weight our longitudinal sample using inverse probability weights, based on the marginal distribution of age, gender, race, and education. Specifically, we run a probit regression where the outcome variable equals 1 if the participants completed both surveys and zero otherwise, using the characteristics in Appendix Table A4 as covariates. We use these probit estimates to obtain predicted probabilities of each participant completing both surveys, conditional on socio-demographic characteristics. We use the inverse of the predicted probability as that participant's weight.

3.2 Beliefs about the extent and sources of educational inequality

Our main measure of beliefs about educational inequality is the ratio of the participant's elicited beliefs about the percentage of college attendees who grew up in a high-income household to the percentage of college attendees who grew up in a low-income household.[25] Although the correct ratio is 4.63 (=37/8), we define underestimation as having a ratio of less than 5 because we provide this number to participants in the treatment infographic.

Appendix Figure A3 summarizes average beliefs about the parental income distribution of college attendees, measured before the treatment. By this measure, the majority of participants (51%) underestimate the extent of inequality in college attendance between high-income and low-income students.[26] This misperception stems from underestimating the percentage of high-income students (average estimate = 31.19; correct answer = 37).[27] Appendix Table A5 shows that ceteris paribus, females, younger participants, and those with lower adult numeracy test scores are more likely to underestimate the ratio.

As the main concern of our paper is beliefs about the sources rather than the extent of inequality, we examine how beliefs in the *sources* of educational inequality (measured as the variation in college attendance attributed to "fixed" rather than "malleable" factors) vary with beliefs about the *extent* of educational inequality and the same socio-demographic characteristics (Columns 3 and 4 of Appendix Table A5). Since the treatment is designed to shift beliefs about the sources of educational inequality, we conduct this analysis for the control group only. Participants who believe that there is greater educational inequality also have stronger beliefs in the relative importance of fixed factors (0.01 SDs, $p<0.05$). This result suggests that beliefs about the extent of inequality in outcomes are correlated with beliefs about how outcomes are determined.

Beliefs about the sources of educational inequality also vary with participant characteristics. Participants who identify as Democrats, have lower perceived socioeconomic status, score lower on the adult numeracy test, and have a lower score on the locus of control measure (where higher scores represent beliefs that individual outcomes are determined by one's effort, rather than fixed factors) have stronger beliefs in the relative importance of fixed factors. We also document some heterogeneity by race, with Blacks, Hispanics, and "other" racial groups having weaker beliefs and Asians having stronger beliefs in the relative importance of fixed factors compared to Whites. Appendix Table A6 shows that among control group participants, those with stronger beliefs in the relative importance of fixed factors also have stronger support for ex-ante policies, stronger support for ex-post income redistribution (lower earnings ratio between college and non-college graduates), and weaker implicit negative stereotypes about the less-educated.

3.3 Stereotypes about the less-educated

Figure 2 shows the distribution of participants' standardized d-scores (a measure of implicit stereotypes), both overall and by educational attainment. The left panel shows that the average education-IAT d-score across all participants is 0.42, which means that the average participant is 0.42 SDs away from having neutral stereotypes about the less-educated. Using the established thresholds in the psychology literature,[28] 54.21% of participants have negative stereotypes about the less-educated and 23.45% have positive stereotypes about the less-educated. Existing literature that uses IATs to measure other implicit stereotypes such as race and gender have found similar proportions of participants that exhibit negative stereotypes about the stigmatized group, but smaller proportions of participants that show positive stereotypes about the stigmatized group.[29]

The right panel shows that compared to non-college graduates, a larger proportion of college graduates have implicit negative stereotypes about the less-educated: among college graduates, 63.72% have negative stereotypes about non-college graduates, while 46.90% of non-college graduates have negative stereotypes. Appendix Table A7 shows that even conditional on participant characteristics, college graduates tend to have stronger negative stereotypes about the less-educated. This finding is consistent with the psychology literature on "educationism", which hypothesizes that the more-educated hold such stereotypes out of self-interest, as it helps them maintain their social status and justify their lack of support for inequality-reducing policies (Kuppens et al., 2018).

Comparing the implicit and explicit measures (bottom rows of Appendix Table A7), a larger proportion of participants have negative attitudes towards the less-educated when using the implicit measure and a much smaller proportion of participants have "neutral" attitudes. For example, while 53% of participants appear to have neutral explicit attitudes (same feeling thermometer rating for both groups), only 22% of participants have neutral stereotypes about the less-educated (IAT d-score between -0.15 and 0.15). These results suggest the value of using implicit measures in settings where explicit measures are prone to social desirability bias.

To benchmark explicit attitudes against the less-educated with other social group attitudes that are widely studied in the literature, Appendix Table A8 presents statistics on the explicit thermometer ratings for education (College vs Non-college), race (White vs Black) and gender (Male vs Female). Compared to explicit attitudes against the less-educated, fewer participants have an explicit racial or gender attitude against the stigmatized group (23% and 10%, respectively) and more participants have explicit attitudes in favor of the stigmatized group (35% and 50%, respectively, compared to 20%). Furthermore, the distribution of explicit attitudes by education is less leftward-skewed than the distribution of explicit attitudes by race or gender: a larger proportion of respondents report having more positive feelings towards the disadvantaged group for race and gender. Overall, these results suggest that explicit attitudes against the less-educated are more socially acceptable than other widely studied attitudes, as noted by Kuppens et al. (2018).[30]

Appendix Figure A4 shows results from regressions of the education-IAT score on participants' reported strength of attachment to different identities (educational qualifications, gender, race, and nationality). The point estimates suggest that identity attachments based on education, but not other characteristics, are strongly correlated with the IAT measure in the regressions without controls.[31] These results are consistent with the existing literature on in-group favouritism and negative attitudes towards out-group members (Tajfel, 1970) and verifies that our education-IAT most likely captures negative stereotypes about individuals based on educational attainment (rather than related characteristics).

Overall, our finding that most participants, particularly college graduates, have slight to severe negative stereotypes about the less-educated, have important policy implications. Negative stereotypes about the less-educated can increase people's tolerance of social and economic inequalities arising from differences in educational attainment. Beliefs in the relative importance of fixed factors, which we have shown to be positively correlated with educationism, can be used to justify and legitimize these negative stereotypes. Taken together, these stereotypes and beliefs reduce support for inequality-reducing policies and initiatives.

4 Causal effects of beliefs in the sources of educational inequality

We now investigate how beliefs in the sources of educational inequality causally affect policy preferences. Our survey experiment allows us to cleanly identify a causal empirical link between beliefs and policy preferences, which would be difficult to achieve using secondary data. We exogenously vary beliefs about the sources of educational inequality by informing participants in the treatment group about the extent and source of inequality in college attendance. We pre-specified the analysis in a document uploaded to the AEA RCT Registry prior to starting the data collection, which may be accessed at following link: https://www.socialscienceregistry.org/trials/9717.

As stated in our pre-analysis plan, our baseline empirical specification is

$$y_i = a + b_1 \text{Treatment}_i + \mathbf{c}'\mathbf{X}_i + e_i$$

where y_i is the outcome of interest and Treatment_i is an indicator for whether participant i received the information treatment. $\mathbf{X}_i$ is a vector of pre-specified indicators: male; age categories (35-44, 45-54, 55 and over); ethnicity (Asian, Black, Hispanic, and other non-White); having a college degree; working full-time; having above-median household income ($65,000); party affiliation (Republicans and Democrats); and regions of the US (Midwest, South, West); e_i is the error term. We use robust standard errors and weight our regressions so that our main sample is representative of the US population in terms of gender, age, racial-ethnic group, and educational attainment. Appendix Table A9 presents the mean and standard deviation of the main variables of interest among participants in the main survey.

Treatment effects may differ based on pre-treatment characteristics. The two key dimensions of heterogeneity we investigate are educational attainment (college graduates vs non-college graduates), and prior beliefs (underestimating the college attendance ratio vs not underestimating the ratio). To investigate heterogeneous treatment effects, we estimate:

$$y_i = a + b_1 \text{Treatment}_i + b_2(\text{Treatment}_i \times \text{Het}_i) + b_3 \text{Het}_i + \mathbf{c}'\mathbf{X}_i + e_i$$

where Het_i is the dimension of interest. For example, when examining whether treatment effects differ by educational attainment, Het_i equals one for college graduates and zero otherwise. When examining whether treatment effects differ by prior beliefs, Het_i equals one if the participant underestimated the ratio of high-income to low-income college attendees and zero otherwise. The regressions examining heterogeneity by prior beliefs include additional interactions between treatment status and an indicator for underestimating the percentage of high-income college attendees, and an interaction term between treatment status and an indicator for overestimating the percentage of low-income college attendees.[32] As secondary analysis, we also examine treatment effects along other dimensions of heterogeneity, including parental education, income, and party affiliation.

4.1 Shifting beliefs about the sources of educational inequality

Our treatment has a large and statistically significant effect on beliefs about the sources of educational inequality, defined as the variation in college attendance attributed to "fixed" vs. "malleable" factors. Treated participants increase beliefs about the variation attributed to fixed factors by 0.53 SDs ($p<0.01$) compared to non-treated participants (Table 1, column 1). Two weeks later, treated participants' beliefs in the importance of fixed factors is still 0.43 SDs ($p<0.01$) higher relative to the control group (Table 2, column 1).

There are no heterogeneous treatment effects by educational attainment or prior beliefs in the main or follow-up survey (Tables 1 and 2, columns 2-3). We also do not find heterogeneous treatment effects across other dimensions, including parental education, experiences of educational mobility,[33] income, political affiliation, or participants' reported confidence in their pre-treatment beliefs about college attendance (Appendix Tables A16-A20).

4.2 Effects on behavior

The information treatment has significant effects on donation behavior that last for at least two weeks after the main experiment (Tables 1 and 2, column 4). On average, treated participants donate more to the designated higher-education charity (National College Attainment Network) than non-treated participants in both the main and follow-up survey (0.29 SDs and 0.24 SDs, respectively). Among the control group, 1 SD corresponds to \$22, so this effect is equivalent to a \$6.4 increase in donations (out of a maximum of \$100). Appendix Table

A10 shows that in both the main and follow-up survey, the treatment effects occur both on the extensive margin (a higher proportion of treated participants chose to donate at all) and the intensive margin (the amount donated by treated participants is higher, conditional on donating). The proportion of participants who chose to donate at all is 10 percentage points higher in the treatment group for the main survey (vs 75% in the control group) and 5 percentage points in the follow-up survey (vs 69% in the control group).[34]

Column 5 shows that the treatment effects are significantly stronger for college graduates, with donations in the main survey being 0.22 SDs higher than among treated non-college graduates in the main and follow-up survey. The main survey results are robust to controlling for participants' numeracy score, excluding participants who failed the comprehension check, and self-reported altruism (Appendix Table A11). Therefore, these heterogeneous results by college attainment are not driven by between-group differences in numeracy, understanding of the treatment information, or altruism.

We can rule out some other alternative explanations for the observed stronger treatment effects among college graduates. Observing significant treatment effects of similar magnitude for treated college graduates in the main and obfuscated follow-up survey suggest that these results are unlikely to be due to demand effects being stronger for college graduates compared to non-college graduates (for example, awareness of how they were supposed to answer the donation question). It is also unlikely that treated college graduates chose to donate more due to having higher incomes: aside from including income as a control, the negative coefficient for non-treated college graduates suggests that willingness and ability to donate is not higher across the board for college graduates. In the main survey, there is some evidence that treated respondents with at least one parent who attended college increase donations more than treated respondents for whom neither parent attended college (Appendix Table A16).

These results support the external validity of our study, as participants make a decision about real money that is relatively high-stakes (winning $100 is equivalent to a sixteen-fold increase in survey earnings) and involves personal consequences. The average increase of \$6 among the 1,000 treated participants would constitute around 60% of the typical grant given by the NCAN to various cities to help students submit their Federal Student Aid (FAFSA) applications. FAFSA applications are required to access federal student aid,[35] so the donations observed among our participants would make a material difference in tackling inequalities in college attainment. Since donation behaviors are strong predictors of policy preferences (Bonica, 2019), our results indicate that pairing information with the opportunity to donate can be an effective way to raise funds for education-related inequalities.

4.3 Effects on support for ex-ante policies

Columns 7 to 9 of Table 1 present treatment effects on support for ex-ante policies designed to increase access to college for low-income students (expanding the Pell Grant or automatic fee waivers). These regressions control for participants' perceived effectiveness of the policy (measured on a 1-5 scale) to ensure that any detected effects are driven by the treatment rather than differential beliefs about policy effectiveness.[36] The information treatment increases stated support for ex-ante policies by 0.22 SDs ($p<0.01$) in the main survey, though effects are insignificant in the follow-up survey (Table 2, columns 7-9). The follow-up survey results are consistent with previous literature on educational inequalities, which even finds statistically insignificant effects of information provision on support for specific education-related policies immediately after the treatment (Lergetporer et al., 2020). We do not observe heterogeneous effects by educational attainment or prior beliefs.[37]

These results are driven by both ex-ante policies: the information treatment significantly increases support for each policy, when considered separately (0.24 SDs for Pell Grant and 0.32 SDs for the fee waiver, $p<0.01$) (Appendix Tables A12 and A13). Aside from perceptions about policy effectiveness, which we control for in the baseline regressions, policy support may be influenced by perceptions about how the policy personally affects the participant. Our main results are robust to including additional controls for anticipated effects on the participant and anticipated effects by gender and race (Appendix Table A14, columns 1 to 3), indicating that these perceptions do not confound our results.[38]

As a placebo check, we also ask participants about their support for a non-education-related policy (quotas for promoting and hiring women). We use the same format and structure to present the placebo policies. We do not observe similar results for the placebo policy, which suggests that the information treatment specifically increases support for policies that tackle educational inequalities (Appendix Table A14, columns 4 to 6).

These results provide timely insights into the recent debate about improving college affordability. The Biden Administration has already invested in expanding the Pell Grant federal financial aid program and plans to double the size of the maximum grant award by 2029.[39] Pell Grant recipients have also been the focus of President Biden's student loan forgiveness program,[40] with $20,000 in student debt canceled per recipient compared to $10,000 for regular student loan recipients. Given the substantial budget implications of these reforms, public support for these measures is especially important. While the debate surrounding Pell Grant reforms is more complex and nuanced than is possible to present in a survey question,[41] our results demonstrate that information campaigns about the source of educational inequalities can be a powerful tool to increase support for ex-ante policies.

4.4 Effects on support for ex-post redistribution

While we do find some treatment effects for ex-ante policies, support for ex-post income redistribution is much weaker. Considering how much the earnings ratio should change, the coefficient on the treatment indicator is negative but not statistically significant in both the main and follow-up survey (Tables 1 and 2, column 10).[42] All regressions for ex-post policy support include a linear and quadratic term for perceived relative productivity.[43]

These results suggest that our information treatment had narrow effects on policy preferences, with increased support for policies that directly tackle the source of inequality rather than broad redistributive policies. This finding is unsurprising given that our treatment focuses on the role of fixed factors in determining access to college (ex-ante factors) rather than the ex-post outcomes that result from one's educational attainment. It provides further evidence that sources of inequality matter for evaluating the fairness of an outcome.

The absence of treatment effects on preferences for ex-post redistribution despite a significant shift in beliefs about the sources of educational inequality is also consistent with existing findings in the experimental literature. Lergetporer et al. (2020) and Grewenig et al. (2022) find that providing information about the extent of educational inequality does not affect support for inequality-reducing policies that require monetary redistribution or public spending, such as increasing government expenditure on schools with many disadvantaged students or bonuses for teachers who teach in such schools.

5 Mechanisms: Conscious Cognition vs Heuristics

We aim to decompose the total effect of information provision into a direct effect (information affects inputs used in conscious decision-making) and an indirect effect (information changes stereotypes used in heuristic decision-making). This analysis allows us to understand why the treatment worked, not just whether it worked. Following the estimation procedure of Keele et al. (2015), we conduct causal mediation analysis to estimate the relative size of the indirect effect, as a percentage of the total effect.

This approach uses two equations, one with the mediator as the dependent variable (mediation equation) and one with the outcome variable as the dependent variable and the mediator as the independent variable (outcome equation). Intuitively, the analysis investigates how the outcome variable would change if the mediator changed from its expected value in the treatment condition to its expected value in the control condition, holding treatment status and covariates fixed. This approach also requires the assumption that there are no unobserved confounders ("sequential ignorability"), meaning that any variables that may affect the mediator (stereotypes) and the outcomes of interest are controlled for. In this context, an unobserved confounder could be exposure to people of different educational levels in childhood, which may affect stereotypes and outcomes of interest.[44]

Table 3 shows that the size of the indirect effect (average causal mediation effect, or ACME) is negligible (accounting for less than 1% for all outcomes in the main and follow-up survey) and not statistically different from zero, meaning that the observed treatment effects are primarily caused by changes in conscious decision-making from exposure to information. For robustness, we conduct sensitivity analysis to examine how our results change when relaxing the sequential ignorability assumption. Appendix Figure A7 shows how the ACME (expressed in SDs) for all outcome variables varies with the correlation between the residuals of the mediation equation and the residuals of the outcome equation (ρ). Relaxing this assumption results in larger indirect effects, though these are also not statistically different from zero.[45] We conduct similar analysis using the explicit attitude measures as mediators and obtain qualitatively similar results (Appendix Table A15 and Appendix Figure A8).

Our results suggest that appealing to participants' cognitive (rather than heuristic) reasoning can have longer-lasting effects on real behavior (donation decisions) and short-term effects on stated policy preferences. These effects arise because providing information about the true extent and sources of educational inequality strengthens beliefs about the relative role of fixed factors in determining educational outcomes. Overall, our results suggest that cognitively understanding both the extent of inequality in educational attainment and the importance of fixed factors in determining educational attainment motivates people to support policies that specifically tackle the sources of these inequalities.

6 Conclusion

The belief that educational achievement is due to "malleable" factors such as effort is widespread (Sandel, 2020). However, growing evidence shows that educational attainment is heavily influenced by "fixed" factors such as family background (Chetty et al., 2020b). Misperceptions about how education outcomes are determined may undermine support for equality-enhancing policies, preventing the educational system from being a vehicle of social mobility.

We study how beliefs about the sources of educational inequality (whether educational inequalities are primarily due to malleable factors, such as effort, rather than fixed factors such as family background) affect support for policies or initiatives that reduce education-related inequalities. We conduct an online survey experiment on a demographically diverse sample of over 2,000 US residents, where we exogenously vary exposure to information about the extent and sources of educational inequalities. Our study documents the causal effects of correcting misperceptions about the extent *and* sources of educational inequalities, which is an important yet understudied area of the empirical literature. We also explore the underlying mechanisms, using a novel adaptation of a test in the psychology literature to measure implicit stereotypes about the less-educated.

We find that the information treatment (1) strengthens participants' beliefs in the relative role of fixed factors in determining educational outcomes, (2) affects real behavior for at least two weeks after the experiment, causing treated participants to donate more to a charity that tackles educational inequalities, and (3) increases stated support for ex-ante policies (expanding the Pell Grant and removing application fees for low-income students) but not ex-post redistribution (reducing the earnings gap between college and non-college graduates). These results suggest that simple information treatments that help people cognitively understand the extent and sources of educational inequalities encourage them to support more equitable policies and initiatives. Our findings complement those of existing studies, which find that narratives about inequality can be effective in increasing support for inequality-reducing policies (Stantcheva, 2020, 2021).

Our study highlights three promising avenues for future research. First, the role of stereotypes in decision-making warrants further investigation. While in our case, the information treatment did not seek to directly influence stereotypes about the less-educated, treatments that use narratives or other methods of appealing to participants' heuristic reasoning may change stereotypes on top of correcting misperceptions.

