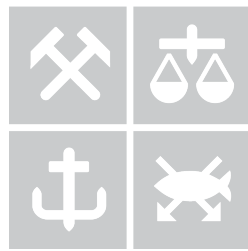


ESSAYS ON BELIEFS AND POLITICAL BEHAVIOR

NHH



INGAR K. HAALAND

DEPARTMENT OF ECONOMICS

NHH Norwegian School of Economics

A thesis submitted for the degree of

Philosophiae Doctor (PhD)

BERGEN 2019

Acknowledgements

I would first like to express my deepest gratitude to my main supervisor, Alexander W. Cappelen, for your continued support and invaluable guidance over the years. You have always been extremely generous with your time and eager to discuss my many ideas throughout the years. Importantly, you have been sufficiently critical to shoot down the less promising ideas while also being encouraging and constructive enough to make the more promising ideas much better.

I am also extremely grateful to Bertil Tungodden—you have always been extremely supportive and eager to provide invaluable advice and guidance. I would like to extend a special thanks to Chris Roth, who is not only an exceptional co-author but also a very good friend. We wrote the first two chapters of this thesis together. Alexander and Bertil also deserve a second special thanks for our joint work on the last chapter of this thesis.

Furthermore, I would like to thank my external supervisor, Björn Bartling, who also have provided very generous guidance and support. The faculty at the Department of Economics have been very supportive and made NHH a great place to pursue a PhD. Erik Sørensen in particular has been very generous in providing advice and feedback; thank you. I also want to thank Ingvild Almås, Mathias Ekstöm, and Frode Steen, all of whom have been very supportive over the years. I also extend a big thanks to the fellow students in my PhD cohort: Oddmund, Ranveig, Tim, and Xu. Furthermore, I would

like to thank the broader The Choice Lab family for creating a great and social research environment for behavioral economics; I have particularly benefited from interactions with Charlotte, Hallgeir, Ingrid, Ranveig, and Thomas. Furthermore, I want to extend a special thanks to Andreas, Erling, Jonas, Ole-Andreas, and Tom for being good colleagues as well as good friends. I am also very grateful to the administrative staff at the Department of Economics and The Choice Lab/FAIR, who always have been supportive and eager to help when needed.

I have been very fortunate to travel a lot during my PhD. Spending one year at University of California, Berkeley was one of the highlights of the PhD; thanks to Shachar Kariv who generously hosted me. I would also like to thank all my supportive friends at I-House who really made the visit extremely memorable. Furthermore, I have been very fortunate to attend some truly world-class workshops in behavioral economics. I want to thank David Laibson and Matthew Rabin for organizing the RSF School Summer School, Armin Falk for organizing the briq Summer School, and Uri Gneezy for co-organizing the Spring School.

I would also like to extend big thanks to my very supportive friends and family. Thomas deserves a special mention; we learned a lot about behavioral economics writing our master thesis together. Lastly, I would like to express my most special thanks to Marte—for everything.

Contents

Introduction	1
1 Beliefs about Racial Discrimination and Support for Pro-Black Policies	6
1.1 Introduction	7
1.2 Experimental design and samples	13
1.3 Beliefs about racial discrimination: Descriptives	26
1.4 Treatment effects on beliefs and policy views	33
1.5 Exploring drivers of polarization in policy views	46
1.6 Concluding remarks	50
Appendices	52
1.A Appendix tables	53
1.B Appendix figures	72
1.C Screenshots	78
2 Labor Market Concerns and Support for Immigration	80
2.1 Introduction	81
2.2 Experimental design and sample	85
2.3 Results	93
2.4 Concluding remarks	109

Appendices	111
2.A Additional figures and tables	113
2.B Deviations from the pre-analysis plan	136
2.C Screenshots	138
3 Beliefs about Behavioral Responses to Taxation	140
3.1 Introduction	141
3.2 Experiment 1: Design and participants	145
3.3 Theory	152
3.4 Empirical strategy: Experiment 1	154
3.5 Results: Experiment 1	157
3.6 Experiment 2: Introducing a political context	168
3.7 Demand for redistribution: The role of beliefs and preferences	175
3.8 Conclusion	178
Appendices	180
3.A Additional figures and tables	181
3.B Main tables with full sample	195
3.C Pre-specified tables	201
References	207

INTRODUCTION

There is a growing concern in Western countries that misinformation and ‘echo chambers’ are increasing political polarization by manipulating voters into taking more extreme partisan views (Sunstein, 2018). This concern is particularly pronounced in the United States, where political polarization is said to be “the defining feature of 21st century American politics” and voters are more divided in their policy preferences than at any point in recent history (Doherty, 2014). Interventions that can reduce political polarization are therefore of special interest to governments in heavily polarized societies, such as the United States.

Economists have traditionally not contributed much to the debate on how to reduce political polarization. The view in economics has been that people on average have unbiased beliefs and that differences in policy views are due to differences in preferences (Bray and Kreps, 1987; Meltzer and Richard, 1981). According to this view, there is limited scope for policy makers and others to influence public opinion as preferences are typically assumed to be stable. More recent research in behavioral economics, however, has relaxed the assumption that people always have unbiased beliefs (Della Vigna, 2009). This raises the question of whether people to some extent hold different policy views because they have systematically different beliefs about policy-relevant issues. If the answer to this question is “yes,” it leaves scope for “polarization entrepreneurs” to reduce—or increase—political polarization

by providing people with new information to change their beliefs about policy-relevant issues.

This thesis uses incentivized economic experiments to study the role of beliefs in driving people's policy preferences. Most previous research has relied on traditional opinion surveys to study this question, but there are two main problems with the survey approach. The first problem is about measurement. Survey questions are typically elicited using ordinal response scales. This makes it difficult to compare responses between groups as people could hold systematically different views on the difference between, say, "a lot of discrimination" and "only some discrimination." If these differences are correlated with background characteristics, such as people's political affiliation, the responses cannot be used to identify group differences in beliefs and policy preferences. Survey questions are typically also non-incentivized, making it difficult to know how informative they are of people's actual beliefs and political behavior. The second problem is about causal identification. To identify the impact of beliefs on people's policy preferences, most previous research has examined correlations between people's stated beliefs and policy preferences. But these correlations cannot be given a causal interpretation as they also could reflect omitted variable bias or reverse causality.

This thesis contains three chapters that use novel methods to study the causal impact of people's beliefs on their political behavior. It examines the following questions:

Chapter 1 Is the political disagreement on support for pro-black policies driven by differences in beliefs about racial discrimination?

Chapter 2 Do beliefs about the labor market impact of immigrants drive people's support for immigration?

Chapter 3 Do voters have conflicting views on redistribution because they differ in their beliefs about the incentive cost of taxation?

While the chapters ask different questions, some methodological features are common to all of them. First, they rely on experimental data collected by me and my co-authors using large, representative samples of the US population. The chapters employ novel behavioral measures of people's political behavior, such as signatures on real online petitions and donations to NGOs, and novel quantitative and incentivized measures of people's beliefs. Furthermore, to address issues of causality, two of the chapters introduce exogenous variation in people's beliefs through randomized information provision. Second, to follow best practices on efforts to increase research transparency and reproducibility of economic research (Christensen and Miguel, 2018; Miguel et al., 2014), we submitted pre-analysis plans to the AEA RCT Registry for all experiments in this thesis. The experiments are also high-powered: in total, this thesis reports results from experiments containing more than 23,000 unique respondents. Third, the experiments were designed to minimize concerns about experimenter demand effects (de Quidt et al., 2018; Zizzo, 2010), e.g., by employing novel obfuscated follow-up studies.