Second, stereotypes could be part of the treatment rather than a mediating variable. While we measured participants' implicit stereotypes using the IAT, we did not inform them of their IAT score. There is an emerging literature that examines how informing participants about their implicit stereotypes affects real behaviour (Alesina et al., 2018a).

Third, our study motivates further work on how fairness views influence decision-making. Our study showed that information about sources of inequality (how outcomes are actually determined) matters for decision-making, but fairness views (how people *think* allocations should be determined) may also matter. An emerging literature studies the relationship between preferences for redistribution and fairness views like beliefs in meritocracy (Almås et al., 2020; Andre, 2021). Future studies could construct such a measure of meritocratic beliefs at the individual level, which does not currently exist, and examine heterogeneity along this dimension.

Main figures and tables

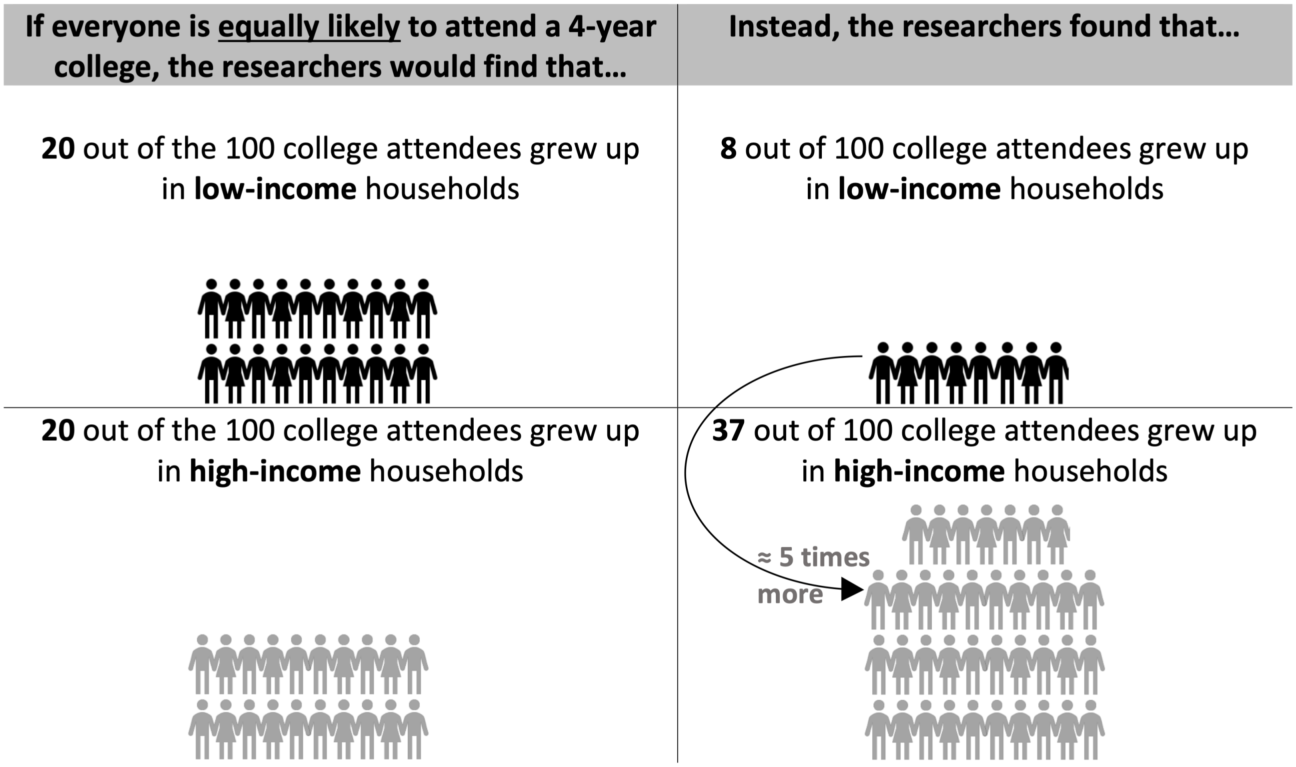

Figure 1. Infographic presented to treatment group participants. The treatment group was also given some text explaining the sources of this inequality, with peer-reviewed evidence. Participants were shown a color version of this infographic, with different colours to represent the information about low-income households and high-income households.

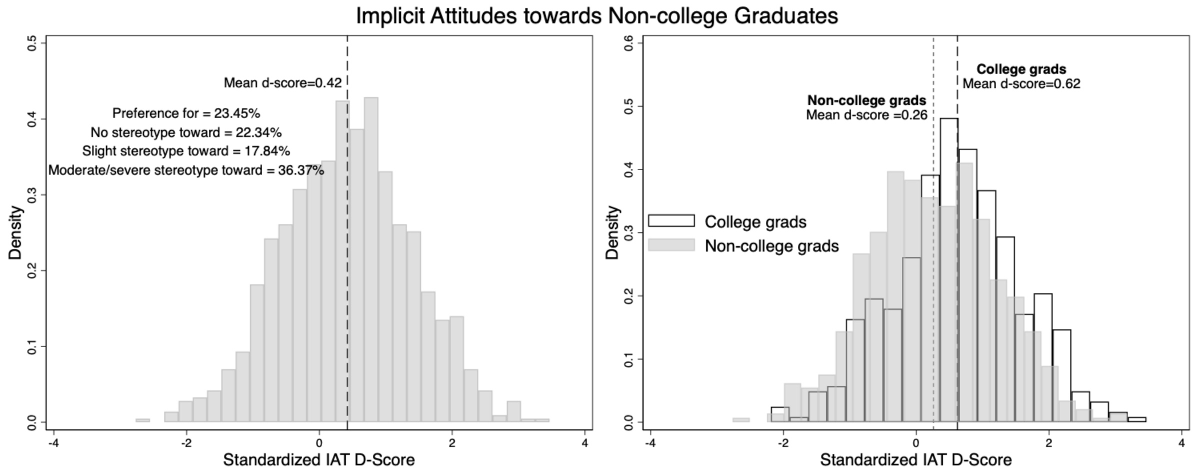

Figure 2. Distribution of standardized education-IAT d-score for all participants (left panel) and by educational attainment (right panel). A d-score below -0.15 shows stereotypes in favor of the non-college graduates, between -0.15 and 0.15 little to no stereotypes, from 0.15 to 0.35 slight negative stereotypes about non-college graduates, and a value higher than 0.35 as moderate to severe negative stereotypes about non-college graduates.

Table 1. Main survey results

	Beliefs in fixed factors			Donations			Ex-ante policy			Ex-post redistribution		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(1) Treated	0.53***	0.55***	0.45***	0.29***	0.22***	0.34***	0.22***	0.20***	0.15*	-0.10*	-0.10	-0.29***
	(0.06)	(0.08)	(0.12)	(0.05)	(0.07)	(0.11)	(0.04)	(0.06)	(0.08)	(0.05)	(0.07)	(0.10)
(2) Treated × College grad		-0.07			0.22**			0.07			0.02	
		(0.11)			(0.09)			(0.08)			(0.10)	
(3) Treated × Underest ratio			0.17			0.03			0.17			0.02
			(0.20)			(0.16)			(0.15)			(0.16)
(4) Underest ratio			-0.32**			0.00			-0.05			-0.00
			(0.16)			(0.11)			(0.13)			(0.12)
(5) College grad	0.07	0.10	0.07	-0.15***	-0.26***	-0.14**	0.01	-0.03	0.01	0.16***	0.15**	0.16***
	(0.07)	(0.08)	(0.07)	(0.06)	(0.07)	(0.06)	(0.04)	(0.06)	(0.04)	(0.05)	(0.07)	(0.05)
(1) + (2)		0.48***			0.44***			0.27***			-0.08	
		(0.07)			(0.06)			(0.06)			(0.07)	
(1) + (3)			0.62***			0.38**			0.32**			-0.26
			(0.22)			(0.19)			(0.16)			(0.19)
R2	0.15	0.15	0.16	0.07	0.08	0.08	0.36	0.36	0.37	0.10	0.10	0.11
Observations	2,008	2,008	2,008	2,008	2,008	2,008	2,008	2,008	2,008	2,008	2,008	2,008

Notes: The table shows results for the main survey. In columns (1)-(3), the outcome is is beliefs in the role of fixed factors (variation in college outcomes attributed to "fixed" rather than "malleable" factors). In columns (4)-(6), the outcome is the amount donated (out of 100 USD) to the National College Attainment Network (a NGO that aims to increase access to tertiary education). In columns (7)-(9), the outcome is support for ex-ante policies (Pell Grant or automatic college application fee waivers). In columns (10)-(12), the outcome is the preferred earnings ratio between a college-graduate and non-college graduate. All outcome variables are standardized by using the mean and standard deviation in the control group. "Underest ratio" equals 1 if the participant underestimated the ratio of the proportion of college students who grew up in high-income households to the proportion of college students who grew up in low-income households. "College grad" equals 1 if the participant has a 4-year college degree. In columns (3), (6), (9), and (12), we also include an interaction term between treatment status and an indicator for whether the participant underestimated the proportion of high-income college attendees, and an interaction term between treatment status and an indicator for whether the participant overestimated the proportion of low-income college attendees. All regressions include the baseline set of controls; additional controls are described in the main text. Regressions are weighted to the our survey sample to a representative US population by age, gender, race, and educational attainment. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2. Follow-up survey results

	Beliefs in fixed factors			Donations			Ex-ante policy			Ex-post redistribution		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(1) Treated	0.43***	0.43***	0.36***	0.24***	0.14**	0.26**	0.06	0.05	0.01	-0.10**	-0.11	-0.07
	(0.04)	(0.06)	(0.08)	(0.05)	(0.07)	(0.10)	(0.04)	(0.05)	(0.08)	(0.05)	(0.07)	(0.09)
(2) Treated × College grad		0.01			0.22**			0.02			0.01	
		(0.09)			(0.11)			(0.08)			(0.09)	
(3) Treated × Underest ratio			0.20			0.16			-0.01			-0.16
			(0.16)			(0.18)			(0.13)			(0.15)
(4) Underest ratio			-0.30***			-0.20*			0.12			0.17
			(0.12)			(0.12)			(0.10)			(0.11)
(5) College grad	0.05	0.05	0.04	-0.01	-0.12	0.00	-0.02	-0.03	-0.02	0.15***	0.15**	0.17***
	(0.05)	(0.07)	(0.05)	(0.06)	(0.07)	(0.06)	(0.05)	(0.06)	(0.05)	(0.06)	(0.07)	(0.06)
(1) + (2)		0.43***			0.36***			0.07			-0.09	
		(0.07)			(0.08)			(0.06)			(0.07)	
(1) + (3)			0.56***			0.42**			-0.01			-0.24
			(0.18)			(0.20)			(0.15)			(0.17)
R2	0.14	0.14	0.15	0.06	0.06	0.06	0.26	0.26	0.27	0.13	0.13	0.13
Observations	1,674	1,674	1,674	1,645	1,645	1,645	1,641	1,641	1,641	1,646	1,646	1,646

Notes: The table shows results for the follow survey. In columns (1)-(3), the outcome is is beliefs in the role of fixed factors (variation in college outcomes attributed to "fixed" rather than "malleable" factors). In columns (4)-(6), the outcome is the amount donated (out of 100 USD) to the National College Attainment Network (a NGO that aims to increase access to tertiary education). In columns (7)-(9), the outcome is support for ex-ante policies (Pell Grant or automatic college application fee waivers). In columns (10)-(12), the outcome is the preferred earnings ratio between a college-graduate and non-college graduate. All outcome variables are standardized by using the mean and standard deviation in the control group. "Underest ratio" equals 1 if the participant underestimated the ratio of the proportion of college students who grew up in high-income households to the proportion of college students who grew up in low-income households. "College grad" equals 1 if the participant has a 4-year college degree. In columns (3), (6), (9), and (12), we also include an interaction term between treatment status and an indicator for whether the participant underestimated the proportion of high-income college attendees, and an interaction term between treatment status and an indicator for whether the participant overestimated the proportion of low-income college attendees. All regressions include the baseline set of controls; additional controls are described in the main text. Regressions are weighted to account for attrition between the main and follow-up survey. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3. Mediation analysis to decompose total effect into indirect and direct effects

Outcome	ACME (Indirect effect)	ADE (Direct effect)	% mediated	P-value (ACME)
	(1)	(2)	(3)	(4)
Main survey				
Donation	0.000	0.291***	0.15%	0.86
Policy support	-0.001	0.281***	0.00%	0.72
Redistribution	0.002	-0.099*	0.00%	0.65
Follow-up survey				
Donation	0.000	0.219***	0.07%	0.96
Policy support	0.000	0.097*	0.26%	0.94
Redistribution	0.002	-0.160**	0.00%	0.74

Notes: This table reports the decomposition of the total treatment effect for each outcome variable (measured in SDs), into the sum of the indirect effect (Average Causal Mediation Effect or ACME; stereotypes) and the direct effect (Average Direct Effect or ADE; conscious cognition). The mediating variable is the education-IAT d-score, measured in standard deviations (positive numbers indicate stronger negative stereotypes about the less-educated). % mediated refers to the size of the ACME, expressed as a percentage of the total effect. Where the ACME and ADE have opposite signs, the % mediated is zero by definition. The p-value corresponds to the null hypothesis that the proportion mediated is zero. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Notes

[1]Across OECD countries, the likelihood of attending a tertiary institution doubles if one parent has at least a high-school diploma, and more than quadruples if one parent has also attained tertiary education (OECD, 2013). This gap cannot be explained by differences in intelligence or academic preparation alone (Damian et al., 2015).

[2]Support for fixed factors related to socio-economic background is generally weak. For example, only 18% of Americans believe that parental education (neither parents having a college degree) should be a major factor in college admissions decisions, and 54% of Americans believe this factor should not be considered at all (Pew Research Center, 2022).

[3]We use "fixed" to describe factors that one has essentially no degree of control over, and "malleable" to describe factors that one has some or a large degree of control over. In the existing literature, this distinction has been conceptualized as an "effort" vs "luck" dichotomy (Alesina and Angeletos, 2005). However, these definitions are not always clear-cut in reality, leading to ambiguity and disagreement about which factors count as "effort"/"malleable" and "luck"/"fixed". For example, it is debatable whether "talent" should be considered as primarily fixed (determined by genetics) or malleable (influenced by deliberate practice) (Alesina and Giuliano, 2011; Benabou, 2000). We therefore allow participants to interpret these terms as they wish, rather than providing them with a prescriptive list of which factors are primarily fixed or malleable. Figure A2 presents evidence of a strong consensus among participants about what factors are "fixed" and "malleable".

[4]One exception is intelligence: it is influenced by fixed factors such as genetics and upbringing, but inequalities that arise from intelligence are perceived as "fair".

[5]These evaluations refer to general preferences to view particular groups favorably in a social context, rather than preferences over characteristics needed for a specific economic context. These measures do not capture labor market preferences to hire educated candidates (who may be more qualified to do a certain job) over less-educated candidates.

[6]See also Dynarski et al. (2021), Bailey and Dynarski (2011), and Chetty et al. (2014).

Some variation in college enrollment rates is likely due to high-income students being more likely than low-income students to apply to college, but statistics only exist for the intensive margin (application rates across different types of college) rather than the extensive margin (whether to apply to college or not) (e.g. Chetty et al. (2020a)).

[7]This empirical literature is complemented by theoretical studies on the relationship between beliefs about the sources of income differences ("effort" or "luck") and support for redistribution and taxation (e.g. Alesina and Angeletos, 2005; Benabou and Tirole, 2006).

[8]As noted in Section 1, differences in college attendance could be due to differences in application rates and differences in other reasons that affect attendance (conditional on being accepted). However, Chetty et al. (2020b) do not have data on application rates across income quintiles that would allow us to present this decomposition to participants.

[9]The correct answers to this question are difficult to find online because participants would need to do additional calculations using the statistics from Chetty et al. (2020b).

[10]See online survey for exact wording.

[11]Participants' beliefs about the distribution of college attendance may depend on their beliefs about differences in college application rates, which we cannot provide information about. However, beliefs in the expected direction (low-income students are less likely to apply than high-income students) are likely to reinforce beliefs about the fairness of the actual distribution, as our information treatment tells participants that fixed factors (financial barriers) affect both the college application and college attendance decision.

[12]An alternative design would be to provide individuals in the control group with no information. We opt for an "active" control group to help disentangle the effects of receiving information (uncertainty reduction, attention, and emotional responses) from correcting misperceptions. See Haaland et al. (2022) for additional benefits of an active control group.

[13]If college attendance was entirely independent of fixed factors, the distribution of attendees may still have more college attendees from the higher income groups due to factors such as assortative matching among parents, which could result in average ability increasing with

income quintile. However, there is no consensus in the literature on what this distribution would be. Even if we were to show participants the distribution of a proxy measure, such as IQ or SAT scores across income groups, the mapping between the proxy measure and a "fair" distribution of college attendance is not definitive. For example, if SAT scores in the top income quintile were twice as high as those in the bottom income quintile, it does not necessarily mean that there should be twice as many college attendees from the top income quintile compared to the bottom income quintile.

[14]Participants are told the range of annual household income for each quintile (below $30,000 for "low-income"; above $135,000 for "high-income") and that each income group is the same size. Appendix A contains further details.

[15]In contrast to our wording, the existing literature focuses on the "effort" versus "luck" dichotomy when examining the reasons for differences in outcomes. Whether or not "effort" is within an individual's control is arguably controversial. For example, some may believe the cost of exerting effort is exogenously determined with limited malleability, such as "laziness" being a trait, while others may believe that effort is completely malleable.

[16]Some variation across participants is expected according to their beliefs about free will; however there seems to be strong consensus that at least some factors are malleable.

[17]We chose the NCAN because it focuses on equality of opportunity in post-secondary education, which is directly related to the treatment group information, and has a national reach. Other education-related charities either focus on pre-college outcomes (e.g. Equal Opportunity Schools) and/or do not have a national reach (e.g. CollegePossible). The NCAN is also larger (measured by annual revenues and expenses) and has greater public awareness, having been cited by prominent news outlets such as the New York Times and ABC News. The NCAN is also reputable, as it has no known donation fraud scandals that could affect perceptions of its effectiveness.

[18]Other studies have also used donations as a measure of behavior (e.g. Alesina et al., 2022; Haaland and Roth, 2020; Andre et al., 2021; Grewenig et al., 2022).

[19]The typical grant that the NCAN gives to various cities to help students submit Federal Student Aid (FAFSA) applications is $10,000; which would be equivalent to 100 participants donating their entire lottery winnings.

[20]We use the latest data release at the time of our survey (2014, taken from https://www.bls.gov/news.release/archives/wkyeng_01212015.pdf). The earnings ratio is calculated from the usual weekly earnings of full-time wage and salary workers aged 25 and over, across all industries and occupations.

[21]For the 2023 Fiscal Year (as of 30 November 2022), 51% of total revenue came from individual income taxes while only 1% came from estate and gift taxes (US Treasury, 2023).

[22]For example, Cruces et al. (2013) and Kuziemko et al. (2015).

[23]This question format is widely used in psychology to measure general attitudes towards a group of people (Kuppens et al., 2018). Feeling thermometers are also used in political science to measure preferences for parties or candidates (Kelley and Mirer, 1974) and partisan affiliation (Weisberg, 1980).

[24]The weighted sample is representative of the US population based on age (25-35, 25-45, 45-55, 55 and above), gender (male vs. non-male), race (White vs. non-White), and highest educational attainment (4-year college degree vs. non-college graduate). We use the 2019 American Community Survey to calculate these age-gender-race-education cells.

[25]Ratios between extreme ends of the distribution, such as the 90/10 ratio, are commonly used by statistical agencies to measure inequality.

[26]We do not find evidence that beliefs of college graduates and non-college graduates differ; for example, the estimated percentage of low-income students is 7.36 for college graduates vs 8.90 for non-college graduates, and the estimated percentage of high-income students is 30.96 for college graduates vs 31.30 for non-college graduates. Appendix Figure A5 shows the full distribution of beliefs and the median belief for each quintile; the median belief increases monotonically and is similar to the average value.

[27]Average misperceptions of a few percentage points for each quintile is in line with ex-

isting experimental literature that elicits beliefs using a constrained question (e.g. Alesina et al., 2018b). This underestimation of high-income college attendees is unlikely to be due to the question format (presenting the quintiles from smallest to largest), as the majority of participants' (65%) beliefs satisfy monotonicity (increasing for each quintile). These misperceptions are also unlikely to be due to lack of comprehension or random guessing, as we design our belief elicitation question to minimize the chances of participants reporting nonsensical answers (see Appendix A). Furthermore, these misperceptions are unlikely to be mainly driven by beliefs in differential college application rates across quintiles, because beliefs that application rates increase with income would result in underestimating the percentage of low-income students and overestimating the percentage of high-income students.

[28]A d-score below -0.15 shows positive stereotypes about stigmatized group, between -0.15 and 0.15 little to no stereotypes, and above 0.15 negative stereotypes about the stigmatized group (Greenwald et al., 2003).

[29]For example, in Glover et al. (2017)'s study on stereotypes about racial minorities, 86% of participants show negative stereotypes about minorities, 9% of participants show little to no stereotypes, and 4% show positive stereotypes. Carlana (2019)'s study on Italian teacher's gender-subject stereotypes finds 16% of teachers associate math with girls, 23% present little to no clear association, and 61% show male math association.

[30]The first row of Appendix Table A8 also shows that implicit negative stereotypes about the less-educated (the IAT d-score) is positively and significantly correlated with explicit attitudes along this dimension (column 1), but also with racial attitudes (column 2). The latter result could be due to the strong overlap between education and race.

[31]When controls are included, the correlation between attachment to different identities and the IAT disappears.

[32]Specifically, we estimate $y_i = a + b'_1(\text{Treatment}_i \times \text{OverRatio}_i) + b'_2(\text{Treatment}_i \times \text{OverQ1}_i) + b'_3(\text{Treatment}_i \times \text{UnderQ5}_i) + b'_4\text{Treatment}_i + b'_5\text{OverRatio}_i + b'_6\text{OverQ1}_i + b'_7\text{UnderQ5}_i + \mathbf{c}'\mathbf{X}_i + e_i$, where OverRatio_i equals 1 if respondent i overestimates the ratio, OverQ1_i equals 1 if

respondent i overestimates the percentage of college attendees who grew up in a low-income household, and Under5_i equals 1 if respondent i underestimates the percentage of college attendees who grew up in a high-income household. Overestimating the ratio can be driven by various types of misperceptions (e.g. overestimating and underestimating the percentage of low- and high-income college students respectively, or underestimating the percentage of low- and high-income college students both result in overestimating the ratio), so we interact indicators for overestimating and underestimating the percentage of college attendees who grew up in low- and high-income households with the treatment variable.

[33]We construct a measure of experiences of educational mobility based on whether the respondent attended college but neither parent did.

[34]The statistical significance and magnitude of the donation results for the main survey are in line with those of Grewenig et al. (2022), who study inequalities in secondary school attendance in Germany (but do not have follow-up results for donations).

[35]According to the NCAN (`https://www.ncan.org/`), in 2021, around 47% of graduating seniors did not complete their FAFSA, leaving \$3.75 billion in Pell Grants unclaimed. Top reasons for not completing a FAFSA include students not being aware they are eligible, and believing the process is too complicated. The NCAN provides information sessions and funds school districts to provide one-on-one advising for low-income students.

[36]To control for the perceived efficacy of these policies, we ask how effective these policies would be in promoting the specified goal.

[37]Appendix Table A19 shows heterogeneous effects by political affiliation. While there is a strong treatment effect among Independents and Republicans, there is no treatment effect among Democrat participants. This lack of treatment effect among Democrats could be because the mean and median support for these ex-ante policies is already at the maximum possible level (10 on a 0-10 scale) in the control group.

[38]To control for the influence of perceived differential effects across demographic groups, we ask participants how they think the following groups will be affected by the policy (ranging

from very negatively affected to very positive affected): White Americans, Black Americans, women, men, you and/or people you care about.

[39]See: https://www.bestcolleges.com/news/analysis/2022/04/05/biden-budget-double-pell-grant-college-financial-aid/ [Accessed: 10 October 2022].

[40]See: https://apnews.com/article/biden-education-higher-pell-grant-fce8a300f7d7400283891dc223cbc378 [Accessed: 10 October 2022].

[41]For example, the general equilibrium effects on tuition fees and student debt are difficult to predict but are a prominent part of the debate.

[42]We also find heterogeneous effects by political affiliation in the follow-up survey, with treated Democrats having a preferred earnings ratio that is 0.17 SDs ($p<0.05$) lower than non-treated participants (Appendix Table A19).

[43]Before the treatment, we measure participants' perceptions about the relative productivity differences by asking them to consider a college graduate and non-college graduate who produced $100 of output per hour in total and then asking how much output (in $) they think the college graduate produced out of that $100.

[44]There is a vast social science literature on the causal effects of exposure to outgroups on stereotypes and prejudices (e.g. Berger et al., 2016; Hsieh et al., 2022).

[45]The largest ACME for the donation outcomes, corresponding to a correlation of 0.9, accounts for 19.9% of the total effect in the main survey and 21.4% in the follow-up survey.

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Online Appendix

Sources of educational inequality and redistributive behaviour:

Experimental evidence

A Survey details

A.1 Pre-treatment beliefs elicitation

Before the treatment, we ask participants to report how many, out of 100 random individuals who attended a 4-year college, grew up in a "low", "below-middle", "middle-income", "above-middle", and "high" income household. Each of these income groups corresponds to a quintile of the parental income distribution, but we chose to use these groups as they are easier to understand than the technical term "quintile". Participants are told the range of annual household income for each quintile (e.g. below $30,000 for "low-income" - the bottom quintile - and above $135,500 for "high-income" - the top quintile) and that each income group (quintile) is the same size.

We chose this specific question because there are population-level correct answers from Chetty et al. (2020b), who use a de-identified administrative dataset to estimate the parental income distribution by college type, by linking college-related outcomes for children born between 1980 and 1982 with their parents' household incomes. Compared to alternative questions to elicit beliefs, such as how many students (out of 100) in each quintile attend college, the correct answers most clearly show the difference in educational opportunities between the top and bottom income quintiles, making a treatment effect more likely.[46]

To help participants understand the question, we give them an example of what the distribution would look like if everyone was equally likely to attend college (20 people in each income group).[47] We chose this example as it was simple to explain in layman terms, rather than report a distribution of what college attendance "should" look like in the absence of fixed factors.[48] Furthermore, to ensure that participants' answers were logically consistent, we constrain the answers across all quintiles to sum to 100. We inform participants that there is at least one college attendee in each income quintile so the minimum and maximum values per quintile would be 1 and 96 respectively. We also ask participants how confident they were in their answers to quintiles 1 and quintiles 5 (on a 1-5 scale). Figure S1 of this appendix shows there is no evidence that this example provides a focal point for participants or signals that this example is the correct answer, as the distribution of confidence for participants who answer 20 for either quintile 1 or 5 does not look significantly different from the distributions for other answers. Finally, we set a timer so that participants have to spend at least one minute on this question before progressing.

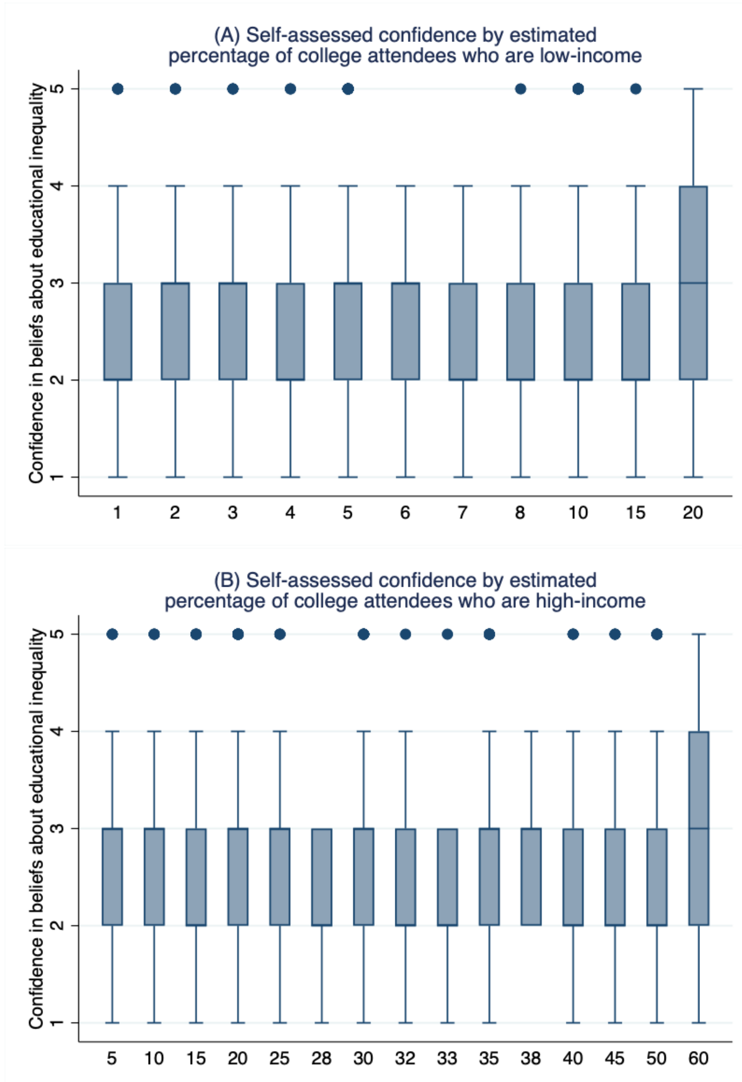


Figure S1. Box and whisker plots showing the distribution of participants' self-reported confidence (1-5 scale) in their answer to the percentage of college students who grew up in a low-income (panel A) or high-income (panel B) household, for each number answered. The middle line is the median, the ends of the box are the first and third quartiles, dots indicate outliers that are more than 1.5 SDs away from the median.

A.2 Obfuscation in the follow-up survey

To further disguise the link between the main and follow-up study, we undertake the following measures: (1) using a vague study title and study description in the recruitment notice, to avoid reminding participants of the main study's content; (2) launching the study from a separate account, enabling the names of the lead researcher of the study to be different between the main and follow-up study; (3) using different consent forms (from different universities); (4) changing the survey's layout and appearance, such as the illustrative images used and the font; (5) asking participants questions about other topics first, including typical demographic questions, and leaving the main outcome questions to the end.

In addition to these steps, we obfuscate the questions in the follow-up survey as follows:

- Ex-ante policy support (barriers to college attendance): Participants are given a different policy than the one they were given in the main survey (i.e. participants who were given the Pell Grant question in the main survey were given the fee waiver question in the follow-up survey, and vice versa).

- Ex-post redistribution (post-college economic inequalities): Participants were asked to indicate, on a continuous unnumbered slider, their desired earnings ratio between the college and non-college graduate, where dragging towards the left indicates a decrease and dragging towards the right indicates an increase. We present the earnings ratio in a different manner (ratio of annual earnings, expressed in USD, rather than 100:173).

- Behavioural measure: Instead of a 0-100 slider, participants were asked to input a number (from 0-100) indicating how much of their lottery winnings they wanted to donate to the NCAN.

- Implicit stereotypes about the less-educated: Participants still do an IAT but with a different color and font scheme and different words in the categorization tasks.

- Explicit attitudes towards the less-educated: Instead of using a feeling thermometer question, we use a trust-points allocation game based on Enke et al. (2022), who use it to measure the level of altruism and trust that participants exhibit towards strangers.[49] Participants are asked to indicate how they would split 100 "trust points" between a randomly selected college graduate who lives in the US and a randomly selected non-college graduate who lives in the US.[50]

B Education Implicit Association Test

The Implicit Association Test (IAT) is a rapid categorization computer-based task that is used to measure stereotypes that individuals may not be explicitly aware of, or are unwilling to express. The underlying principle behind the IAT is that easier mental tasks can be completed more quickly and with fewer errors (Greenwald et al., 1998, 2003).

At the start of the IAT, participants are given two lists of words: words with positive/negative valence ("pleasant" or "unpleasant") and education-related words ("college grad" or "non-college grad"). Since the IAT involves rapid categorization, we chose to use words rather than pictures or resumes because words can quickly and unambiguously convey the intended meaning (an individual's level of education) within the brief time the participant views it. For the positive/negative list, we use the same words as those in the standard IAT (for example, "happy" as a positive word and "gloom" as a negative word). For the education-related list, we create our own list of words, each consisting of a name that either does or does not include an education-related abbreviation (BSc, J.D., MBA, MSc, M.D., PhD).[51] To ensure that our measure of stereotypes based on education are not contaminated by racial or gender-related stereotypes, we use names that have been verified in the literature as predominantly associated with White males (Bertrand and Mullainathan, 2004).[52] We inform participants about the education-related abbreviations and how to use them to categorize the words. For example, participants are told that "Greg, BSc" is an example of a word in the "college grad" category and "Neil" is an example of a word in the "non-college grad" category. We also give participants sufficient practice in categorizing these words correctly.

Table IAT1 of this appendix shows the full list of words used for the IAT tasks in the main and follow-up survey. For obfuscation purposes, in the follow-up survey we use different names (variations of the names from the main survey or similar-sounding male names) and different words for the "pleasant" and "unpleasant" criteria.

The IAT consists of seven categorization tasks, detailed in Table IAT2 of this appendix.

Tasks 1, 2, and 5 are practice tasks, while tasks 3, 4, 6, and 7 are used to calculate the measure of implicit stereotypes about the less-educated. In Tasks 3 and 6, participants are given 20 randomly selected words (10 names with/without degrees and 10 positive or negative words) to categorize, and in Tasks 4 and 7, participants are given 40 randomly selected words (20 names with/without degrees and 20 positive or negative words). Tasks 3 and 4 present the words with their "stereotypical" association, with the college-graduate/pleasant criteria grouped together on one side and the non-college graduate/unpleasant criteria grouped together on the other side (Appendix Figure IAT1, panel A). Tasks 6 and 7 present the words with their "non-stereotypical" association, with the college-graduate/unpleasant on one side and non-college graduate/pleasant on the other side (Appendix Figure IAT1, panel B).

Since the order in which participants complete the stereotypical and non-stereotypical tasks and the location of the "pleasant" criterion (left or right of the screen) may matter, we randomly assigned half of our participants to receive the tasks in the order shown in Table IAT2 of this appendix, while the other half received the tasks in the order 1, 5, 6, 7, 2, 3, 4. We also randomized (with equal probability) whether the participant would complete all the tasks with the college-educated prime (and associated "pleasant"/"unpleasant" criteria) on the left or the right of the screen. Therefore, each participant may receive one of four different versions of the education-IAT for the main and the follow-up survey (not necessarily the same version in both surveys): 1) "stereotypical" first, "college" on right (with "pleasant" criteria); 2) "non-stereotypical" first, "college" on right (with "unpleasant" criteria); 3) "stereotypical" first, "college" on left (with "pleasant" criteria); 4) "non-stereotypical" first, "college" on left (with "unpleasant" criteria). Appendix Table IAT3 shows that versions 2 and 4 are associated with lower education-IAT scores in the main survey compared to versions 1 and 3, but there are no significant correlations between version and education-IAT score in the follow-up survey.

Our measure of implicit stereotypes about the less-educated is the d-score, which is the normalized difference in average response times (in milliseconds) between categorization tasks that use "stereotypical" pairs of concepts (Tasks 3 and 4) and tasks that use "non-stereotypical" pairs of concepts (Tasks 6 and 7). To calculate the d-score, we use R's "iatgen" command, following the procedure specified in Greenwald et al. (2003). A positive d-score indicates implicit negative stereotypes about the less-educated while negative d-scores indicate implicit positive stereotypes about the less-educated. For ease of interpretation, we divide raw IAT scores by the pooled standard deviation in our sample (0.48). Following standard practice, participants who were too slow or made too many mistakes in any of the key tasks do not have a d-score, because these types of responses could indicate that participants either did not understand the task or were trying to manipulate their test results (Nosek et al., 2002).

Table IAT1. Words used in the education-IAT

	Criteria	Words
Main survey	Pleasant	gentle, enjoy, heaven, cheer, happy, love, friend
	Unpleasant	poison, evil, gloom, damage, vomit, ugly, hurt
	Names	Geoffrey, Brendan, Matthew, Neil, Greg, Brad, Todd
	Degrees	BSc, J.D., MBA, MSc, M.D., PhD
Follow-up survey	Pleasant	vacation, happy, enjoyment, fun, hug, delight, joy
	Unpleasant	unhappy, nightmare, stress, starvation, disaster, virus, disease
	Names	Gregory, Matthias, Brandon, Brad, Geoff, Tom, Nathan
	Degrees	BSc, J.D., MBA, MSc, M.D., PhD

Table IAT2. Categorization tasks used in the education-IAT

Task	Left-side criteria	Right-side criteria	# words
1	Non-college graduate	College graduate	20
2	Unpleasant	Pleasant	20
3	Non-college graduate OR Unpleasant	College graduate OR Pleasant	20
4	Non-college graduate OR Unpleasant	College graduate OR Pleasant	40
5	Pleasant	Unpleasant	20
6	Non-college graduate OR Pleasant	College graduate OR Unpleasant	20
7	Non-college graduate OR Pleasant	College graduate OR Unpleasant	40

Table IAT3. Correlation between education IAT score and order of IAT blocks

	Main survey			Follow-up		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Control	Treatment	All	Control	Treatment
Order 2	-0.60***	-0.48***	-0.70***	0.06	0.08	0.04
	(0.06)	(0.08)	(0.09)	(0.07)	(0.09)	(0.10)
Order 3	0.08	0.09	0.09	-0.03	-0.06	-0.00
	(0.06)	(0.08)	(0.09)	(0.07)	(0.09)	(0.10)
Order 4	-0.56***	-0.52***	-0.60***	0.09	0.02	0.18*
	(0.06)	(0.08)	(0.09)	(0.07)	(0.09)	(0.10)
R2	0.16	0.16	0.17	0.06	0.08	0.07
Observations	2,008	998	1,010	1,702	846	856

Notes: This table reports OLS estimates of the correlation between the IAT score and the order of IAT blocks. "Omitted order" = "stereotypical" first, "college" on right (with pleasant criteria). "Order 2" = "non-stereotypical" first, "college" on right (with "unpleasant" criteria). "Order 3" = "stereotypical" first, "college" on left (with "pleasant" criteria). "Order 4" = "Non-stereotypical" first, "college" on left (with "unpleasant" criteria). In columns (1)-(3), we weight the regressions by probability weights to match our survey sample to a representative US population by age, gender, race, and educational attainment. In columns (4)-(6), we weight the regressions to account for attrition between the main and follow-up survey. All regressions include the baseline set of controls. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