Chapter 1: Beliefs about Racial Discrimination and Support for Pro-Black Policies This chapter, written with Christopher Roth, examines the role of people's beliefs about racial discrimination in shaping their support for pro-black policies. The chapter introduces a new approach to measure be-

beliefs about discrimination by leveraging correspondence studies to measure beliefs. In contrast to traditional survey questions, this approach allows us to elicit quantitative and incentivized beliefs about racial discrimination in a precisely defined environment. Furthermore, the approach allows us to shift people's beliefs about racial discrimination by giving them information about the results from an actual correspondence study testing for racial discrimination in the labor market. The chapter also employs a behavioral outcome measure, namely donations to a pro-black civil rights organization, to assess treatment effects on people's actual political behavior.

The chapter documents pronounced differences in beliefs between Republicans and Democrats. Furthermore, Republicans and Democrats strongly update their beliefs about the extent of racial discrimination in response to information based on research evidence. However, the information fails to narrow Republican–Democrat differences in support for pro-black policies. Overall, the results demonstrate that correcting biases in beliefs about the extent of racial discrimination is not sufficient to reduce political polarization in support for pro-black policies.

Chapter 2: Labor Market Concerns and Support for Immigration This chapter, written with Christopher Roth, examines the role beliefs about the labor market impact of immigrants play in driving people's support for immigration. The chapter proposes a novel way to measure and change the respondents' beliefs about the labor market impact of immigration by assigning some respondents to research information showing no adverse labor market impacts of immigration. The chapter also employs a behavioral outcome measure, namely signatures on real online petitions, to assess whether changes in beliefs affect people's real political behavior. Finally, to address

concerns about experimenter demand effects, it employs a novel obfuscated follow-up study that hides the connection between the main study and the follow-up one week later from respondents.

The chapter establishes that people are willing to update their beliefs about the labor market impact of immigration and, as a result, display higher support for immigration, as measured by self-reported attitudes and signatures on the online petitions. In contrast to the current consensus in the literature, the causal estimates demonstrate that labor market concerns are an important determinant of people's support for immigration.

Chapter 3: Beliefs about Behavioral Responses to Taxation This chapter, written with Alexander W. Cappelen and Bertil Tungodden, examines how beliefs about behavioral responses to taxation and preferences over equality–efficiency trade-offs relate to the political disagreement on redistribution. The chapters employs a novel task to elicit quantitative and incentivized beliefs from a sample of 13,900 Democrats and Republicans about how taxes affect people's effort choices. It also employs an incentivized and quantitative task to elicit people's equality-efficiency preferences.

The chapter documents that Democrats and Republicans have virtually identical beliefs about behavioral responses to taxation. Furthermore, it finds that beliefs about behavioral responses to taxation fail to predict people's support for equalization of incomes in society. Equality–efficiency preferences, by contrast, strongly predict both people's political affiliation and their support for equalization of incomes in society. The findings suggest that the political divide on redistribution relates more to people's preferences than to their beliefs about the behavioral responses to taxation.

Chapter 1

Beliefs about Racial

Discrimination and Support for

Pro-Black Policies

Ingar K. Haaland and Christopher Roth*

Abstract

We provide nationally representative evidence of people's beliefs about racial discrimination in the US and explore whether these beliefs causally affect support for pro-black policies. In an online experiment on a large, representative sample of Americans, we elicited incentivized beliefs about the extent of racial labor market discrimination against blacks. 55 percent of Americans

*Haaland: Department of Economics, NHH Norwegian School of Economics; Roth: Institute on Behavior & Inequality. We would like to thank Roland Bénabou, Björn Bartling, Alexander W. Cappelen, James Druckman, Stefano DellaVigna, Jon de Quidt, Armin Falk, Eleonora Freddi, Jeremy Freese, Thomas Graeber, Johannes Haushofer, Lukas Hensel, Johannes Hermle, Simon Jäger, Fabian Kosse, Matt Lowe, Simon Quinn, Gautam Rao, Eirik Strømmland, Erik Sørensen, Bertil Tungodden, Jonas Tungodden, Justin Valasek, and seminar participants in Bergen, Bonn, Cologne (EEA), Copenhagen, Oslo, Oxford, Mannheim, and Munich for helpful comments and discussions. Financial support from the Centre for Ethics and Economics at NHH, the Russell Sage Foundation (Small Awards in Behavioral Economics), the Research Council of Norway through its Centre of Excellence Scheme (FAIR project No 262675), and data collection by Time-sharing Experiments for the Social Sciences (NSF Grant 0818839, Jeremy Freese and James Druckman, Principal Investigators) is gratefully acknowledged. The experiment is registered in the AEA RCT Registry as trial 2273. IRB approvals were obtained from the University of Oxford and the NHH Norwegian School of Economics. The usual disclaimer applies.

overestimate the extent of discrimination against blacks, and Republicans are 19 percentage points less likely than Democrats to overestimate the extent of discrimination against blacks. To introduce exogenous variation in beliefs, we provided a random subset of our respondents with research evidence from a correspondence study that tested for discrimination against blacks in the labor market. Respondents strongly and persistently update their beliefs about racial discrimination in response to the information. Treated respondents who underestimate racial discrimination increase their donations to a pro-black civil rights organization by 17 percent of a standard deviation. This effect is entirely driven by non-Republicans, which means that the treatment fails to narrow Democrat–Republican differences in donations. In contrast to the donations, self-reported attitudes towards pro-black policies are generally unresponsive to new information. Our findings demonstrate that correcting people’s biases in beliefs about the extent of racial discrimination is not sufficient to reduce political polarization in support for pro-black policies. (*JEL* C91, D83, F22, J15)

1.1 Introduction

Racial discrimination is a pervasive phenomenon that affects many spheres of society (Arrow, 1998; Bertrand and Duflo, 2017; List, 2004). In the United States, several studies have documented high levels of racial discrimination in various domains, such as the labor market (Bertrand and Mullainathan, 2004; Fryer et al., 2013; Neumark et al., 1996; Nunley et al., 2015; Oreopoulos, 2011), the housing market (Bartoš et al., 2016; Edelman et al., 2017), sports (Price and Wolfers, 2010), and the judicial system (Abrams et al., 2012; Alesina and La Ferrara, 2014).

To deal with this large degree of racial discrimination, the US government has introduced policies aiming to actively counteract the effects of racial discrimination. However, Americans are deeply divided in their support for such policies. For instance, while 73 percent of Democrats support affirmative action programs for racial minorities, only 38 percent of Republicans support this.² There is a strong perception in the public debate that this political disagreement is rooted in differences in perceptions of the extent of racial discrimination in society (Newkirk, 2017). Furthermore, in a seminal article on the drivers of opposition to pro-black policies, Bobo and Kluegel (1993) argue that it is necessary to correct people's biases in beliefs to gain support for pro-black policies.

This chapter provides the first causal evidence of the relationship between people's beliefs about racial discrimination against blacks and their support for pro-black policies. Specifically, we address the following two questions using incentivized data on people's beliefs and support for pro-black policies: First, do Republicans and Democrats hold different beliefs about the extent of racial discrimination in society? Second, would a convergence in beliefs about the extent of racial discrimination in society reduce the differences in support for pro-black policies between Republicans and Democrats?