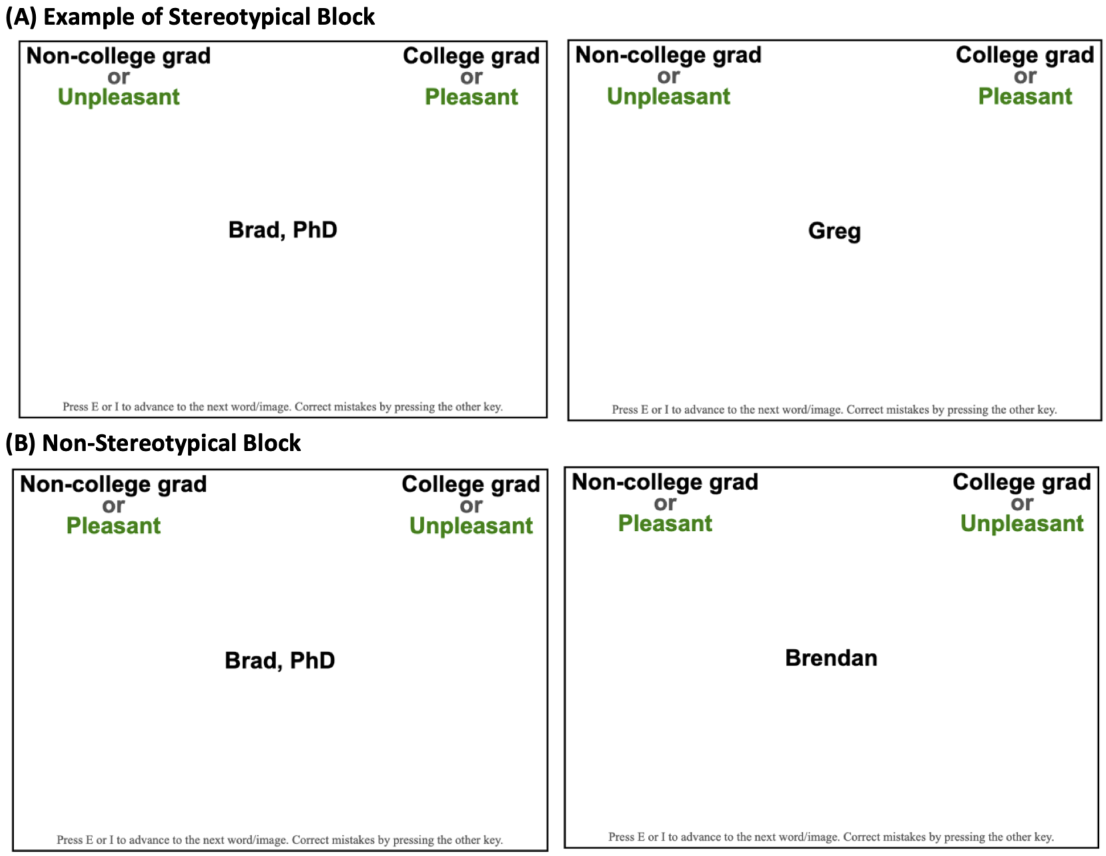


Figure IAT1. Screenshot of the IAT tasks used to calculate the d-score. Panel A provides examples of the "stereotypical" task, where the college-graduate/pleasant criteria are grouped together on one side and the non-college graduate/unpleasant criteria are grouped together on the other side. Panel B provides examples of the "non-stereotypical" task where the college-graduate/unpleasant criteria are grouped together on one side and the non-college graduate/pleasant criteria are grouped together on the other side.

C Appendix tables

Table A1. Summary statistics

	Unweighted	Weighted	ACS	GSS
	(1)	(2)	(3)	(4)
Age 25-34	0.33	0.19	0.20	0.22
Age 35-44	0.23	0.19	0.19	0.21
Age 45-54	0.20	0.22	0.20	0.20
Age 55+	0.23	0.43	0.43	0.39
Male	0.44	0.48	0.48	0.45
White	0.78	0.74	0.74	0.73
College	0.44	0.33	0.33	0.33
Observations	2,008	2,008	2,327,346	2,144

Notes: This table displays summary statistics from our main sample (columns 1 and 2), (weighted) averages based on the 2019 American Community Survey (ACS) sample (column 3), and the 2018 General Social Survey (GSS) (column 4). To weight our sample, we create representative sample weights to match the 2019 ACS sample.

Table A2. Balance of covariates in main experiment

	Control	Treatment	Difference	P-val
	(1)	(2)	(3)	(4)
Age	42.90	42.51	0.38	0.53
Male	0.43	0.45	-0.02	0.35
White	0.77	0.78	-0.01	0.46
Democrat	0.45	0.48	-0.03	0.24
Republican	0.18	0.16	0.02	0.25
College	0.43	0.44	-0.01	0.66
Income above median	0.46	0.47	-0.01	0.77
College(father)	0.32	0.35	-0.03	0.19
College(mother)	0.29	0.29	0.00	0.81
Children	0.48	0.44	0.03	0.13
Ratio under 5	0.50	0.54	-0.04	0.08
Beliefs about Q1	8.06	8.42	-0.36	0.20
Beliefs about Q5	30.90	30.14	0.76	0.18
Observations	998	1,010		

Notes: This table displays covariate means for the treatment and control group in the main experiment. "Income above median" equals 1 if total (pre-tax) household income last year was above 65,000 USD. "College (father)" and "College (mother)" take the value 1 if the father or mother of the participant attended college. "Ratio under 5" equals 1 for participants who underestimated the ratio of the proportion of college students who grew up in high-income households to the proportion of college students who grew up in low-income households. "Beliefs about Q1" and "Beliefs about Q5" respectively refer to beliefs about the proportion of college students who grew up in low-income and high-income households. The p-value of a joint F-test of a regression of the treatment indicator on all of the covariates is 0.75.

Table A3. Balance of covariates in follow-up survey

	Control	Treatment	Difference	P-val
	(1)	(2)	(3)	(4)
Age	44.23	43.54	0.68	0.31
Male	0.42	0.45	-0.03	0.19
White	0.77	0.79	-0.02	0.34
Democrat	0.46	0.50	-0.03	0.15
Republican	0.18	0.15	0.03	0.06
College	0.47	0.48	-0.01	0.61
Income above median	0.45	0.48	-0.02	0.34
College(father)	0.34	0.37	-0.03	0.23
College(mother)	0.30	0.31	-0.01	0.73
Children	0.49	0.45	0.04	0.13
Ratio under 5	0.49	0.53	-0.04	0.07
Beliefs about Q1	8.00	8.39	-0.39	0.20
Beliefs about Q5	31.36	30.20	1.16	0.06
Observations	832	842		

Notes: This table displays covariate means for the treatment and control group in the follow-up survey. "Income above median" equals 1 if total (pre-tax) household income last year was above 65,000 USD. "College (father)" and "College (mother)" take the value 1 if the father or mother of the participant attended college. "Ratio under 5" equals 1 for participants who underestimated the ratio of the proportion of college students who grew up in high-income households to the proportion of college students who grew up in low-income households. "Beliefs about Q1" and "Beliefs about Q5" respectively refer to beliefs about the proportion of college students who grew up in low-income and high-income households. The p-value of a joint F-test of a regression of the treatment indicator on all of the covariates is 0.36.

Table A4. Correlates of treatment status and follow-up status

	Treatment (main)	Treatment (follow)	Recontacted
	(1)	(2)	(3)
Female	-0.06	-0.09	-0.02
	(0.06)	(0.06)	(0.07)
35-44 y/o	0.02	-0.00	0.20**
	(0.08)	(0.08)	(0.09)
45-54 y/o	0.00	-0.03	0.43***
	(0.08)	(0.08)	(0.11)
≥55 y/o	-0.03	-0.04	0.55***
	(0.08)	(0.08)	(0.11)
Black	-0.11	-0.15	-0.08
	(0.11)	(0.12)	(0.13)
Asian	0.01	-0.03	-0.04
	(0.13)	(0.15)	(0.16)
Hispanic	-0.07	-0.10	-0.00
	(0.12)	(0.13)	(0.14)
Other race	-0.01	-0.03	0.17
	(0.16)	(0.18)	(0.20)
College grad	0.01	0.01	0.34***
	(0.06)	(0.07)	(0.08)
College (mother)	-0.04	-0.03	0.10
	(0.07)	(0.08)	(0.09)
College (father)	0.10	0.08	0.11
	(0.07)	(0.08)	(0.09)
Democrat	0.05	0.05	0.11
	(0.06)	(0.07)	(0.08)
Republican	-0.05	-0.12	-0.02
	(0.09)	(0.09)	(0.10)
Inc. abv. median	-0.06	-0.01	-0.12
	(0.06)	(0.07)	(0.08)
Fulltime	-0.00	0.03	-0.08
	(0.06)	(0.07)	(0.08)
Midwest	-0.13	-0.05	0.01
	(0.09)	(0.10)	(0.12)
South	-0.11	-0.07	-0.09
	(0.08)	(0.09)	(0.10)
West	-0.16*	-0.08	-0.13
	(0.09)	(0.10)	(0.12)
F-stat	11.5	12.21	90.27
P-val	0.97	0.95	0.00
Psuedo R2	0.00	0.01	0.06
Observations	2,008	1,674	2,008

Notes: In column (1), the sample includes all participants and the dependent variable is a binary indicator for being in the treatment group. In column (2), the sample includes participants who responded to the follow-up survey and the dependent variable is a binary indicator for being in the treatment group. In column (3), the sample includes all participants and the dependent variable is a binary indicator for responding to the follow-up survey. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5. Correlates of beliefs in educational inequality and in the role of fixed factors

	(Misperceptions in) Educ Inequality		Importance of fixed factors	
	(1)	(2)	(3)	(4)
Ratio (Q5/Q1)			0.01**	0.01**
			(0.00)	(0.00)
Female	0.30**	0.17	0.04	0.11
	(0.12)	(0.13)	(0.08)	(0.09)
35-44 y/o	-0.28**	-0.33**	0.15*	0.17*
	(0.13)	(0.13)	(0.08)	(0.09)
45-54 y/o	-0.30**	-0.39**	-0.16*	-0.09
	(0.15)	(0.16)	(0.10)	(0.10)
$\geq$ 55 y/o	-0.30**	-0.42**	-0.23**	-0.13
	(0.15)	(0.17)	(0.10)	(0.10)
Black	0.36	0.23	-0.50***	-0.31**
	(0.23)	(0.23)	(0.15)	(0.15)
Asian	0.41	0.44	0.53**	0.48**
	(0.33)	(0.33)	(0.23)	(0.23)
Hispanic	0.32	0.28	-0.39**	-0.26*
	(0.26)	(0.28)	(0.17)	(0.16)
Other race	-0.15	-0.16	-0.54**	-0.58**
	(0.36)	(0.36)	(0.26)	(0.27)
College grad	-0.17	-0.07	0.03	-0.02
	(0.12)	(0.12)	(0.08)	(0.08)
Fulltime	-0.17	-0.20	-0.02	0.08
	(0.13)	(0.13)	(0.08)	(0.08)
Inc. abv. median	-0.15	-0.15	-0.10	-0.01
	(0.13)	(0.15)	(0.08)	(0.09)
Democrat	-0.04	-0.04	0.25***	0.22***
	(0.13)	(0.13)	(0.09)	(0.08)
Republican	-0.08	-0.15	-0.26**	-0.15
	(0.18)	(0.18)	(0.12)	(0.12)
Has children		0.11		-0.04
		(0.13)		(0.08)
Family low SES		-0.12		-0.05
		(0.13)		(0.09)
Current low SES		-0.07		0.19**
		(0.15)		(0.10)
Numeracy		-0.27***		0.19***
		(0.07)		(0.04)
Locus of control		0.03		0.13**
		(0.06)		(0.05)
Altruism		0.01		0.04
		(0.06)		(0.04)
College (mother)	0.17	0.19	-0.24***	-0.26***
	(0.15)	(0.15)	(0.09)	(0.10)
College (father)	-0.07	-0.07	0.30***	0.28***
	(0.15)	(0.15)	(0.10)	(0.10)
R2 (pseudo)	0.02	0.03		
R2			0.15	0.20
Observations	2,008	2,008	998	998

Notes: In columns (1) and (2), the dependent variable takes on value one if the participant underestimated the ratio of the proportion of high-income college attendees to the proportion of low-income college attendees. In columns (3) and (4), the dependent variable is beliefs in the role of fixed factors (variation in college outcomes attributed to "fixed" rather than "malleable" factors), standardized by using the mean and standard deviation in the control group. "Ratio (Q5/Q1)" is the ratio of the participant's belief about the percentage of college attendees who grew up in a high-income to low-income household. In all columns, we weight the regressions by probability weights to match our survey sample to a representative US population by age, gender, race, and educational attainment. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6. Association between beliefs in the role of fixed factors and outcomes of interest

	Donations	Ex-ante	Ex-post	Stereotypes
	(1)	(2)	(3)	(4)
Fixed factors important (stndzd)	-0.02	0.12***	-0.14***	-0.09***
	(0.03)	(0.03)	(0.03)	(0.03)
R2	0.06	0.22	0.07	0.09
Observations	998	998	998	998

Notes: This table shows OLS regressions among control group participants. We regress the outcome indicated in each column on standardized beliefs in the role of fixed factors (variation in college outcomes attributed to "fixed" rather than "malleable" factors). In column (1), the outcome is donation to the National College Attainment Network. In column (2), the outcome is support for ex-ante policies. In column (3), the outcome is support for ex-post redistribution. In column (4), the outcome is implicit stereotypes against the less-educated, measured using the IAT (higher positive numbers mean stronger negative stereotypes). All outcomes have been standardized. All regressions include the baseline set of controls. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7. Correlates of implicit stereotypes and explicit attitudes towards the less-educated

	D-score	D-score>0	Feelings diff
	(1)	(2)	(3)
Female	0.05	-0.03	1.13
	(0.09)	(0.19)	(1.53)
35-44 y/o	-0.04	0.11	-2.13
	(0.08)	(0.20)	(1.34)
45-54 y/o	-0.05	0.07	-1.30
	(0.10)	(0.24)	(1.24)
$\geq$ 55 y/o	0.12	0.01	-0.14
	(0.11)	(0.22)	(1.58)
Black	0.32^{**}	0.43	3.33
	(0.15)	(0.36)	(2.55)
Asian	0.15	0.06	5.00^{***}
	(0.26)	(0.54)	(1.63)
Hispanic	-0.01	-0.24	-0.52
	(0.18)	(0.37)	(2.55)
Other race	-0.53^{**}	-0.52	2.41
	(0.21)	(0.54)	(2.03)
College grad	0.35^{***}	0.95^{***}	4.46^{***}
	(0.08)	(0.18)	(1.13)
Fulltime	-0.22^{**}	-0.68^{***}	-0.84
	(0.09)	(0.20)	(1.04)
Inc. abv. median	0.15	0.30	0.36
	(0.09)	(0.21)	(1.16)
Democrat	0.07	-0.08	2.33
	(0.09)	(0.20)	(1.67)
Republican	0.14	-0.21	-0.21
	(0.13)	(0.27)	(1.80)
College (mother)	-0.11	-0.02	-0.83
	(0.09)	(0.21)	(1.18)
College (father)	0.11	0.10	2.49^{*}
	(0.10)	(0.21)	(1.27)
% w/ score	(-0.15-0.15) 22.34		(= 0) 53.21
% w/ score	(> 0.15) 54.21		(> 0) 27.15
% w/ score	(< -0.15) 23.45		(< 0) 19.64
R2 (pseudo)		0.05	
R2	0.08		0.08
Observations	998	998	998

Notes: Positive coefficients indicate stronger negative stereotypes against the less-educated. In columns (1) and (2), "D-score" refers to the IAT d-score (the difference in average response times between the stereotypical and nonstereotypical blocks in our education-IAT measure, divided by the standard deviation of response times). In column (3), "Feelings diff" refers to the difference in self-reported thermometer ratings towards college graduates and ratings towards non-college graduates. In all columns, we weight the regressions by probability weights to match our survey sample to a representative US population by age, gender, race, and educational attainment. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8. Correlates of explicit stereotypes (feeling thermometer) and IAT d-score

	College v Non-College	White v Black	Male v Female
	(1)	(2)	(3)
IAT d-score	2.18***	2.31**	-1.05
	(0.72)	(1.05)	(0.75)
% w/ score =0	53.21	41.88	40.08
% w/ score >0	27.15	22.65	10.42
% w/ score <0	19.64	35.47	49.50
Mean	.41	-4.65	-11.19
10th pctile	-11	-36	-43
25th pctile	0	-10	-18
Median	0	0	0
75th pctile	1	0	0
90th pctile	12	15	1
R2	0.09	0.19	0.13
Observations	998	998	998

Notes: In each column, the dependent variable is the difference in self-reported thermometer ratings towards two groups. Only participants from the control group are included. In column (1), the dependent variable is the difference in thermometer ratings towards college graduates and non-college graduates. In column (2), the dependent variable is the difference in thermometer ratings towards White and Black Americans. In column (3), the dependent variable is the difference in thermometer ratings towards men and women. "IAT-d-score" refers to the IAT d-score (the difference in average response times between the stereotypical and nonstereotypical blocks in our education-IAT measure, divided by the standard deviation of response times). All regressions include the baseline set of controls. In all columns, we weight the regressions by probability weights to match our survey sample to a representative US population by age, gender, race, and educational attainment. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9. Summary statistics for variables of interest

	All		Control		Treatment	
	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
Fixed factors important	61.52	23.93	55.86	24.05	67.12	22.46
Donation	26.00	25.07	21.62	21.70	30.32	27.34
Ex-ante policy	7.95	2.70	7.55	3.01	8.35	2.28
Ex-post redistribution	1.68	0.32	1.69	0.32	1.67	0.32
D-score	0.41	1.01	0.42	0.98	0.41	1.04

Notes: This table displays summary statistics for the main outcomes of interest in our main survey (N=2,008). All variables apart from the d-score is not standardized. Row 1: the variable of interest is beliefs in the role of fixed factors (range = 0-100). Row 2: the variable of interest is donations to the National College Attainment Network (max = 100 USD). Row 3: the variable of interest is support for ex-ante policies (range=0-10). Row 4: the outcome is support for ex-post redistribution measured by the desired earnings ratio between college to non-college graduates. Row 5: the outcome is the IAT d-score (the difference in average response times between the stereotypical and nonstereotypical blocks in our education-IAT measure, divided by the standard deviation of response times).

Table A10. Summary statistics for donation behavior

	Control	Treatment	Difference	P-val
	(1)	(2)	(3)	(4)
% Donate (main)	0.75	0.85	0.10	0.00
% Donate (follow)	0.69	0.73	0.05	0.04
Amount donated | donate (main)	28.92	35.82	6.90	0.00
Amount donated | donate (follow)	30.63	36.66	6.03	0.00

Notes: This table presents summary statistics donation behavior. Column (1) presents statistics for the control group while column (2) presents statistics for the treatment group. Rows (1) and (2) present the percentage of participants in tje main survey and follow-up survey who donated a positive amount. Rows (3) and (4) present the mean amount donated by participants in the main survey and follow-up survey, conditional on donating a positive amount.

Table A11. Investigating heterogeneity by educational attainment in effects on real behavior

	Main survey			Follow-up		
	Numeracy	Comprehension	Altruism	Numeracy	Comprehension	Altruism
	(1)	(2)	(3)	(4)	(5)	(6)
(1) Treated	0.22***	0.19***	0.19***	0.13*	0.12*	0.09
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
(2) Treated × College grad	0.22**	0.25***	0.23**	0.23**	0.25**	0.27**
	(0.09)	(0.10)	(0.09)	(0.11)	(0.11)	(0.11)
(3) College grad	-0.25***	-0.30***	-0.27***	-0.10	-0.12	-0.12*
	(0.07)	(0.07)	(0.06)	(0.08)	(0.08)	(0.07)
(1) + (2)	0.43***	0.44***	0.42***	0.36***	0.36***	0.36***
	(0.06)	(0.06)	(0.06)	(0.08)	(0.08)	(0.08)
R2	0.08	0.07	0.12	0.06	0.06	0.08
Observations	2,008	1,941	2,008	1,645	1,595	1,645

Notes: This table investigates heterogeneous treatment effects by educational attainment on donation choice. The dependent variable is the amount donated (out of 100 USD) to the National College Attainment Network (an NGO that aims to increase access to tertiary education), standardized by using the mean and standard deviation in the control group. Columns (1) and (4) include additional controls for numeracy. Columns (2) and (5) exclude participants who failed the comprehension check after being presented with information. Columns (3) and (6) include controls for altruism. "College grad" equals 1 if the participant has a 4-year college degree. All regressions include the baseline set of controls. In columns (1)-(3), we weight the regressions by probability weights to match our survey sample to a representative US population by age, gender, race, and educational attainment. In columns (4)-(6), we weight the regressions to account for attrition between the main and follow-up survey. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12. Treatment effects on support for expanding the Pell Grant

	Main survey			Follow-up		
	(1)	(2)	(3)	(4)	(5)	(6)
(1) Treated	0.24***	0.16	0.22	0.09	0.11	0.09
	(0.07)	(0.10)	(0.15)	(0.06)	(0.08)	(0.12)
(2) Treated × College grad		0.25*			-0.05	
		(0.13)			(0.12)	
(3) Treated × Underest ratio			-0.24			-0.08
			(0.23)			(0.21)
(4) Underest ratio			0.29			0.13
			(0.20)			(0.15)
(5) College grad	-0.10	-0.22**	-0.10	-0.07	-0.05	-0.07
	(0.07)	(0.10)	(0.07)	(0.07)	(0.09)	(0.07)
(1) + (2)		0.41***			0.06	
		(0.09)			(0.09)	
(1) + (3)			-0.02			0.01
			(0.27)			(0.23)
R2	0.21	0.21	0.22	0.24	0.24	0.24
Observations	1,000	1,000	1,000	823	823	823

Notes: The table shows estimates from our main regressions where the dependent variable is support for expanding the Pell Grant, standardized by using the mean and standard deviation in the control group. Participants are randomly asked about support for expanding the Pell Grant (the federal financial aid program for low-income college students) or automatic college application fee waivers. "Underest ratio" equals 1 if the participant underestimated the ratio of the proportion of college students who grew up in high-income households to the proportion of college students who grew up in low-income households. "College grad" equals 1 if the participant has a 4-year college degree. In columns (3) and (6), we also include an interaction term between treatment status and an indicator for whether the participant underestimated the proportion of high-income college attendees, and an interaction term between treatment status and an indicator for whether the participant overestimated the proportion of low-income college attendees. In addition to the baseline set of controls, all regressions include controls for the perceived effectiveness of the policy (4 indicators for each point on a 5-point Likert scale, excluding the omitted scale-point). In columns (1)-(3), we weight the regressions by probability weights to match our survey sample to a representative US population by age, gender, race, and educational attainment. In columns (4)-(6), we weight the regressions to account for attrition between the main and follow-up survey. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13. Treatment effects on support for automatic college application fee waivers

	Main survey			Follow-up		
	(1)	(2)	(3)	(4)	(5)	(6)
(1) Treated	0.32***	0.34***	0.14	0.09	0.02	-0.01
	(0.07)	(0.09)	(0.12)	(0.06)	(0.08)	(0.10)
(2) Treated × College grad		-0.05			0.15	
		(0.13)			(0.12)	
(3) Treated × Underest ratio			0.42*			-0.03
			(0.22)			(0.19)
(4) Underest ratio			-0.23			0.20
			(0.18)			(0.16)
(5) College grad	-0.03	-0.00	-0.03	-0.06	-0.13	-0.07
	(0.07)	(0.10)	(0.07)	(0.07)	(0.10)	(0.07)
(1) + (2)		0.29***			0.18*	
		(0.09)			(0.09)	
(1) + (3)			0.56**			-0.05
			(0.25)			(0.21)
R2	0.20	0.20	0.21	0.17	0.17	0.18
Observations	1,008	1,008	1,008	818	818	818

Notes: The table shows estimates from our main regressions where the dependent variable is support for colleges automatically exempting low-income students from paying an application fee, standardized by using the mean and standard deviation in the control group. Participants are randomly asked about support for expanding the Pell Grant (the federal financial aid program for low-income college students) or automatic college application fee waivers. "Underest ratio" equals 1 if the participant underestimated the ratio of the proportion of college students who grew up in high-income households to the proportion of college students who grew up in low-income households. "College grad" equals 1 if the participant has a 4-year college degree. In columns (3) and (6), we also include an interaction term between treatment status and an indicator for whether the participant underestimated the proportion of high-income college attendees, and an interaction term between treatment status and an indicator for whether the participant overestimated the proportion of low-income college attendees. In addition to the baseline set of controls, all regressions include controls for the perceived effectiveness of the policy (4 indicators for each point on a 5-point Likert scale, excluding the omitted scale-point). In columns (1)-(3), we weight the regressions by probability weights to match our survey sample to a representative US population by age, gender, race, and educational attainment. In columns (4)-(6), we weight the regressions to account for attrition between the main and follow-up survey. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14. Robustness checks for treatment effects on support for ex-ante policies

	Original + effect on self/others			Placebo policy		
	(1)	(2)	(3)	(4)	(5)	(6)
(1) Treated	0.23***	0.19***	0.11	0.06	0.03	-0.04
	(0.04)	(0.06)	(0.08)	(0.04)	(0.06)	(0.09)
(2) Treated × College grad		0.10			0.09	
		(0.08)			(0.08)	
(3) Treated × Underest ratio			0.21			-0.05
			(0.15)			(0.15)
(4) Underest ratio			-0.03			0.06
			(0.13)			(0.11)
(5) College grad	-0.01	-0.06	-0.01	0.01	-0.03	0.02
	(0.05)	(0.07)	(0.05)	(0.04)	(0.06)	(0.04)
(1) + (2)		0.29***			0.12**	
		(0.06)			(0.05)	
(1) + (3)			0.32*			-0.09
			(0.17)			(0.16)
R2	0.34	0.34	0.35	0.46	0.46	0.47
Observations	2,008	2,008	2,008	2,008	2,008	2,008

Notes: The table examines the robustness of the estimated treatment effects on support for ex-ante policies. In columns (1) to (3), the dependent variable is support for ex-ante policies (Pell Grant or college application fee waivers) and controls for perceptions about how the policy would affect Blacks, Whites, women, men, and the participant personally are included (4 indicators for each point on a 5-point Likert scale, excluding the omitted scale-point). In columns (4) to (6), the dependent variable is support for a placebo policy (quotas for promoting and hiring women). All outcome variables are standardized by using the mean and standard deviation in the control group. "Underest ratio" equals 1 if the participant underestimated the ratio of the proportion of college students who grew up in high-income households to the proportion of college students who grew up in low-income households. "College grad" equals 1 if the participant has a 4-year college degree. In columns (3) and (6), we also include an interaction term between treatment status and an indicator for whether the participant underestimated the proportion of high-income college attendees, and an interaction term between treatment status and an indicator for whether the participant overestimated the proportion of low-income college attendees. In addition to the baseline set of controls, all regressions include controls for the perceived effectiveness of the policy (4 indicators for each point on a 5-point Likert scale, excluding the omitted scale-point). In all columns, we weight the regressions by probability weights to match our survey sample to a representative US population by age, gender, race, and educational attainment. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15. Mediation analysis to decompose total effect into indirect and direct effects: Explicit attitudes

Outcome	ACME (Indirect effect)	ADE (Direct effect)	% mediated	P-value (ACME)
	(1)	(2)	(3)	(4)
Main survey				
Donation	-0.001	0.292***	0.00%	0.80
Policy support	-0.001	0.281***	0.00%	0.81
Redistribution	-0.001	-0.088*	9.82%	0.43
Follow-up survey				
Donation	0.003	0.215***	1.27%	0.65
Policy support	-0.000	0.097*	0.00%	0.94
Redistribution	-0.004	-0.155**	2.30%	0.65

Notes: This table reports the decomposition of the total treatment effect for each outcome variable (measured in SDs), into the sum of the indirect effect (Average Causal Mediation Effect or ACME; stereotypes) and the direct effect (Average Direct Effect or ADE; conscious cognition). The mediating variable is explicit attitudes towards the less-educated, measured using a "feeling thermometer" question or a "trust point" question (higher numbers indicate stronger negative attitudes towards the less-educated). % mediated refers to the size of the ACME, expressed as a percentage of the total effect. Where the ACME and ADE have opposite signs, the % mediated is zero by definition. The p-value corresponds to the null hypothesis that the proportion mediated is zero. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A16. Heterogeneous effects by parental education

	Beliefs	Beliefs(F)	Donations	Donations(F)	Ex-ante	Ex-ante(F)	Ex-post	Ex-post(F)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Treated	0.55***	0.44***	0.21***	0.21***	0.20***	0.07	-0.06	-0.08
	(0.07)	(0.06)	(0.06)	(0.07)	(0.06)	(0.05)	(0.06)	(0.07)
(2) Treated X Parent college	-0.07	-0.02	0.22**	0.07	0.06	-0.03	-0.11	-0.07
	(0.12)	(0.09)	(0.11)	(0.11)	(0.09)	(0.08)	(0.11)	(0.10)
(3) Parent college	-0.09	-0.02	-0.07	-0.08	-0.05	-0.10	-0.00	-0.06
	(0.17)	(0.12)	(0.14)	(0.15)	(0.12)	(0.11)	(0.14)	(0.12)
(1) + (2)	0.48***	0.42***	0.43***	0.28***	0.26***	0.04	-0.17*	-0.15**
	(0.10)	(0.07)	(0.09)	(0.08)	(0.07)	(0.06)	(0.09)	(0.07)
R2	0.15	0.14	0.08	0.06	0.36	0.26	0.10	0.10
Observations	2,008	1,674	2,008	1,645	2,008	1,641	2,008	1,646

Notes: The table examines heterogeneous treatment effects by parental education. "Parent college" equals 1 if at least one parent attended college and zero otherwise. In columns (1) and (2), the outcome is beliefs in the role of fixed factors (relative to malleable factors). In columns (3) and (4), the outcome is donations to the National College Attainment Network. In columns (5) and (6), the outcome is support for ex-ante policies. In columns (7) and (8), the outcome is support for ex-post redistribution. All regressions include the baseline set of controls. In odd columns (main survey), we weight the regressions by probability weights to match our survey sample to a representative US population by age, gender, race, and educational attainment. In even columns (follow-up survey), we weight the regressions to account for attrition between the main and follow-up survey. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A17. Heterogeneous effects by experience of educational mobility

	Beliefs	Beliefs(F)	Donations	Donations(F)	Ex-ante	Ex-ante(F)	Ex-post	Ex-post(F)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Treated	0.54***	0.41***	0.25***	0.18***	0.19***	0.03	-0.11*	-0.13**
	(0.07)	(0.05)	(0.06)	(0.06)	(0.05)	(0.05)	(0.06)	(0.06)
(2) Treated X Educ mobile	-0.05	0.05	0.19*	0.19*	0.11	0.11	0.04	0.08
	(0.11)	(0.10)	(0.10)	(0.12)	(0.09)	(0.09)	(0.10)	(0.10)
(3) Eudc mobile	0.00	-0.01	-0.04	-0.17	-0.05	-0.05	0.04	-0.01
	(0.12)	(0.11)	(0.12)	(0.13)	(0.10)	(0.10)	(0.12)	(0.11)
(1) + (2)	0.49***	0.46***	0.44***	0.37***	0.31***	0.13*	-0.06	-0.05
	(0.08)	(0.08)	(0.08)	(0.10)	(0.07)	(0.07)	(0.08)	(0.08)
R2	0.15	0.14	0.07	0.06	0.36	0.26	0.10	0.13
Observations	2,008	1,674	2,008	1,645	2,008	1,641	2,008	1,646

Notes: The table examines heterogeneous treatment effects by experiences of educational mobility. "Educ mobile" equals 1 if the participant attended college but neither parent did and zero otherwise. In columns (1) and (2), the outcome is beliefs in the role of of fixed factors (relative to malleable factors). In columns (3) and (4), the outcome is donations to the National College Attainment Network. In columns (5) and (6), the outcome is support for ex-ante policies. In columns (7) and (8), the outcome is support for ex-post redistribution. "(F)" denotes outcomes in the followup survey. All regressions include the baseline set of controls. In odd columns (main survey), we weight the regressions by probability weights to match our survey sample to a representative US population by age, gender, race, and educational attainment. In even columns (follow-up survey), we weight the regressions to account for attrition between the main and follow-up survey. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A18. Heterogeneous effects by household income

	Beliefs	Beliefs(F)	Donations	Donations(F)	Ex-ante	Ex-ante(F)	Ex-post	Ex-post(F)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Treated	0.53***	0.42***	0.24***	0.18***	0.21***	0.04	-0.11	-0.12*
	(0.08)	(0.06)	(0.06)	(0.07)	(0.06)	(0.05)	(0.07)	(0.07)
(2) Treated X Abv-med income	-0.03	0.02	0.16	0.17	0.02	0.08	0.06	0.02
	(0.12)	(0.09)	(0.11)	(0.11)	(0.09)	(0.08)	(0.11)	(0.10)
(3) Abv-med income	-0.07	-0.07	0.11	0.08	-0.03	-0.19***	-0.02	0.11
	(0.09)	(0.07)	(0.07)	(0.08)	(0.07)	(0.06)	(0.07)	(0.07)
(1) + (2)	0.50***	0.44***	0.40***	0.35***	0.23***	0.12*	-0.06	-0.10
	(0.09)	(0.07)	(0.09)	(0.09)	(0.07)	(0.07)	(0.08)	(0.07)
R2	0.14	0.14	0.07	0.06	0.36	0.26	0.10	0.12
Observations	1,971	1,645	1,971	1,616	1,971	1,612	1,971	1,617

Notes: The table examines heterogeneous treatment effects by income. “Abv-med income” ("above median income") is defined as having a household income of 65,000 USD or more. The sample size in this analysis is smaller than that in the main analysis due to 37 participants preferring not to report information on their income. In columns (1) and (2), the outcome is beliefs in the role of fixed factors (relative to malleable factors). In columns (3) and (4), the outcome is donations to the National College Attainment Network. In columns (5) and (6), the outcome is support for ex-ante policies. In columns (7) and (8), the outcome is support for ex-post redistribution. “(F)” denotes outcomes in the follow-up survey. In addition to the baseline set of controls, all regressions include controls for the perceived effectiveness of the policy (4 indicators for each point on a 5-point Likert scale, excluding the omitted scale-point). In odd columns (main survey), we weight the regressions by probability weights to match our survey sample to a representative US population by age, gender, race, and educational attainment. In even columns (follow-up survey), we weight the regressions to account for attrition between the main and follow-up survey. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A19. Heterogeneous effects by political affiliation

	Beliefs	Beliefs(F)	Donations	Donations(F)	Ex-ante	Ex-ante(F)	Ex-post	Ex-post(F)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Treated	0.57***	0.42***	0.35***	0.43***	0.31***	0.06	-0.11	-0.04
	(0.09)	(0.07)	(0.08)	(0.09)	(0.08)	(0.07)	(0.09)	(0.08)
(2) Treated X Democrat	-0.15	0.00	-0.16	-0.31**	-0.25***	-0.02	0.04	-0.09
	(0.12)	(0.10)	(0.11)	(0.12)	(0.09)	(0.09)	(0.12)	(0.11)
(3) Treated X Republican	0.10	0.06	0.06	-0.26*	0.12	0.10	-0.02	-0.10
	(0.17)	(0.14)	(0.14)	(0.15)	(0.14)	(0.14)	(0.15)	(0.13)
(4) Democrat	0.31***	0.28***	0.26***	0.34***	0.34***	0.28***	-0.06	-0.20**
	(0.09)	(0.07)	(0.07)	(0.08)	(0.07)	(0.06)	(0.08)	(0.08)
(5) Republican	-0.18	-0.29***	-0.09	-0.11	-0.52***	-0.54***	0.25**	0.33***
	(0.14)	(0.10)	(0.09)	(0.10)	(0.12)	(0.10)	(0.10)	(0.09)
(1) + (2)	0.42***	0.42***	0.19**	0.13	0.06	0.03	-0.07	-0.13*
	(0.08)	(0.06)	(0.08)	(0.08)	(0.05)	(0.05)	(0.07)	(0.07)
(1) + (3)	0.67***	0.48***	0.41***	0.18	0.43***	0.15	-0.13	-0.14
	(0.15)	(0.12)	(0.12)	(0.12)	(0.12)	(0.13)	(0.13)	(0.10)
R2	0.15	0.14	0.07	0.06	0.37	0.26	0.10	0.13
Observations	2,008	1,674	2,008	1,645	2,008	1,641	2,008	1,646

Notes: The table examines heterogeneous treatment effects by political affiliation. The omitted category is "Independents" and those with no political affiliation. In columns (1) and (2), the outcome is beliefs in the role of fixed factors (relative to malleable). In columns (3) and (4), the outcome is donations to the National College Attainment Network. In columns (5) and (6), the outcome is support for ex-ante policies. In columns (7) and (8), the outcome is support for ex-post redistribution. "(F)" denotes outcomes in the follow-up survey. All regressions include the baseline set of controls. In odd columns (main survey), we weight the regressions by probability weights to match our survey sample to a representative US population by age, gender, race, and educational attainment. In even columns (follow-up survey), we weight the regressions to account for attrition between the main and follow-up survey. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A20. Heterogeneous effects by confidence in beliefs about educational inequalities

	Beliefs	Beliefs(F)	Donations	Donations(F)	Ex-ante	Ex-ante(F)	Ex-post	Ex-post(F)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Treated	0.54***	0.44***	0.29***	0.22***	0.18***	0.05	-0.08	-0.11**
	(0.06)	(0.05)	(0.06)	(0.06)	(0.05)	(0.05)	(0.06)	(0.05)
(2) Treated X Conf about Q1	-0.02	-0.15	0.08	-0.03	-0.20	0.07	0.27	0.12
	(0.21)	(0.16)	(0.16)	(0.16)	(0.15)	(0.14)	(0.18)	(0.16)
(3) Treated X Conf about Q5	-0.04	0.03	-0.01	0.14	0.28**	0.02	-0.21	-0.02
	(0.18)	(0.12)	(0.13)	(0.14)	(0.13)	(0.11)	(0.15)	(0.12)
(4) Conf about Q1	0.01	0.08	0.20*	0.03	0.13	-0.11	-0.03	-0.05
	(0.15)	(0.12)	(0.10)	(0.11)	(0.12)	(0.10)	(0.12)	(0.11)
(4) Conf about Q5	-0.11	-0.10	-0.04	-0.07	-0.18*	0.00	0.13	0.03
	(0.11)	(0.09)	(0.08)	(0.09)	(0.11)	(0.08)	(0.10)	(0.09)
(1) + (2)	0.52**	0.29*	0.37**	0.19	-0.03	0.11	0.19	0.01
	(0.21)	(0.16)	(0.16)	(0.16)	(0.16)	(0.15)	(0.19)	(0.16)
(1) + (3)	0.50***	0.47***	0.28**	0.35***	0.46***	0.07	-0.29**	-0.13
	(0.17)	(0.12)	(0.13)	(0.13)	(0.12)	(0.10)	(0.14)	(0.11)
R2	0.15	0.14	0.08	0.06	0.36	0.26	0.10	0.13
Observations	2,008	1,674	2,008	1,645	2,008	1,641	2,008	1,646