We introduce a new approach to elicit quantitative and incentivized beliefs about racial discrimination. With respondents from a high-quality, probability-based sample of the US household population, we elicited incentivized beliefs about the results of a correspondence study testing for racial discrimination against blacks in the labor market (Bertrand and Mullainathan, 2004).³ Respondents were told that researchers sent out resumes

²news.gallup.com/poll/184772/higher-support-gender-affirmative-action-race.aspx (accessed November 30, 2018).

³While the correspondence study by Bertrand and Mullainathan (2004) was conducted

that were identical in all respects except for the perceived race of the sender to help wanted ads in Boston and Chicago newspapers. After informing the respondents that resumes with white-sounding names had to be sent out ten times to get one callback on average, we asked them how many times they thought that resumes with black-sounding names had to be sent out to get one callback on average. In contrast to traditional survey questions, which typically ask about “how much discrimination is there” on a scale from “a lot” to “none at all,” this approach allows us to elicit quantitative and incentivized beliefs about racial discrimination in a precisely defined environment.

To examine whether beliefs about racial discrimination causally affect people’s support for policies aiming to counteract the effects of racial discrimination, we introduced exogenous variation in people’s beliefs by informing a random subset of the respondents about the actual results from the correspondence study by Bertrand and Mullainathan (2004), namely that white-sounding names received 50 percent more callbacks for interviews than black-sounding names. To measure whether people update their beliefs about racial discrimination in response to this evidence, we elicited their beliefs about a second correspondence study that tested for racial discrimination in the housing market (Edelman et al., 2017). Furthermore, to measure whether the information provision affects people’s political behavior, respondents decided whether to receive money versus making a real donation to a pro-black civil rights organization. Finally, respondents answered a series of questions on self-reported views on pro-black policies.

We document several novel findings on beliefs about racial discrimination

in 2001 and 2002, a recent meta-analysis of field experiment on racial labor discrimination in the US shows no change in racial discrimination over time (Quillian et al., 2017).

and support for pro-black policies in America. Our first finding is that 55 percent of Americans overestimate the extent of racial discrimination against blacks. Beliefs vary systematically by people's self-identified party affiliation: Republicans are about 19 percentage points less likely than Democrats to overestimate racial discrimination in the labor market. Republicans are thus more accurate in their beliefs about racial discrimination than Democrats are. While Republicans on average overestimate the extent of racial discrimination by 16 percent (i.e., how many resumes with black-sounding names had to be sent out to get one callback on average), Democrats overestimate the extent of discrimination by 71 percent. Second, eliciting incentivized beliefs about the results from a second correspondence study in the housing market, we document that people's beliefs about racial discrimination respond strongly to the research evidence. Treated Republicans and Democrats hold virtually identical beliefs about racial discrimination. Third, we find that beliefs about racial discrimination causally affect people's political behavior: Respondents who underestimate the extent of racial discrimination increase their donations by 17 percent of a standard deviation. This effect size corresponds to almost one-third of the Democrat–Republican difference in donations. However, since the increase in donations among those who underestimate discrimination is entirely driven by non-Republicans, the treatment fails to narrow the Democrat–Republican difference in donations. Furthermore, examining treatment responses on self-reported attitudes towards pro-black policies, we find that these are generally unresponsive to new information. Overall, these findings demonstrate that correcting people's biases in beliefs about the extent of racial discrimination is not sufficient to reduce political polarization in support for pro-black policies.

To address concerns about social desirability bias, we conducted an addi-

tional experiment where the main outcome questions on self-reported policy views were only asked one week later in an obfuscated follow-up study hiding the connection between the treatment provision and the main outcome questions. We find evidence of strong and persistent belief updating about the extent of racial labor market discrimination in response to the information. The treatment completely eliminates the gap in beliefs between Democrats and Republicans. Furthermore, the results from the obfuscated follow-up study support our finding from the first experiment that self-reported attitudes towards pro-black policies are generally unresponsive to changes in beliefs about racial discrimination. The only exception compared to Experiment 1 is that we find some evidence of backfiring for Republicans; that is, treated Republicans who underestimate racial discrimination display even less support for pro-black policies.

We also ran two additional experiments to shed light on the role of two further potential determinants of support for pro-black policies. Our first additional experiment was motivated by strong correlational evidence which suggests an important role of beliefs about differences in work ethic between blacks and whites for explaining views on pro-black policies. In this experiment, we provided our respondents with information challenging the stereotype that blacks have a worse work ethic than whites (Gilens, 2009). Our experiment reveals that people who receive information about racial differences in work ethic do not adjust their views on pro-black policies. Finally, after establishing that information about racial discrimination or about racial stereotypes regarding work ethic does not affect self-reported policy views, our last experiment sheds light on a different prominently discussed causal determinant of policy views, namely political identity (Bursztyjn et al., 2016). We show that making party views on pro-black policies more salient does

not increase Democrat–Republican differences in self-reported policy views, suggesting that political identity is not the main driver of people’s views on pro-black policies. Overall, these two additional experiments corroborate our previous finding that self-reported attitudes towards pro-black policies are generally hard to move, suggesting that these may have an important “cultural” component that is very stable over time (Luttmer and Singhal, 2011).

Our main contributions are as follows: We collect the first incentivized measures of support for pro-black policies along with quantitative and incentivized data on people’s beliefs about racial discrimination in the labor market and in the housing market.⁴ We introduce a new approach for measuring incentivized beliefs about discrimination by leveraging correspondence studies, which provide a useful tool to elicit well-defined and incentivized beliefs. In contrast to traditional survey questions, our approach allows us to obtain a quantitative measure of people’s beliefs about racial discrimination that is incentivized and easily comparable across respondents. Since incentives have been shown to reduce partisan bias in people’s stated beliefs (Bullock et al., 2015; Prior et al., 2015), an incentivized belief elicitation is particularly important for highly contested issues such as racial discrimination. Second, we provide the first causal evidence of the role of people’s beliefs about racial discrimination on their demand for policies that try to counteract the effects of this discrimination.⁵ We thereby inform the debate on the determinants of support for pro-black policies (Bobo and Kluegel,

⁴Our study is related to concurrent work by Kraus et al. (2017) who measure people’s beliefs about racial income inequality in the US.

⁵More generally, we add to the broader literature on how information provision affects people’s policy preferences (Alesina et al., 2018b; Cruces et al., 2013; Gilens, 2001; Grigorieff et al., 2018; Haaland and Roth, 2019; Karadja et al., 2017; Kuklinski et al., 2000; Kuziemko et al., 2015). This is also related to models of belief updating in response to information that conflicts with people’s prior beliefs (Fryer et al., 2018).

1993; Harrison et al., 2006; Jacobson, 1985; Kluegel and Smith, 1983; Kuklinski et al., 1997; Tuch and Hughes, 2011). More generally, by exploring how beliefs about racial discrimination affect people's political behavior, our results contribute to the literature on the relevance of race for US politics (DellaVigna, 2010; Kuziemko and Washington, 2018; Stephens-Davidowitz, 2014). Moreover, our results complement previous work examining whether the awareness of racial discrimination reduces racial bias in the NBA (Pope et al., 2018).