Notes: The table examines heterogeneous treatment effects by how confident participants are in their beliefs about educational inequality. Confidence is measured on a 5-point Likert scale, ranging from “not confident” to “extremely confident". “Conf about Q1” equals 1 if the participant is very or extremely confident about their estimates of the proportion of college attendees who are low-income. “Conf about Q5” equals 1 if the participant is very or extremely confident about their estimates of the proportion of college attendees who are high-income. In columns (1) and (2), the outcome is beliefs in the role of fixed factors (relative to malleable). In columns (3) and (4), the outcome is donations to the National College Attainment Network. In columns (5) and (6), the outcome is support for ex-ante policies. In columns (7) and (8), the outcome is support for ex-post redistribution. “(F)” denotes outcomes in the follow-up survey. In addition to the baseline set of controls, all regressions include controls for the perceived effectiveness of the policy (4 indicators for each point on a 5-point Likert scale, excluding the omitted scale-point). In odd columns (main survey), we weight the regressions by probability weights to match our survey sample to a representative US population by age, gender, race, and educational attainment. In even columns (follow-up survey), we weight the regressions to account for attrition between the main and follow-up survey. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Appendix figures

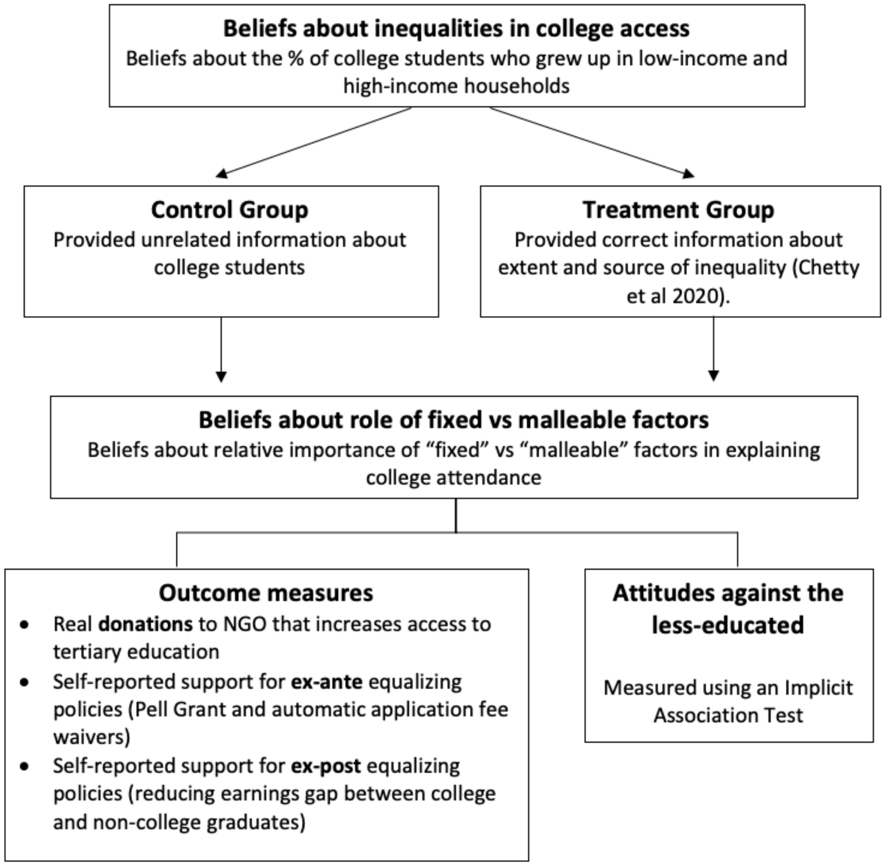


Figure A1. Overview of the main experiment

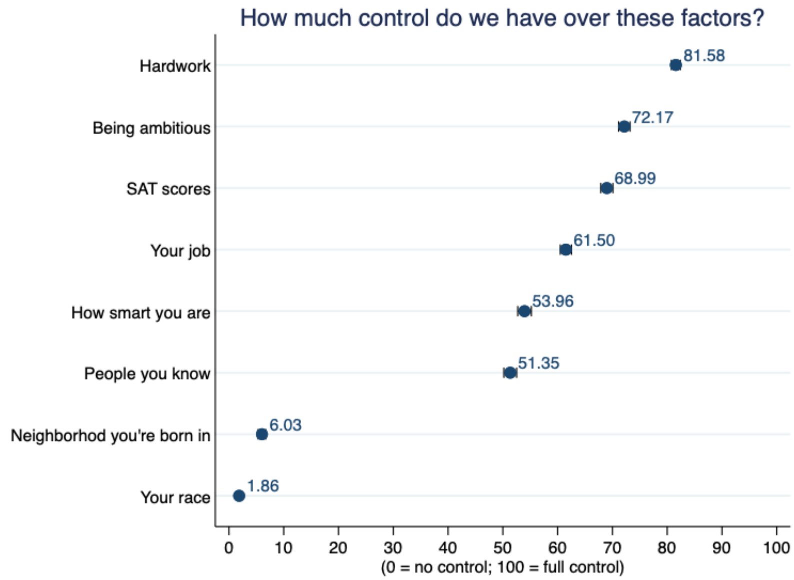


Figure A2. Beliefs about the degree to which different factors are "fixed" or "malleable" (pooled across the treatment and control groups). The differences in beliefs between the treatment and control group are not statistically significant.

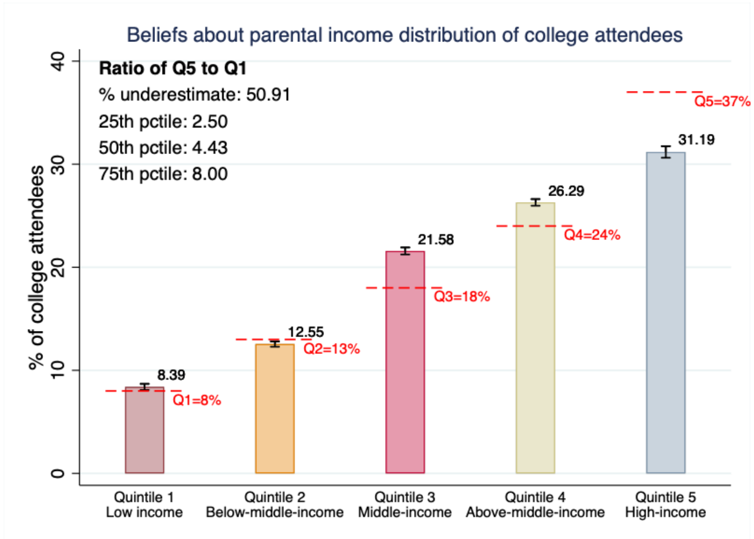


Figure A3. Beliefs about % of college attendees from each quintile of the income distribution. Bars represent averages across participants, with 95% confidence intervals. Red dashed lines represent the correct answer from Chetty et al. (2020b).

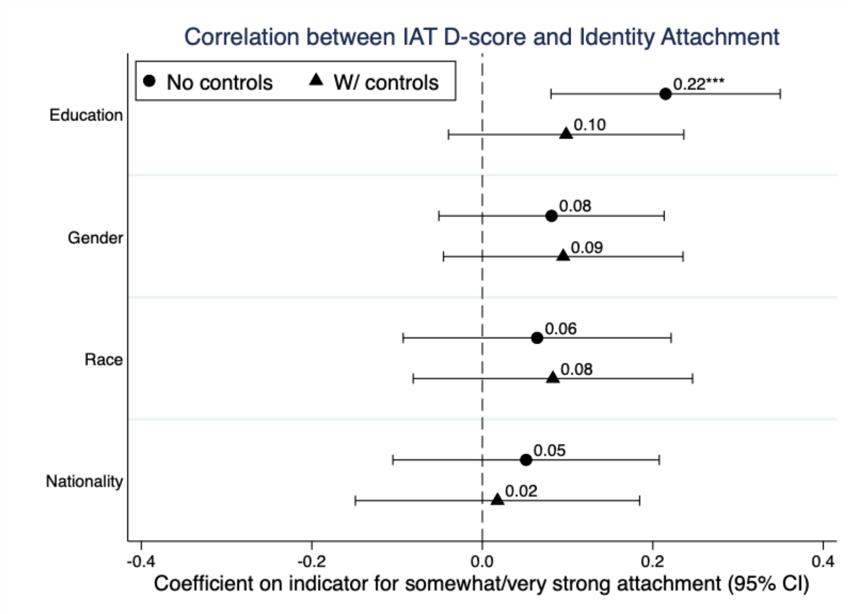


Figure A4. Correlations between IAT d-score and participants' strength of attachment to different identities.

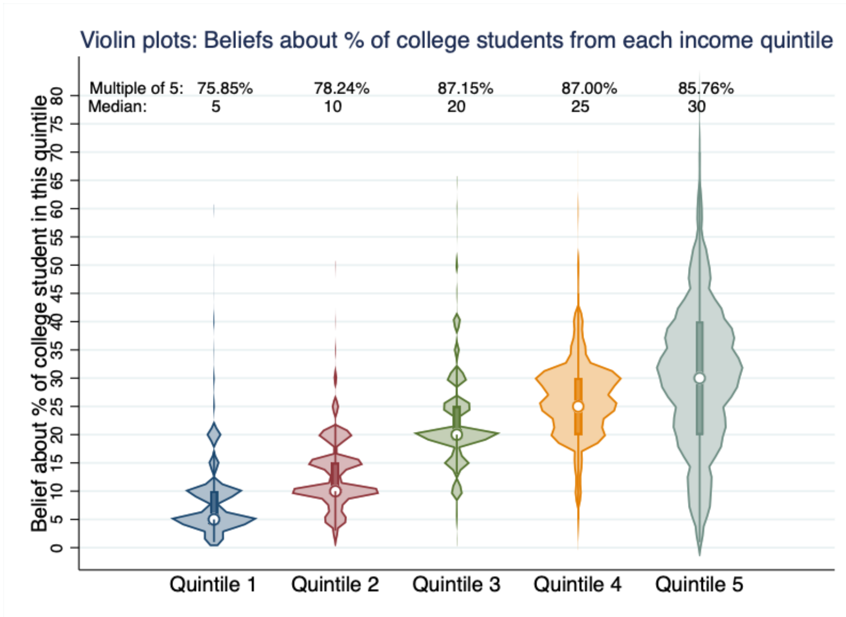


Figure A5. Violin plots showing the distribution of beliefs about the number of college attendees in each income quintile. The numbers above each violin plot report the percentage of participants who reported a "focal" value (multiple of 5) for that quintile, and the median belief for that quintile (respectively).

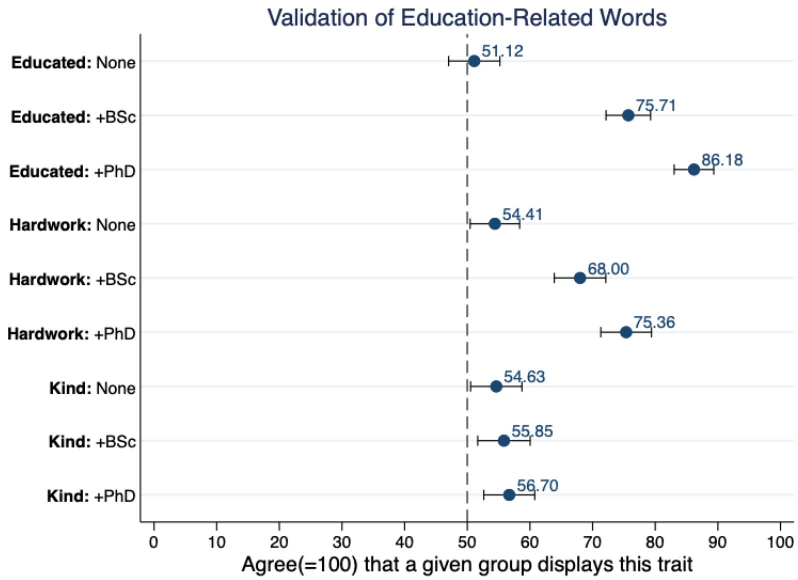
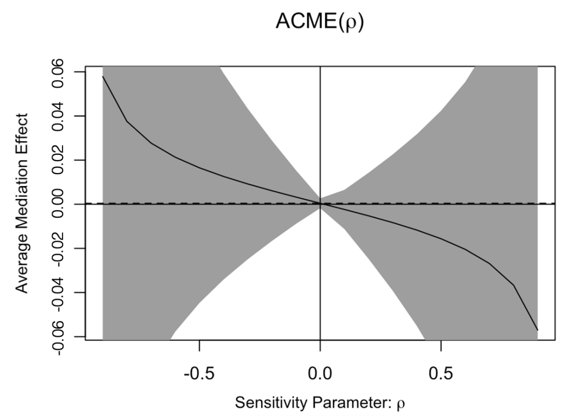
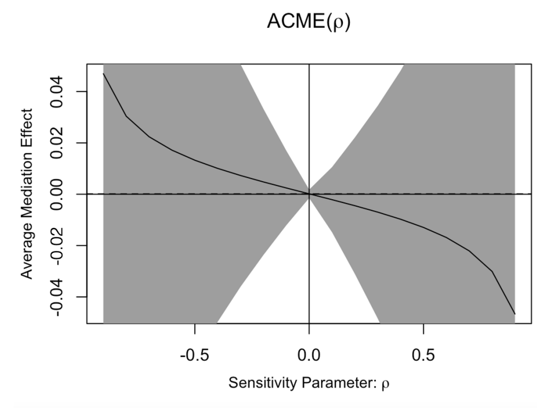


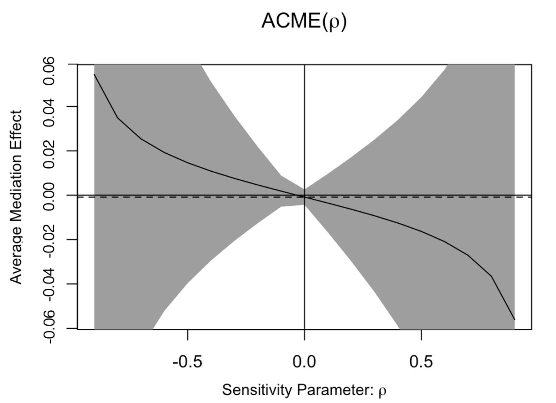
Figure A6. Associations between traits and educational qualifications. Sample consists of participants recruited through a separate survey (N=100).



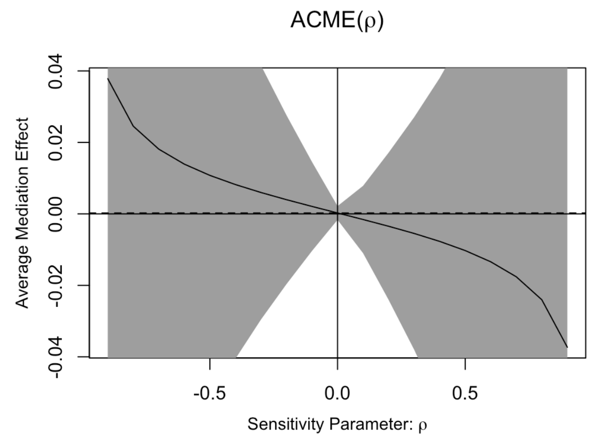
(a) Main survey: Donations



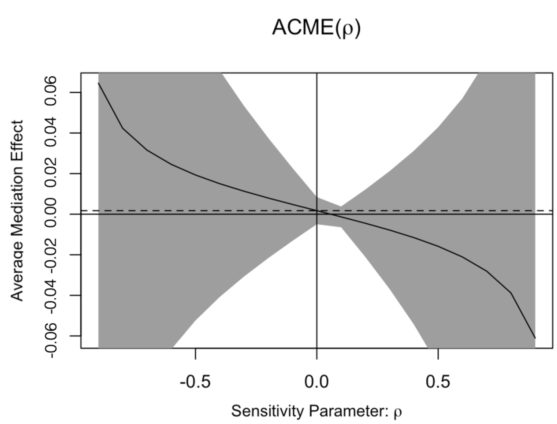
(b) Followup survey: Donations



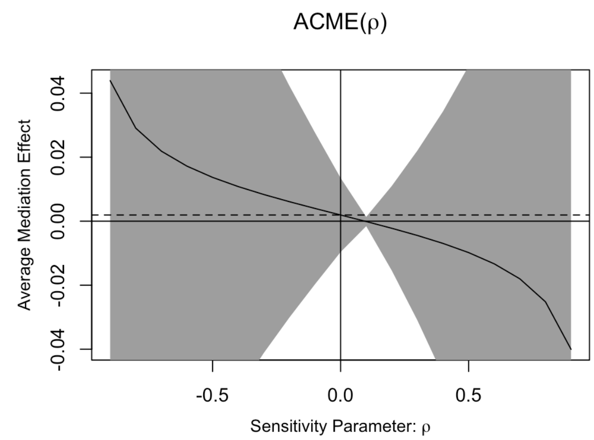
(c) Main survey: support for ex-ante policies



(d) Followup survey: support for ex-ante policies

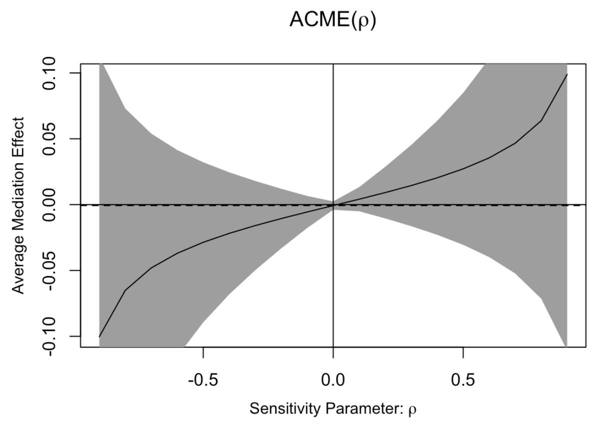


(e) Main survey: support for redistribution

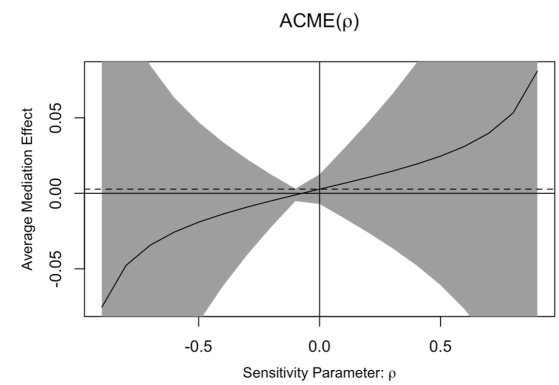


(f) Followup survey: support for redistribution

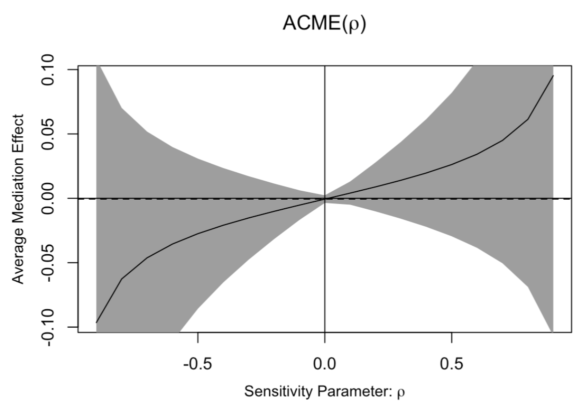
Figure A7. Sensitivity analysis for mediation analysis of *implicit* stereotypes. Each figure shows how the estimated average causal mediation effect (ACME) changes for the respective outcome of interest in the main (left column) and follow-up (right column) surveys as we change the correlation between the residuals of the mediation equation and the residuals of the outcome equation (ρ).