The remainder of the chapter proceeds as follows. Section 1.2 describes the experimental design and samples. Section 1.3 provides descriptive data on people's beliefs about racial discrimination. Section 1.4 presents treatment effects of the provision of research evidence about the extent of racial discrimination against blacks on beliefs and policy views. Section 1.5 presents results from two experiments that explore the roles of beliefs about differences in the work ethic between blacks and whites as well as political identity in driving political differences in views on pro-black policies. Section 1.6 concludes. The appendix provides additional results. The full set of experimental instructions are available from the pre-analysis plans.

1.2 Experimental design and samples

We conducted two complementary online experiments with different samples. In Experiment 1, we collected data on a probability-based sample of the US population in collaboration with NORC at the University of Chicago. In Experiment 2, we collected data on a US sample representative in terms of several observables, collaborating with Research Now, a US market research company.

Figure 1.1: Experiment 1 (NORC sample)

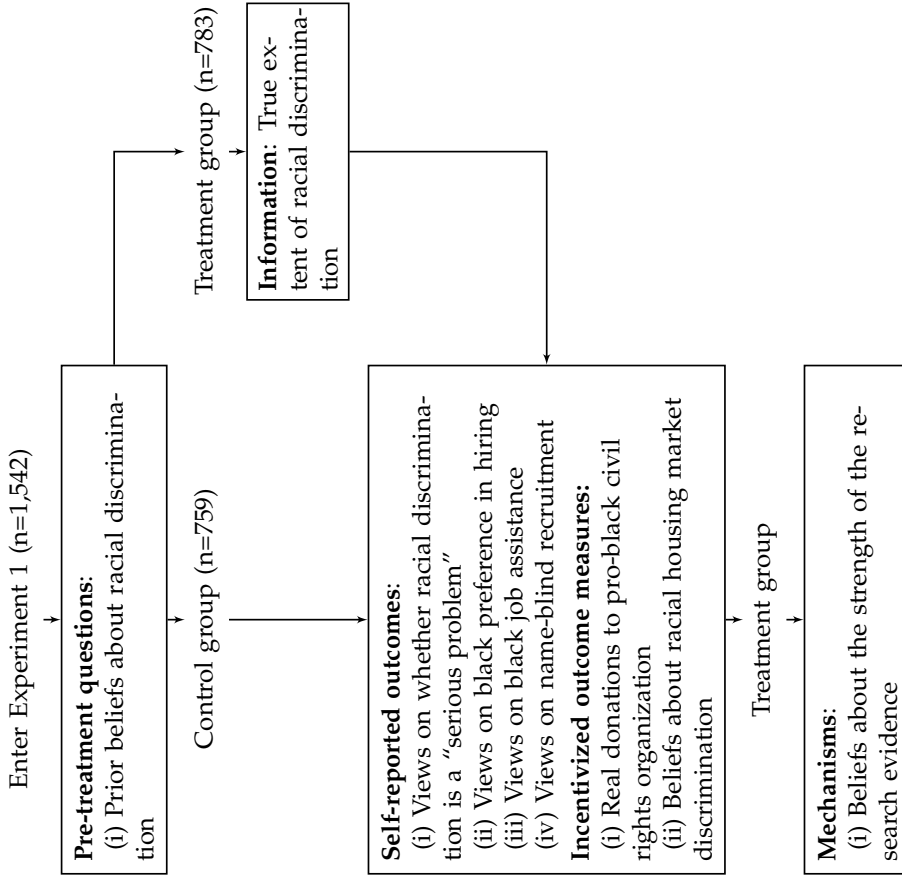
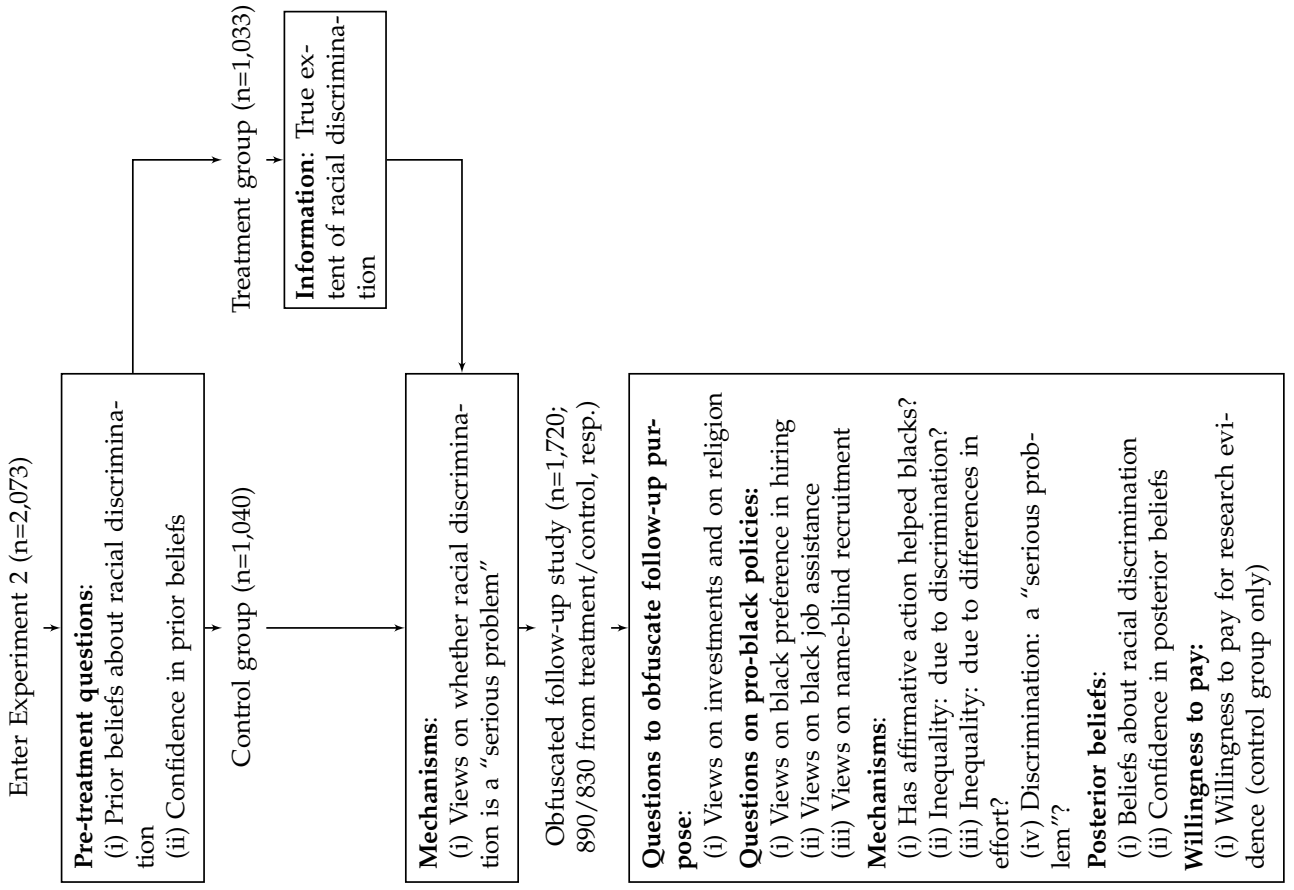


Figure 1.2: Experiment 2 (Research Now sample)



1.2.1 Experiment 1: Design

The structure of Experiment 1 is as follows (Figure 1.1 provides an overview). We first measured our respondents' beliefs about the extent of racial labor market discrimination in the US. We then exposed half of our respondents to the information treatment. Subsequently, we measured people's support for policies to address racial discrimination in the labor market using both self-reports and a behavioral measure. We also elicited post-treatment beliefs about racial discrimination in the housing market.

Pre-treatment beliefs about racial labor market discrimination

We used a correspondence study to measure people's beliefs about racial discrimination in the labor market. Correspondence studies rely on fictitious resumes to study discrimination in the labor market (Bertrand and Duflo, 2017). Specifically, by manipulating whether a fictitious resume is assigned a minority name, researchers can study racial labor market discrimination by comparing the outcomes for resumes with and without the perceived minority name. A seminal correspondence study by Bertrand and Mullainathan (2004) found that white-sounding names were 50 percent more likely to receive a callback than black-sounding names; a finding that has been closely replicated in several subsequent correspondence studies (Bertrand and Duflo, 2017; Quillian et al., 2017). We rely on this study in our experiment. To familiarize our respondents with the study, we presented them with the following text:

Researchers from Harvard University and the University of Chicago conducted an experiment to study racial discrimination in the labor

market. They did so by sending out fictitious resumes to help-wanted ads in Boston and Chicago newspapers.

The resumes were exactly the same except for one thing: the name of the job applicant. Half of the resumes had typically white-sounding names like “Carrie” and “Todd”. The other half of the resumes had typically black-sounding names like “Tanisha” and “Kareem”. The idea was to make sure that the applicants were seen as having identical qualifications, but that the employers would use the applicants’ names to infer whether they were white or black.

We then informed respondents that resumes with white-sounding names had to be sent out on average ten times to get one callback for an interview. To measure their beliefs about racial discrimination in the labor market, we then asked how many times they believe resumes with black-sounding names had to be sent out on average to get one callback for an interview. Furthermore, we promised respondents a \$2 bonus if their answer was the same “as what the researchers found.”

Our belief elicitation has several advantages compared to qualitative survey questions that have traditionally been used to study beliefs about racial discrimination. First, we measure beliefs on a quantitative scale that is easily comparable across respondents and has the same interpretation for everyone. By contrast, many previous studies have assessed beliefs about racial discrimination using a question from the General Social Survey about the amount of discrimination that blacks face in “getting good jobs,” which is measured on a 4-point scale from “none at all” to “a lot.”⁶ One concern with

⁶Details about this variable are available at the following link: <https://gssdataexplorer.norc.umd.edu/variables/1244/vshow> (accessed November 30, 2018).

using subjective response scales to measure beliefs is that different people may have different opinions about what, e.g., “some” or “only a little” discrimination means.⁷ Furthermore, in our setting, racial discrimination is precisely defined and we can hold our respondents’ beliefs about the circumstances of racial discrimination constant. For qualitative survey questions, people may hold different beliefs about what constitutes “discrimination.” These beliefs may be correlated with demographics, which makes it difficult to draw strong conclusions on differences in beliefs about racial discrimination across demographic groups. Our measure avoids these confounds. Second, unincentivized survey questions are more prone to the misreporting of beliefs. Indeed, small incentives for correct answers have been shown to strongly increase the accuracy of survey responses and to reduce gaps in reported beliefs across party lines (Bullock et al., 2015; Prior et al., 2015). Since our question has a factual answer, we can incentivize correct responses.

Introducing exogenous variation in beliefs

Two central identification challenges when studying the impact of beliefs on policy preferences are omitted variable bias and reverse causality. We address these identification challenges by introducing exogenous variation in beliefs, namely by informing respondents in the treatment group about the extent of racial discrimination found in the study by Bertrand and Mullainathan (2004). Specifically, we showed the following text to treated respondents:

The researchers found that resumes with black-sounding names on average had to be sent out 15 times to get one callback for an

⁷For a discussion of problems associated with subjective response scales, see Bond and Lang (2018).

interview.

Since resumes with white-sounding names on average only had to be sent out 10 times to get one callback for an interview, this means that employers were 50 percent more likely to give callbacks to applicants with white-sounding names compared to applicants with black-sounding names.

By contrast, respondents in the control group did not receive any information and proceeded directly from the belief elicitation to the outcome questions.

Measuring support for pro-black policies: Behavioral measure

A common critique of self-reported survey questions is that they might not be reflective of real political behavior and that they are prone to experimenter demand effects. To address these concerns, we collected a novel behavioral outcome measure, namely real donations to a pro-black civil rights organization. We told our respondents that they have the opportunity to financially support a civil rights organization that works to reduce discrimination against blacks in the labor market. We elicited the respondents' marginal rate of substitution between money for themselves and money for the civil rights organization through a multiple price list. The respondents chose between donating \$5 to the civil rights organization and money for themselves in \$1-increments from \$0 to \$5. One of the six choices was randomly implemented.⁸

⁸The experiment involved no deception and we actually donated the relevant amount to the civil rights organization after the experiment.

Measuring support for pro-black policies: self-reported policy views

In addition to the behavioral measure, we also collected some data on people's self-reported policy views. Since our treatment was tailored to shift beliefs about racial discrimination in the labor market, we focused on labor market policies. We asked questions about three commonly-discussed policies attempting to counteract the effects of labor market discrimination. First, we asked respondents whether they "support or oppose government and private programs that give qualified black candidates preference over equally qualified white candidates in getting a job." Second, we asked respondents whether they "support or oppose government and private programs that give qualified black candidates assistance in getting a job." Third, we asked respondents whether they "support or oppose mandatory name-blind recruitment for hiring in public and private jobs." For all three questions, respondents reported their answer on a 5-point scale ranging from 1 (Strongly oppose) to 5 (Strongly support).

Measuring beliefs about racial discrimination in the housing market

To measure whether respondents updated their beliefs in response to the research evidence, we relied on a second correspondence study that tested for racial discrimination in the housing market (Edelman et al., 2017). We chose to focus on racial discrimination in a different domain out of a concern that demand effects, numerical anchoring, or a taste for consistency in survey responses could bias responses if we re-asked the question about discrimination in the labor market shortly after the information provision. The housing market is a good candidate for several reasons. First, racial discrimination in

the housing market holds strong economic importance. Second, the study by Edelman et al. (2017), which serves as our benchmark for incentivizing beliefs, used the same names as Bertrand and Mullainathan (2004). This allows us to easily explain the methodology to respondents and makes the results across domains more comparable. Specifically, we used the following text to familiarize our respondents with the second study:

Researchers from Harvard Business School conducted an experiment to study racial discrimination in the rental market by sending out reservation requests from invented accounts to hosts on Airbnb, a website for private rental accommodations. The requests were exactly the same except for one thing: the name of the person who sent the request. Half of the requests came from typically white-sounding names, while the other half came from typically black-sounding names. The idea was that the hosts would use the applicants' name to infer whether the reservation requests came from white or black requesters.

We then told them that the researchers found that white-sounding names were accepted 49 percent of the time. To measure their beliefs about racial discrimination in the housing market, we then asked what percent of the time they believe that black-sounding names were accepted. We offered a \$2 bonus for answers that fall within “2 percentage points of what the researchers found.”

We purposefully designed the second belief elicitation to avoid potential bias stemming from numerical anchoring by (i) using a different response scale than the first belief elicitation, and (ii) using a scale in which higher values implied less racial discrimination. Since higher values implied more

discrimination in the first belief elicitation, numerical anchoring would make finding evidence for belief updating in the expected direction less likely.