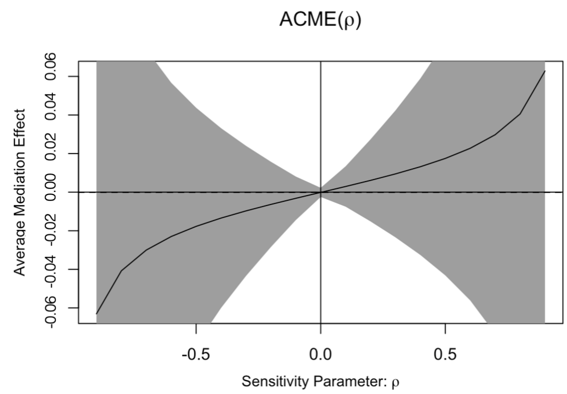
(a) Main survey: Donations



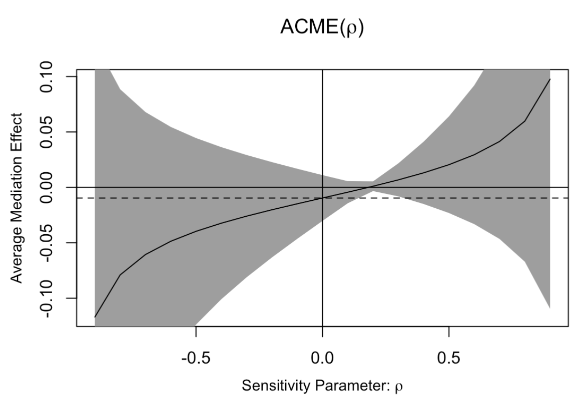
(b) Followup survey: Donations



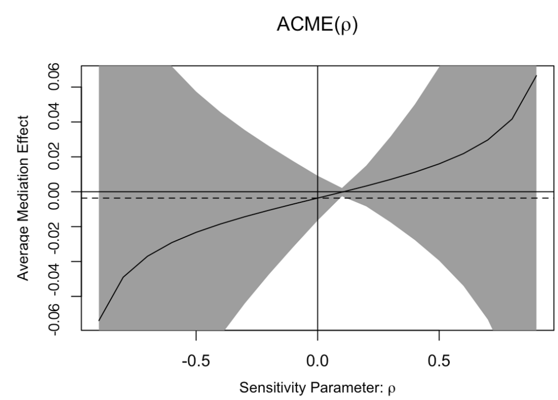
(c) Main survey: support for ex-ante policies



(d) Followup survey: support for ex-ante policies



(e) Main survey: support for redistribution



(f) Followup survey: support for redistribution

Figure A8. Sensitivity analysis for mediation analysis of *explicit* stereotypes. Each figure shows how the estimated average causal mediation effect (ACME) changes for the respective outcome of interest in the main (left column) and follow-up (right column) surveys as we change the correlation between the residuals of the mediation equation and the residuals of the outcome equation (ρ).

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E. Experimental design

Figure 1 shows a flowchart of the experiment.

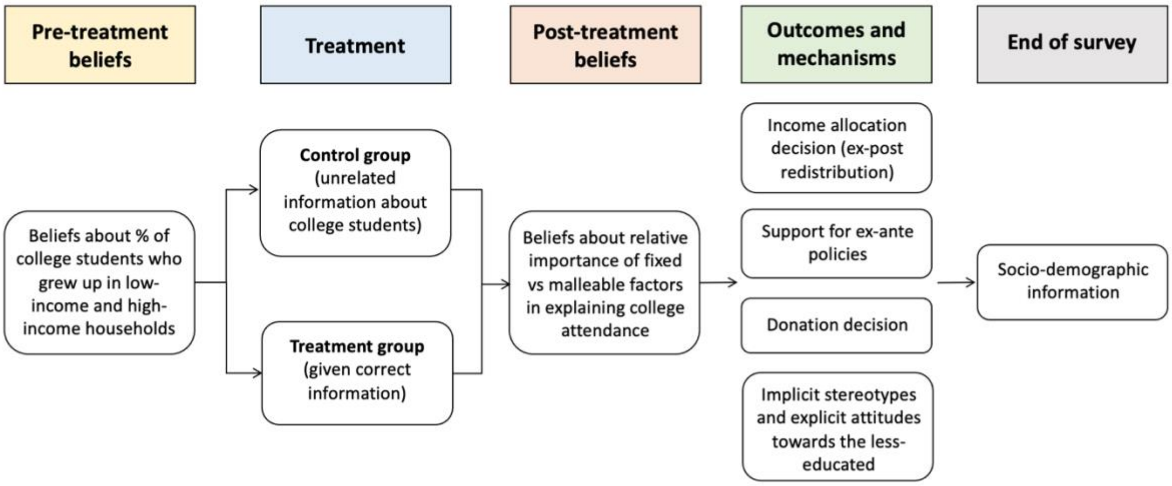

Figure 1. Illustration of survey experiment

E.1. Pre-treatment beliefs about socioeconomic inequality in college attendance

Participants will be asked to estimate how access to higher education varies across socioeconomic status. Specifically, participants are asked to consider 100 four-year-college attendees born in 1980-1982 and estimate how many come from each quintile of the income distribution. If children from each quintile of the income distribution are equally likely to attend college when they grow up (i.e. external circumstances ("fixed" factors) play a small role in influencing academic outcomes), then each quintile should be represented equally in the college student population. The correct answer to this question is provided by Chetty et al. (2020), who use federal data on college attendance.

To ensure that participants pay sufficient attention to answering these questions, we will provide monetary incentives. Specifically, participants will be told that they are automatically entered into a lottery to win $50 and that their chances of winning this lottery increases the closer their answers are to the correct answer. Note that the answers to these questions are difficult to find online because the statistics are computed using the statistics reported in Table VI of Chetty et al. (2020) (using the relevant rows for four-year colleges). To prevent participants from rushing through this part of the survey, we set a timer so that participants have to spend at least one minute on this question before progressing. We ask participants how confident they feel about their responses on a 5-point scale, ranging from 1 (not confident at all) to 5 (extremely confident).

E.2. Treatment: Information about socioeconomic inequality in college attendance

Participants will be randomly assigned to a control group or a treatment group. The treatment group will be given feedback on their response (whether it is an over or underestimate), the correct answers to the college attendance questions for the bottom and top quintile (in the form of an infographic), and a non-technical interpretation of these statistics.

E.3. Control group

The control group will receive some information that is unrelated to sources of inequalities in educational attainment (the average employment rate of college attendees at age 30 and the different types of colleges that this cohort attended), as reported in Chetty et al. (2020).

E.4. Post-treatment beliefs in the sources of educational inequality

Participants are asked about the importance of “fixed” and “malleable” factors in explaining variation in college attendance. “Fixed” factors are defined as factors that are outside one’s control (e.g. parents’ socioeconomic status). “Malleable” factors are defined as factors that are within one’s control (e.g. how hard an individual works). Aside from this guidance, participants are free to interpret “fixed” and “malleable” as they wish.

E.5. Outcomes: Income redistribution decision

Participants will be given the current income difference (expressed as a ratio) between the average college graduate and average non-college graduate and asked what they think the income ratio between these two individuals should be.

In addition to beliefs about sources of educational inequality, preferences over redistribution may be influenced by various factors, including perceived productivity differences between college and non-college graduates. We measure participants’ perception about the relative productivity differences between a college and noncollege graduate.

E.6. Outcomes: Real charitable donation decision

Participants will be told they have been automatically enrolled in a lottery for $100 and, if they win, they can choose to donate some (or all or none) of their winnings to a charity whose primary mission is to tackle inequalities in educational attainment at the tertiary level.

E.7. Outcomes: Support for ex-ante equalizing policies

We present participants with information on one of two policies that aim to increase equality of college attendance by reducing financial barriers: expanding the size of the Pell Grant, and encouraging colleges to offer automatic application fee waivers for low-income students. Participants are shown one of these policies for the main survey and the other for the follow-up survey. They are then asked how much they would support the given policy. We ask participants whether they believe the policy will be effective in increasing opportunities to access higher education and how they think various groups, including themselves/people they care about, would personally be affected if such policies were implemented.

To assess whether these effects are limited to education policies, we also present participants with information on a non-education related policy (e.g. positive discrimination towards women in the workplace). We ask them corresponding questions about perceived effectiveness of these non-educational related policies and how it is likely to affect various groups.

E.8. Mechanisms: Implicit stereotypes and explicit attitudes towards the less educated

We design an implicit measure of stereotypes about the less-educated based on the Implicit Association Test (IAT). The IAT is a computer-based tool developed by psychologists (Greenwald et al., 1998) and recently used by economists to study discrimination in the context of gender and race

(Carlana, 2019; Glover et al., 2017; Lowes et al., 2015). In our version of the IAT, we assess the ease with which participants make pleasant or unpleasant associations between typically white male names, which are either listed with or without an educational qualification (e.g. BSc, J.D., PhD).

To assess whether participants are aware of their implicit stereotypes and/or are reluctant to express their true opinions due to social desirability bias, we also collect two explicit attitude measures: 1) a "feeling thermometer" indicator of "warmness" towards college graduates and non-college graduates, and 2) the extent to which participants trust college graduates compared to non-college graduates, using the "trust point" allocation question of (Enke et al., 2022). One measure will be used for the main study and another measure will be used for the follow-up study.

E.9. Demographic information

We ask participants the following information: gender, year of birth, ethnicity, educational attainment, party affiliation, number of children, participants' household income, state in which they reside.

We also collect the following variables to use as controls or potential dimensions of heterogeneity: parents' highest educational attainment, the socioeconomic status of the household in which they grew up, whether the participant considers particular socio-demographic factors (such as ethnicity or education level) important for their identity, participants' level of numeracy (Lipkus et al., 2001; Schwartz et al., 1997), self-reported altruism (Falk et al., 2018), and a version of the locus of control measure (Cobb-Clark and Schurer, 2013).

E.10. Follow-up study

We will conduct an "obfuscated follow-up study" approximately two weeks after the main study, to assess the persistence of any treatment effects and mitigate concerns that results from the main study are driven by experimenter demand effects (De Quidt et al., 2018; Zizzo, 2010). We chose a two-week time lag so that participants will have completed several other unrelated surveys between our main and follow-up study, so are less likely to remember completing the main survey.

To make the follow-up seem like an independent study, we will undertake the following measures:

1. Using a vague study title and study description in the recruitment notice, to avoid reminding participants of the main study's content
2. Changing the survey's layout and appearance, such as the illustrative images used and the font.
3. Using different consent forms (from different universities)
4. Asking participants a series of typical demographic questions
5. Asking participants questions about other topics first, leaving the main outcome questions to the end. Doing so will help obscure the purpose of the study.

E.11 References

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F. Survey questionnaire: Main experiment

A. Start of survey

A.1. Survey information

We are a group of non-partisan researchers. In this study, our goal is to understand your views about education and attitudes towards different groups in society. Our survey will give you an opportunity to express your own views.

It is very important for the success of our research that you answer honestly and read the questions very carefully before answering. Whenever you don't know an answer, just give your best guess. To ensure the quality of the survey data, your responses will be subject to sophisticated statistical control methods. **Responding without adequate effort may result in your responses being flagged for low quality.**

To take part, you must ordinarily **be a resident in the US** and be **at least 25 years old**. If you do not fulfil these requirements, please do not continue any further.

It is very important for the success of our research project that you **complete the entire survey**. This study will take you around **20 minutes**. We will compensate you via a bonus if you need significantly more than 20 minutes to complete this study. If you have already completed this survey, only your first complete response will be counted and be paid.

<u>Please complete this study on a computer or laptop, not a tablet or phone.</u> This study requires you to look at some images and they may not appear clearly on a small screen.

Your participation is entirely voluntary. If you do decide to take part, you can still withdraw at any point for any reason by closing the browser. If you choose to withdraw, your responses will not be recorded or used for the study.

To proceed, please tick the box that applies to you

- No, I would not like to participate
- Yes, I would like to participate and confirm that **I live in the US** and am **25 years old or older**

A.2. Attention check

Most modern theories of decision making recognize that decisions do not take place in a vacuum. Individual preferences and knowledge, along with situational variables can greatly impact the decision process. To demonstrate that you’ve read this much, just go ahead and select both "Strongly disagree" and "Strongly agree" among the alternatives below, no matter what your opinion is.

Do you agree or disagree with the following statement: *"It is easy to find accurate and reliable information in the media these days"*?

- Strongly disagree
- Somewhat disagree
- Neither agree nor disagree
- Somewhat agree
- Strongly agree

B. Beliefs about college attendance

B.1. Pre-treatment belief elicitation question

Opportunity to win a bonus: By answering this question, you are automatically entered into a lottery to win a bonus of $50. Your chance of winning this lottery depends on how close your answers are to the correct answers. The closer your numbers are to the correct answers, the higher your chance of winning.

A group of researchers are studying the higher education system in the U.S. They looked at data on individuals born between 1980 and 1982. Among this group, over 3.5 million individuals (around 1 out of 3 individuals) attended a 4-year college.

{page break}

Think about 100 random individuals from this group of students who attended a 4-year college ("college attendees").

These 100 college attendees grew up in one of the following households:
a.) A ***low-income*** household (<$30,000 per year)
b.) A ***below-middle-income*** household ($30,000-$55,800 per year)
c.) A ***middle-income*** household ($55,801-$89,700 per year)
d.) An ***above-middle-income*** household ($89,701 -$135,500 per year)
e.) A ***high-income*** household (>$135,501 per year)

Please fill in how many of these 100 college attendees grew up in each of these household groups.

According to these income group definitions, across the U.S. there is an equal number of households in each group. For example, 20% of households in the U.S. are low-income, 20% of households in the U.S. are below-middle-income, and so on. You can assume that across the U.S. all household income groups have the same number of children.

This means that: If you think that everyone is ***equally likely*** to attend a 4-year college, then ***20 out of 100*** college attendees would come from each of these groups. If you think that individuals who grew up in certain households are ***more likely*** to attend a 4-year college, ***more than 20 out of 100*** college attendees would come from that group.

There is at least 1 college attendee from each household income group. Since there are 100 college attendees, the numbers in each household income group must total 100.

[5 boxes for low income, below-middle income, middle-income, above-middle income, high income, and a 'total' box that sums the findings]

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B.2. Confidence Questions

You said that out of 100 college attendees, [respondent answer] grew up in a ***low-income*** household.

How confident are you about your answer?

- Not confident at all
- Slightly confident
- Moderately confident
- Very confident
- Extremely confident

You said that out of 100 college attendees, [respondent answer] grew up in a ***high-income*** household.

How confident are you about your answer?

- Not confident at all
- Slightly confident
- Moderately confident
- Very confident
- Extremely confident

C. Control group

C.1. Information treatment

What did the researchers find?
When the researchers looked at the earnings of these 100 college attendees in 2014 (when the individuals were 32-34 years old), they found that on average over 90% were in full-time or part-time employment.

The researchers found that individuals in this cohort attended various types of colleges. For example, some attended selective public colleges, some attended non-selective public colleges, and others attended for-profit colleges.

C.2. Comprehension check

The researchers found that among a group of individuals born between 1980 and 1982 who attended college...

- 50% were in full-time or part-time employment
- 60% were in full-time or part-time employment
- 70% were in full-time or part-time employment
- 80% were in full-time or part-time employment
- Over 90% were in full-time or part-time employment

D. Treatment group
D.1. Information treatment

[Feedback]
You said that if you met 100 random individuals who attended a 4-year college, you expect to find that...
[Respondent's answer here] of them grew up in a low-income household. This is correct/You have underestimated/overestimated the number of college attendees who grew up in a low-income household.
[Respondent's answer here] of them grew up in a high-income household. This is correct/You have underestimated/overestimated the number of college attendees who grew up in a low-income household.

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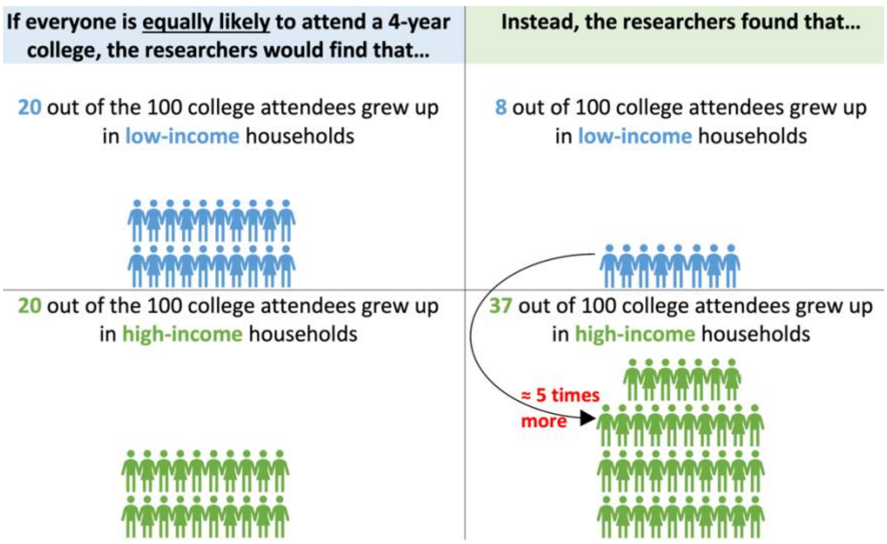
[Information]
What did the researchers find?



This means students from high-income families are almost **5 times more likely** to attend a 4-year college than those from low-income families.

Some people may think that this difference in college-related outcomes is due to low-income students not having good enough grades to go to college.

But the researchers found that when we look at low-income and high-income students with *the same test scores*, high-income students are still more likely to attend 4-year colleges than low-income students.

The researchers concluded that *almost two-thirds* of the difference in college-related outcomes between low-income and high-income students are due to factors related to parental income, even after controlling for how prepared the students are for college.

There are many reasons why parental income matters. Prior research has shown that among low-income students who get good enough test scores to apply to good colleges, the following factors are important *barriers to attending* college:

- **Lack of support** and guidance to apply to college
- Not being able to pay for college **application fees**
- Not being able to pay for college costs like **tuition fees**

D.2. Comprehension check

The researchers found that if you met 100 random individuals who attended a 4-year college, you would find that...

- 15 grew up in a *low-income* household and 50 grew up in a *high-income* household
- 12 grew up in a *low-income* household and 46 grew up in a *high-income* household
- 8 grew up in a *low-income* household and 37 grew up in a *high-income* household
- 5 grew up in a *low-income* household and 25 grew up in a *high-income* household

E. Post-Treatment Belief Elicitation

Consider two groups of individuals. All individuals in group 1 attended college. All individuals in group 2 did **not** attend college.

How important are the following factors in explaining this difference in college attendance between groups 1 and 2?

- **Fixed factors:** Factors that are *fixed* at birth (e.g. whether they are born into a rich or poor household)
- **Malleable factors:** Factors that are *not fixed* at birth (e.g. their mindset towards hard work)

Please use the slider below to indicate how important you think each factor is. Drag the slider to the *right* if you think *malleable* factors are more important. Drag the slider to the *left* if you think *fixed* factors are more important.

[Slider with 3 labels ("differences in malleable factors more important" (left), "differences in malleable and fixed factors equally important" (center), "differences in fixed factors more important" (right))

Note: Slider order is randomized across participants. Some participants get a slider where malleable factors are on the left and fixed factors are on the right. Other participants get a slider where malleable factors are on the right and fixed factors are on the left.

F. Outcomes

F.1. Income allocation

Generally speaking, college graduates earn more than non-college graduates.

According to recent data provided by the US Bureau of Labor Statistics, before taxes, for every $100 that the typical non-college graduate makes, the typical college graduate makes $173.