1.2.2 Experiment 2: Design

While an important question is whether treatment effects persist over time, a potential drawback of re-asking the main outcome questions in a follow-up study is that people's taste for consistency in their survey responses may bias treatment effects (Falk and Zimmermann, 2013). To avoid this confound, we conducted a separate experiment in which we only asked the main outcome questions in a follow-up study (Figure 1.2 provides a summary of the structure). Furthermore, to address concerns about social desirability bias, we obfuscated the purpose of the follow-up study.

Design of the first wave

We first elicited beliefs about racial discrimination in the same way as in Experiment 1. We also elicited confidence by asking respondents how sure they were on a scale of 1 (Very Unsure) to 5 (Very Sure) of their answer to the previous question.⁹ Finally, we asked respondents whether they think that racial discrimination against blacks "is a serious problem."

⁹We did not ask this question in Experiment 1 owing to budget constraints. The cost of adding questions to Experiment 1 was much higher than in Experiment 2 because it used a probability-based sample.

Design of the second wave

Approximately one week after the first wave, respondents were invited to participate in the second wave. We chose to have one week between the two waves to strike a balance between testing for persistence of treatment effects and minimizing attrition.

One general concern with information experiments is that the information provision could alter participants' perceptions about how the experimenter expects them to behave. Even though recent evidence suggests that demand effects are not quantitatively important (de Quidt et al., 2018; Mummolo and Peterson, 2018), we took several steps to obfuscate the purpose of the second wave. First, respondents received a generic invitation from the survey provider to participate in a five-minute survey which did not reveal that the two waves were connected (Figure 2.11 provides a screenshot of the invitation from wave 1).¹⁰ Second, we used different Qualtrics accounts for the two studies: in wave 1, the Qualtrics account was from the University of Oxford; in wave 2, the Qualtrics account was from the NHH Norwegian School of Economics. We also varied the layout of the survey between the waves. Third, we asked respondents several obfuscation questions about their views on investment and religion before asking our main outcome questions.

Following the obfuscation questions, we asked the same questions on self-reported policy views as in Experiment 1: support for (i) a preference for hiring qualified black candidates over equally qualified white candidates, (ii) assistance programs for blacks in getting a job; and (iii) name-blind

¹⁰The actual number of days between wave 1 and wave 2 varied between one and 19 days for all respondents, with an average of eight days.

recruitment. We also asked a series of questions to examine mechanisms. Possible mechanisms include the belief that affirmative action programs are ineffective in improving the lives or general opportunities of blacks, which could engender opposition to those initiatives. To examine whether the treatment affects beliefs about the effectiveness of affirmative action, we asked respondents whether they think that affirmative action programs over the last fifty years have “have helped blacks, hurt them, or had no effect one way or the other.” Some people may also oppose affirmative action because they think that differences in outcomes between blacks and whites are mainly due to differences in work ethics between blacks and whites. To explore whether the treatment affected beliefs about the source of inequality between blacks and whites, we asked the following two questions: (i) to what extent they think that differences in economic outcomes between blacks and whites are “primarily the result of racial discrimination against blacks,” and (ii) to what extent they think that differences in economic outcomes between blacks and whites are “primarily the result of whites working harder than blacks.”

Near the end of the survey, we elicited posterior beliefs about the extent of racial labor market discrimination using the same correspondence study as in the first wave. As in the first wave, we incentivized correct answers with a \$2 bonus. Since we use the same belief elicitation across the two waves, it is natural to assume that respondents realized that the two waves are connected at this point.

1.2.3 Sample characteristics

Experiment 1: NORC AmeriSpeak

For Experiment 1, we recruited 1538 respondents through NORC's AmeriSpeak panel.¹¹ AmeriSpeak is a probability-based panel of the US population. The panel uses NORC's National Frame, which is designed to provide at least 97 percent sample coverage of the US population. The NORC National Frame is used for several landmark studies in the US, including the General Social Survey (GSS), which is one of the most frequently-analyzed data sets in the social sciences.¹²

Table 1.5 provides summary statistics for this sample. 46 percent of respondents are male, 66 percent are Non-Hispanic white, and 11 percent are Non-Hispanic black. The median household income in our sample is \$55,270. 80 percent of our sample have at least some college education. The sample is also representative in terms of regions: 16 percent of our respondents come from the North-East, 29 percent from the Midwest, 33 percent from the South, while the remaining respondents are from the West. In terms of political affiliation, 24 percent of respondents self-identify as Republicans; 36 percent self-identify as Democrats; 26 percent self-identify as Independents; and the remaining 14 percent do not have any particular political affiliation.

Observations in the treatment and control group are balanced in terms of

¹¹NORC does not force their respondents to answer any questions on their surveys. For some questions we therefore have less than 1538 observations, e.g., only 1382 respondents gave an answer to the question on the number of times resumes with black-sounding names had to be sent. There are no significant differences between Republicans and Democrats or between blacks and whites in not responding to this question. Our main specification includes only respondents who completed the question on beliefs about racial discrimination.

¹²More information about the panel is available at the following web page: <https://amerispeak.norc.org/about-amerispeak/Pages/Panel-Design.aspx> (accessed November 30, 2018).

observables (Table 1.7).¹³

Experiment 2: Research Now

In Experiment 2, we, in collaboration with Research Now, one of the leading marketing research companies in the US, successfully recruited 2075 respondents for the first wave of the experiment. The first wave was the second component of a follow-up study from another experiment that we also conducted with Research Now.¹⁴ Out of these 2075 respondents, 1720 also completed the second wave.

Table 1.6 provides summary statistics for the Research Now sample. The sample is broadly representative of the US population in terms of several important observable characteristics: 50 percent of our respondents are male; 49 percent are non-Hispanic white; and 6 percent are Non-Hispanic black. The median household income in our sample is \$56,000. 83 percent of our sample have at least some college education. 23 percent of our respondents come from the North-East, 19 percent from the Midwest, 35 percent from the South, and the remaining 23 percent of respondents are from the West. In terms of political affiliation, 26 percent of respondents self-identify as Republicans, 38 percent of our respondents self-identify as Democrats, and the remaining respondents self-identify as Independents. There is balance across treatment arms (Tables 1.8 and 1.9). Treatment status is not correlated with completing the follow-up (Table 1.10).

¹³We did not ask any questions about demographics or political affiliation as part of the experiment. This data was appended by NORC.

¹⁴In the first wave, respondents also answered demographic questions, questions about their views on the role of the government, and questions about their views on immigration.

1.3 Beliefs about racial discrimination: Descriptives

This section uses data from Experiment 1 to provide representative evidence of people's beliefs about racial discrimination. We first explore heterogeneity in people's beliefs regarding the extent of racial discrimination in America and investigate whether these beliefs correlate with some key background characteristics. We then examine whether beliefs about racial discrimination correlate with people's policy preferences.

1.3.1 Heterogeneity in beliefs about racial discrimination

Figure 1.3 provides representative evidence of people's beliefs about racial discrimination in the labor and housing markets. Panel A shows the cumulative distribution function for beliefs about how many resumes with black-sounding names had to send out to get one callback on average (respondents were told that the corresponding number for white-sounding names was ten). This quantitative belief elicitation allows us to assess the fraction of respondents who overestimate and underestimate racial discrimination in society. Taking the results from Bertrand and Mullainathan (2004) as given, who found that resumes with black-sounding names needed to be sent out 15 times before receiving one callback on average, we find that 35 percent of our respondents underestimate racial discrimination in the labor market, 10.3 percent have correct beliefs, and the remaining 54.7 percent overestimate the extent of racial discrimination in the labor market.¹⁵

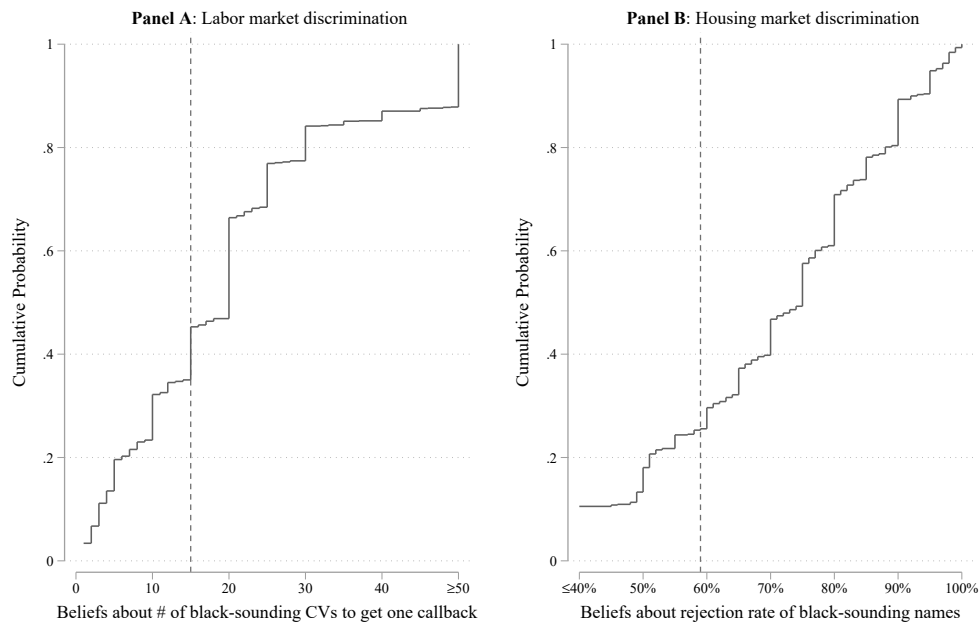
¹⁵A recent meta-analysis of field experiments on racial labor discrimination in the US shows no change in racial discrimination over time (Quillian et al., 2017).

Panel B of Figure 1.3 shows the cumulative distribution function for beliefs about the rejection rate of reservation requests from black-sounding names on Airbnb (respondents were told that the corresponding number for white-sounding names was 51 percent). Taking the results from Edelman et al. (2017) as given, who found that requests from black-sounding names were rejected 59 percent of the time, we find that 19 percent of our respondents underestimate racial discrimination in the housing market and the remaining 81 percent overestimate the extent of racial discrimination in the housing market.

The data also allows for the measurement of the share of respondents who think that there is discrimination against whites, discrimination against blacks, and the fraction who think that there is no racial discrimination at all. For the labor market, 23 percent of our respondents believe that there is discrimination against whites, nine percent believe that there is no discrimination, and the remaining 68 percent believe that there is discrimination against blacks. For the housing market, 12 percent think that there is discrimination against whites, two percent believe that there is no racial discrimination, and the remaining 86 percent think that there is discrimination against blacks. One reason for why a higher fraction of our respondents think that there is discrimination against blacks in the housing market might be that they think affirmative action programs in hiring make discrimination in the labor market less prevalent.

Figure 1.4 examines whether beliefs about racial discrimination vary systematically by people's background characteristics. Panel A shows correlations between background characteristics and beliefs about racial discrimination in the labor market. We find especially pronounced differences in beliefs

Figure 1.3: Beliefs about racial discrimination in the labor and housing market

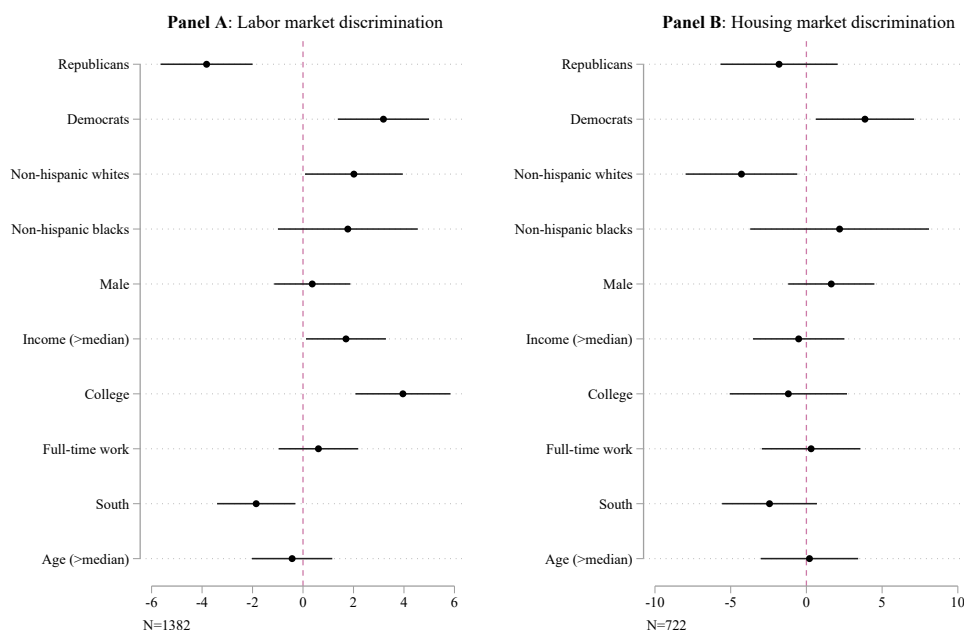


Notes: This figure uses data from Experiment 1 (the NORC sample). **Panel A** shows data on beliefs about how many times resumes with black-sounding names on average had to be sent out to get one callback for an interview. Respondents were informed that the corresponding number for resumes with white-sounding names was ten (as found in the study by Bertrand and Mullainathan, 2004). **Panel B**, using only control group respondents, shows data on beliefs about the rejection rate on reservation requests sent from accounts with black-sounding names. Respondents were initially asked about the percent rate of acceptances of reservation requests for black-sounding names on Airbnb (true rate is 41 percent, as found in the study by Edelman et al., 2017). They were told that the corresponding number for white-sounding names was 49. We have recoded the values to implied rejection rates by subtracting each estimate from 100. In both panels, the dashed lines indicate the correct answer.

based on people’s political affiliation: Relative to Republicans, Democrats believe that seven additional resumes with black-sounding names had to be sent out to get one callback on average ($p < 0.01$). Taking the results from Bertrand and Mullainathan (2004) as given, Republicans on average overestimate the extent of racial labor market discrimination by 16 percent, whereas Democrats overestimate the extent of discrimination by 71 percent. Beliefs about racial discrimination also correlate significantly with college education and income. Relative to those with no college education, college-educated respondents believe that four additional resumes with black-sounding names

had to be sent out to get to get one callback on average ($p < 0.01$). Relative to respondents with below median income, above-median income respondents believe that 1.7 additional resumes with black-sounding names had to be sent out to get one callback on average ($p < 0.05$). Surprisingly, we find no significant differences between blacks and whites in their beliefs about discrimination in the labor market ($p = 0.85$).¹⁶

Figure 1.4: Correlates of beliefs about racial discrimination



Notes: This figure uses data from Experiment 1 (the NORC sample). The dots indicate the mean values of the estimated multiple regression coefficients. The dependent variable in **Panel A** is people’s beliefs about the number times resumes with black-sounding names on average had to be sent out to get one callback for an interview. The dependent variable in **Panel B** is people’s beliefs about the percent of time reservation requests from black-sounding names on Airbnb were rejected. Lines indicate 95 percent confidence intervals.