Some people consider this difference in earnings as fair. Other people consider this difference in earnings as unfair. One way to address unfair differences in earnings is through taxation (e.g. by increasing taxes on those who earn over a certain amount).

For every $100 that the typical non-college graduate makes, do you think the typical college graduate should make less than, equal to, or more than $173?

- **Less than $173**(Earnings difference should be **smaller**)
- **Equal to $173**(Earnings difference **doesn't need to change**)
- **More than $173**(Earnings difference should be **larger**)

{page break}

(If respondent selected "should be smaller")
You suggested that for every $100 that the non-college graduate makes, the typical college graduate should make **less than** $173.

How much do you think the typical college graduate should earn relative to the typical non-college graduate? (Please enter a number below $173)

(If respondent selected "should be larger")
You suggested that for every $100 that the non-college graduate makes, the typical college graduate should make **more than** $173.

How much do you think the typical college graduate should earn relative to the typical non-college graduate? (Please enter a number above $173)

F.2. Donation

By taking this survey, you are automatically enrolled in a lottery to win $100.

If you win the lottery, would you be willing to donate some of this money to the National College Attainment Network (NCAN)?

The NCAN is a charity that aims to increase access to college, especially among students underrepresented in postsecondary education. The NCAN does this by helping students prepare for and apply to college.

You can find out more about the NCAN by clicking here.

If you win the lottery, we will contact you in a few days to let you know. You will be paid this extra money (minus your donations) in addition to your payment for participating in the survey.

Use the slider below to indicate how much you would like to donate to the charity:
[Slider ranging from $0 to $100]

F.3. Policy Support

Pell Grant Question
[Note: Participants randomly receive information on either this question or the next one]

Even after they've been accepted to college, many low-income students cannot attend college because they cannot afford it. The **Pell Grant** is the federal government's financial aid program for low-income students who need help to pay for college costs (e.g. tuition, fees, room and board).

In 2022, the maximum size of the Pell Grant was $6,495. This covers roughly 25% of the average cost of attendance at a public four-year institution.

Some argue the federal government should **double the size of the Pell Grant** so that more low-income students can afford to attend college. By clicking here, you can find out more about organizations such as ***#DoublePell*** that aim to expand the Pell Grant.

Others argue that the government should spend the fiscal budget on other issues instead.

Do you think the government should double the size of the Pell Grant?
[Slider from 0 "definitely should not" (0) to 10 "definitely should"]

Fee Waiver Question
[Note: Participants randomly receive information on either this question or the previous one]

One of the barriers to applying to college for low-income students is application fees. US colleges charge an average of $45 for each application and application fees can be as high as $90 for some colleges (e.g. Stanford). By clicking here, you can find out more about college application fees across the US.

Some argue that one way to address this issue is for colleges to provide automatic fee waivers to low-income students. When students apply to colleges, the application system detects their eligibility for an application fee waiver so low-income students can apply without any costs and without filling in any additional paperwork.

Others argue that these fees are required by colleges to cover the administrative costs of reviewing and evaluating applications, so everyone should pay for them.

Do you think colleges should automatically exempt low-income students from paying an application fee? [Slider from 0 "definitely should not" (0) to 10 "definitely should"]

Women quota question

[Note: All participants get the following placebo question]

Generally speaking, female workers earn less than male workers. Some people argue that to reduce earning differences between male and female workers, employers should make special efforts to hire and promote qualified women. Others argue there is no need to do so.

Do you think employers should make special efforts to hire and promote qualified women? [Slider from 0 "definitely should not" (0) to 10 "definitely should"]

Policy Effectiveness

[Note: Participants get the following question for the policy they were asked about and for the women quota question]

Previously, we asked you [explain policy here]. **If [policy description] were implemented, how do you think the following groups would be affected?** [5 options: Very negatively affected, negatively affected, unlikely to be affected, positively affected, very positively affected]

- White Americans
- Black Americans
- Women
- Men
- You and/or people you care about

If colleges automatically exempt low-income students from paying an application fee, how effective would it be in improving everyone's likelihood of attending college if they wish to?

- Not effective at all
- Slightly effective
- Moderately effective
- Very effective

- Extremely effective

F.4. Implicit Association Test

__Background information__

In this section, you will see items that represent the names of individuals with and without a **college degree** and some **positive or negative words**.

As each item appears, you will be asked to categorize the items to the left or right side of the screen using the 'E' (left side) and 'I' (right side) keys on your keyboard.

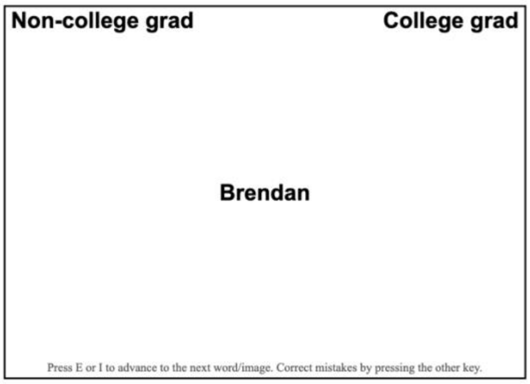
All of the following abbreviations indicate that someone has a college degree. If the item does not have any of the following abbreviations, you can assume that the individual does not have a college degree.

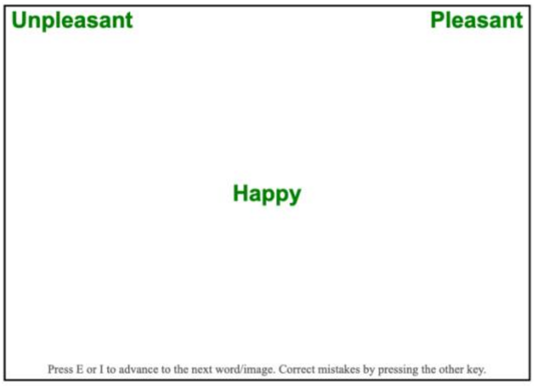
- BSc, J.D, MBA, MSc, M.D., PhD

Here are the positive and negative words you may see:

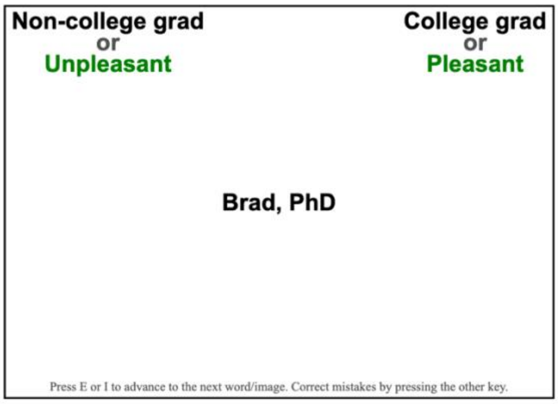
- **Positive** words: Gentle, Enjoy, Heaven, Cheer, Happy, Love, Friend
- **Negative** words: Poison, Evil, Gloom, Damage, Vomit, Ugly, Hurt

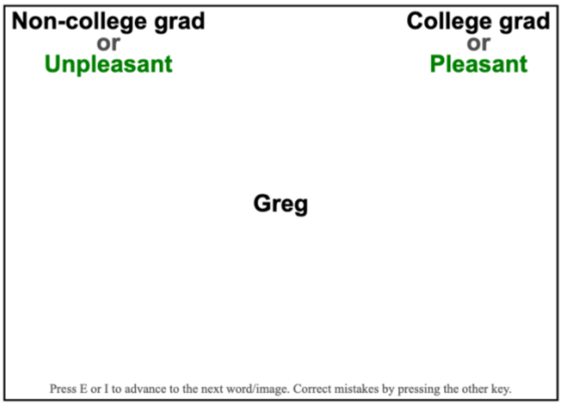
__Examples of practice blocks__



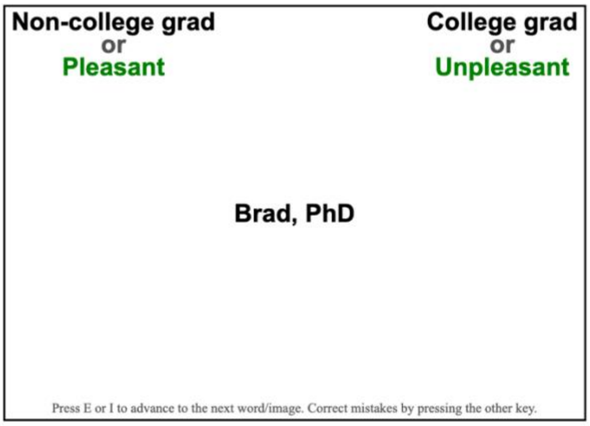


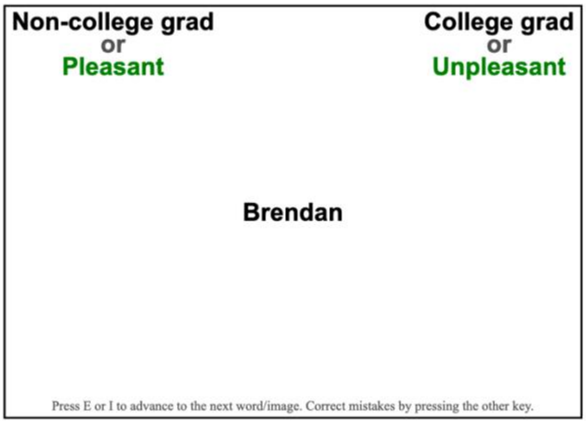
__Example of stereotypical block__





<u>Examples of non-stereotypical block</u>





F.5. Explicit Attitudes (Feelings Thermometer)

We would now like to get your feelings about some groups in society. For each of the following groups, use the slider to show how warm or cold you feel towards the group.

If you don’t feel particularly warm or cold toward a group, you would rate them at 50 degrees. If you feel warm toward the group, you would rate them between 50 to 100 degrees. If you feel cold toward a group, you would rate them between 0 and 50 degrees.

- Americans who identify as **black** [Slider from 0 (cold) to 100 (warm)]
- Americans who identify as **white** [Slider from 0 (cold) to 100 (warm)]
- Americans **without** a college degree [Slider from 0 (cold) to 100 (warm)]
- Americans **with** a college degree [Slider from 0 (cold) to 100 (warm)]
- Americans who identify as **female** [Slider from 0 (cold) to 100 (warm)]
- Americans who identify as **male** [Slider from 0 (cold) to 100 (warm)]

E. Other Variables

E.1. Beliefs about relative productivity

[Asked before the treatment]

Economists often measure **productivity** in terms of dollar output per hour. Think about a typical college-graduate and a typical non-college graduate, both of whom work full time. In total, they produce $100 of output per hour.

[Note: participants randomly receive one of the following questions]

Of this $100, how much output (in $) do you think the college graduate produced?
Of this $100, how much output (in $) do you think the non-college graduate produced?

E.2. Altruism

[Asked before the treatment]

How willing are you to give to good causes without expecting anything in return? [Slider from 0 (completely unwilling to do so) to 10 (very willing to do so)]

E.3. Locus of control

[Asked before the treatment]

To what extent do you agree with the following statements? [Scale from 1 (strongly disagree) to 7 (strongly agree)]

- I have little control over the things that happen to me
- There is really no way I can solve some of the problems I have
- There is little I can do to change many of the important things in my life
- I often feel helpless in dealing with the problems of life
- Sometimes I feel that I'm being pushed around in life
- What happens to me in the future mostly depends on me
- I can do just about anything I really set my mind to do

E.4. Numeracy

[Asked after the treatment]

- Imagine that we rolled a fair, 6-sided die 1000 times. Out of 1,000 rolls, how many times do you think the die would come up even (2,4, or 6)?
- In the BIG BUCKS LOTTERY, the chances of winning a $10.00 prize is 1%. What is your best guess about how many people would win a $10.00 prize if 1,000 people each buy a single ticket to BIG BUCKS?
- In the ACME PUBLISHING SWEEPSTAKES, the chance of winning a car is 1 in 1,000. What percent of tickets to ACME PUBLISHING SWEEPSTAKES win a car?

E.5. Beliefs about fixed and malleable factors

[Asked after the treatment]

In general, do you think people can change the following factors about themselves if they want to? [Slider from 0 (Can't change this factor) to 100 (Can change this factor)]

- Can people change how **hardworking** they are?
- Can people change how **ambitious** they are?
- Can people change whether they **know the right people**?
- Can people change whether they are **born in the right neighborhood**?
- Can people change how **smart** they are?
- Can people change their **race or ethnicity**?
- Can people influence whether they get a **high SAT score**?
- Can people influence whether they get a **high-paying job**?

E.6. Demographics

[Asked after the treatment]

- **How do you describe yourself? [**Male, Female, Non-binary/third gender, Prefer to self-describe]
- **In what year were you born? [**Dropdown from 1933 or earlier to 2004]
- **Which of the following do you most identify with?** [White/Caucasian, Black/African American, American Indian/Native American/Alaska Native, Asian/Asian American, Spanish/Hispanic/Latino, Native Hawaiian/Other Pacific Islander, Other]
- **How many children do you have?** [0 to 5 or more]
- **What was your total household income before taxes during the past 12 months? [**Less than $25,000, $25,000 to $44,999, $45,000 to $64,999, $65,000 to $84,999, $85,000 to $99,999, $100,000 or more, Prefer not to say]
- **What is your current employment status? [**Full-time employee, Part-time employee, Self-employed or small business owner, Unemployed and looking for work, Student, Not in labor force (e.g retired/full-time parent)]
- **Which category best describes your highest level of education? [**Some high school or less, High school diploma or GED, Some college, but no degree, 2-year college degree, 4-year college degree, Master's degree, Graduate or professional degree (e.g. JD, MD, MBA), Doctoral degree (PhD)]
- **Which category best describes your <u>father's</u> highest level of education? [**Some high school or less, High school diploma or GED, Some college, but no degree, 2-year college degree, 4-year college degree, Master's degree, Graduate or professional degree (e.g. JD, MD, MBA), Doctoral degree (PhD)]
- **Which category best describes your <u>mother's</u> highest level of education? [**Some high school or less, High school diploma or GED, Some college, but no degree, 2-year

college degree, 4-year college degree, Master's degree, Graduate or professional degree (e.g. JD, MD, MBA), Doctoral degree (PhD)]

- **When you were growing up,** compared with American families back then, would you say your family income was:[Far below average, Below average, Average, Above average, Far above average]
- **Right now,** compared with American families, would you say your own household income is: [Far below average, Below average, Average, Above average, Far above average]
- **Generally speaking, do you usually think of yourself as a Republican, Democrat, or Independent?** [Democrat, Independent, Republican, Other party]
- **In which state do you currently reside?** [Dropdown menu of US states]
- **How strong would you say your attachment is to each of the following identities?** [Not strong at all, Slightly strong, Somewhat strong, Very strong]
    - Identity based on my **nationality**
    - Identity based on my **race or ethnicity**
    - Identity based on my **educational qualifications**
    - Identity based on my **occupation**
    - Identity based on my **gender**

E.7. Questions about the study

[Asked after the treatment]

Do you feel that this survey was biased?

- Very left-wing biased
- Somewhat left-wing biased
- Neither left-wing or right-wing biased
- Somewhat right-wing biased
- Very right-wing biased

G. Survey questionnaire: Follow-up study

A. Start of survey

A.1. Survey information

We are a non-partisan group of academic researchers from the University of Cambridge. Our goal in this survey is to understand your views on various policies. No matter what your political views are, you are contributing to our knowledge as a society.

This study will take you around 10 minutes. To take part, you must ordinarily be a resident in the US and be at least 25 years old. If you do not fulfil these requirements, please do not continue any further.

Please complete this study on a computer or laptop, not a tablet or phone. This study requires you to look at some images and they may not appear clearly on a small screen. Please ensure you read each question carefully and answer honestly.

Your participation in this study is purely voluntary. Your name will never be recorded. Results may include summary data, but you will never be identified.

To proceed, please tick the box that applies to you

- No, I would not like to participate
- Yes, I would like to participate and confirm that **I live in the US** and am **25 years old or older**

A.2. Attention Check

The next question is about the following problem. In questionnaires like ours, sometimes there are subjects who do not carefully read the questions and just quickly click through the survey. This means that there are a lot of random answers which compromise the results of research studies. To show that you read our questions carefully, please choose “Not at all interested” and “Extremely interested” as your answer in the next question.

- Not at all interested
- Slightly interested
- Moderately interested
- Very interested
- Extremely interested

B. Demographics

- We hear a lot of talk these days about liberals and conservatives. Where would you place yourself on this scale? [1=Extremely liberal; 7=Extremely conservative]
- Please indicate your marital status [Single/ Married/ Other]
- Were you born in the US? [No/ Yes]
- Were both of your parents born in the US? [No/ Yes]

- If you compare your job (or your last job if you currently don't have a job) with the job your <u>father</u> had while you were growing up, would you say that the level of status of your job is: [Much higher than my father's, Higher than my father's, About equal to my father's, Lower than my father's, Much lower than my father's, My father did not have a job while I was growing up/My father was not present]
- If you compare your job (or your last job if you currently don't have a job) with the job your <u>mother</u> had while you were growing up, would you say that the level of status of your job is: [Much higher than my mother's, Higher than my mother's, About equal to my mother's, Lower than my mother's, Much lower than my mother's, My mother did not have a job while I was growing up/My mother was not present]

C. Beliefs about the sources of educational inequality

Think about two groups of people. Everyone in Group 1 attended college. Everyone in Group 2 did **not** attend college.

How important are the following factors in explaining this difference in college attendance between groups 1 and 2?

- Fixed factors: Factors that are *fixed* at birth (e.g. whether they are born into a rich or poor household)
- Malleable factors: Factors that are *not fixed* at birth (e.g. their mindset towards hard work)

Please use the slider below to indicate how important you think each factor is.

[Slider with 3 labels ("differences in malleable factors more important" (left), "differences in malleable and fixed factors equally important" (center), "differences in fixed factors more important" (right))

D. Outcomes

D.1. Income Allocation

Generally speaking, college graduates earn more than non-college graduates. According to recent data, before tax, the typical college graduate earns over $62,000 per year while the typical non-college graduate earns below $36,000 per year.

Some people argue that we should use the tax system to reduce differences in earnings between college and non-college graduates. Others argue that there is no need to reduce differences in earnings between college and non-college graduates.

Do you think that the difference in earnings between the typical college graduate and non-college graduate should decrease, stay the same, or increase? [Should decrease, should stay the same, should increase]

D.2. Donation

By taking this survey, you are automatically enrolled in a lottery to win $100.

If you win the lottery, do you want to donate some of this money to the National College Attainment Network?

The National College Attainment Network is a charity that aims to increase access to college, especially among communities underrepresented in postsecondary education. The NCAN does this by helping students prepare for and apply to college.

If you win the lottery, we will contact you in a few days to let you know. You will be paid this extra money (minus your donations) in addition to your payment for participating in the survey.

Please enter how much you would like to donate to the charity:___

D.3. Policy Support

Education-related policy
[Participants receive the policy question that they *not* get in the main survey (Pell Grant question or application fee question)]

D.4. Implicit Association Test

[Same setup as in main survey except that (1) the names of the primes are different, (2) the positive/negative words are different, (3) the color scheme and font are different]

D.5. Explicit Attitudes

In each row below, how would you split 100 "trust points" between the two individuals displayed on either end of the slider?

The closer you drag the slider to one individual, the more you trust that individual, relative to the other individual.

- A randomly-selected **college graduate** who lives in the U.S. and a randomly-selected **non-college graduate** who lives in the U.S.
- A randomly-selected **white person** who lives in the US and a randomly-selected **black person** who lives in the U.S.
- A randomly-selected **man** who lives in the US and a randomly-selected **woman** who lives in the U.S.