Concerning beliefs about the housing market (Panel B of Figure 1.4), we also find pronounced differences based on people’s political affiliation: Relative to Republicans, Democrats think that reservation requests from black-sounding

¹⁶We also elicited willingness to pay for the research evidence through a multiple price list at the end of Experiment 2 for control group respondents. In the appendix, we show that whites, males and Republicans had a lower willingness to pay for the research evidence (Table 1.13).

names were 5.7 percentage points more likely to be rejected ($p < 0.01$). Taking the results from Edelman et al. (2017) as given, Republicans on average overestimate housing market discrimination by 14 percent, whereas Democrats overestimate housing market discrimination by 27 percent. While we do not find evidence of differences in beliefs in the housing market across people with different education levels, we find significant racial differences: Relative to whites, blacks think that reservation requests from black-sounding names were 6.5 percentage points more likely to be rejected ($p < 0.05$).

Given all of the findings discussed above, our first main result is as follows:

Result 1. *The majority of Americans overestimate racial discrimination against blacks in both the labor market and in the housing market. Furthermore, in both domains, we document that Democrats are more likely to overestimate the extent of racial discrimination than Republicans.*

1.3.2 The association between beliefs and policy preferences

Table 1.1 provides evidence of whether our measure of beliefs about racial labor discrimination correlates with some of our key outcome measures using only control group respondents. Column 1 of Panel A shows a regression of people's actual donations to the pro-black civil rights organization on their beliefs about racial discrimination in the labor market. A one standard deviation increase in beliefs is associated with 0.22 of a standard deviation higher donations to the pro-black civil rights organization ($p < 0.01$). This corresponds to 36 percent of the Democrat–Republican difference in donations to the pro-black civil rights organization. Including controls in the regression reduces the estimated association to 0.17 of a standard deviation ($p < 0.01$,

Column 1 of Panel B).

Columns 2 and 3 of Table 1.1 show significant associations between beliefs about racial discrimination and support for preference in hiring and job assistance for blacks, respectively. Column 4 shows that a one standard deviation change in beliefs about racial discrimination in the labor market is associated with a 0.22 of a standard deviation change in beliefs about discrimination in the housing market. Furthermore, column 5 shows that our belief measure is also predictive of whether people think that racial discrimination against blacks in the labor market is a “serious problem.” Our next main result is as follows.

Result 2. *Beliefs about racial discrimination in the labor market are associated with higher donations to a pro-black civil rights organization. The magnitude of a one standard deviation change in beliefs corresponds to about 36 percent of the Democrat–Republican difference in donations. Beliefs about racial discrimination are also positively correlated with self-reported support for pro-black policies.*

Overall, these correlations suggest that our belief measure has high external validity. Not only does it predict responses to qualitative survey questions, it also predicts real donations to a pro-black civil rights organization. But naturally, these correlations need to be interpreted cautiously. The estimated effect of beliefs on donations and self-reported policy views could be confounded due to measurement error, reverse causality, and omitted variable bias. The next section addresses causality by studying the effects of the randomly assigned information treatment.

Table 1.1: The association between beliefs and preferences

	(1) Donations to NGO	(2) Black preference	(3) Black assistance	(4) Disc. housing	(5) Disc. ser. problem
Panel A: Without controls					
Beliefs about discrimination	0.219*** (0.040)	0.241*** (0.036)	0.246*** (0.035)	0.217*** (0.039)	0.294*** (0.035)
Panel B: With controls					
Beliefs about discrimination	0.171*** (0.041)	0.167*** (0.034)	0.169*** (0.035)	0.213*** (0.040)	0.231*** (0.031)
N	653	676	677	673	679

Note: The table show OLS regressions from control group respondents in Experiment 1 (NORC). In **Panel A**, we regress the outcome indicated in each column on standardized beliefs about racial discrimination in the labor market (i.e., beliefs about the number of times resumes with black-sounding names had to be sent out to receive one callback on average). In **Panel B**, we also include pre-specified controls in the regressions (gender, age, race, region, income, education, employment, and political views). *Donations to the NGO* refers to the number of times the respondents preferred money to the pro-black civil rights organization over money for themselves (responses range from 0 to 6). For the outcomes *Black preference* (support for giving qualified black candidates preference over equally qualified white candidates in getting a job) and *Black assistance* (support for giving qualified black candidates assistance in getting a job), answers were given on a scale from 1 (Strongly oppose) to 5 (Strongly support). *Disc. housing* refers to beliefs about the rejection rate of black-sounding names in the housing market (elicited on a scale from 0 to 100). *Disc. ser. problem* refers to the question of whether “racial discrimination against blacks in the labor market is a serious problem” which was elicited on a scale from 1 (Not a problem at all) to 5 (A very serious problem). All outcomes are z-scored.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

1.4 Treatment effects on beliefs and policy views

This section presents treatment effects from providing people with research evidence about the results from the correspondence study by Bertrand and Mullainathan (2004). We first outline our empirical strategy and then present three sets of results: First, we investigate whether people update their beliefs in response to the treatment. Second, we analyze how the treatment affects people’s political behavior as measured by incentivized donations. Third, we analyze how the treatment affects people’s self-reported policy preferences on pro-black policies.

1.4.1 Empirical strategy

We pre-specified the analysis of both experiments in two documents uploaded to the AEA RCT Registry prior to starting the data collection. The empirical strategy outlined in this section follows the pre-analysis plans, which may be accessed with the following link: <https://www.socialscienceregistry.org/trials/2273>. The appendix includes all pre-specified results that are not discussed in the main text.

Main specification Since we expect different treatment effects based on whether the respondents initially overestimate or underestimate racial discrimination, our main specification is the following difference-in-differences equation which we estimate using OLS:

$$y_i = \alpha_0 + \alpha_1 \text{Treatment}_i + \alpha_2 \text{Treatment}_i \times \text{prior}_i + \alpha_3 \text{prior}_i + \alpha_4 \mathbf{x}_i + \varepsilon_i \quad (1.1)$$

where y_i is the outcome of interest; Treatment_i is an indicator for whether respondent i received the research evidence; prior_i is an indicator for initially overestimating racial labor market discrimination (i.e., for having pre-treatment beliefs that resumes with black-sounding names had to be sent out more than 15 times to get one callback on average)¹⁷; \mathbf{x}_i is a vector of pre-specified controls¹⁸; and ε_i is an individual-specific error term. We use robust error terms for inference. Throughout the section, we refer to respondents who initially underestimate and overestimate racial discrimination in the labor market as “underestimators” and “overestimators,” respectively.

Heterogeneity by political views There are several reasons to expect Republicans to respond differently to the information than non-Republicans. For instance, Republicans are much more likely than non-Republicans to oppose government action on ideological grounds. In the second main specification of interest, we therefore allow for political heterogeneity in treatment responses by estimating the following triple-difference equation:

$$y_i = \alpha_0 + \alpha_1 \text{Treatment}_i + \alpha_2 \text{Treatment}_i \times \text{Prior}_i + \alpha_3 \text{Treatment}_i \times \text{Republican}_i + \alpha_4 \text{Treatment}_i \times \text{Prior}_i \times \text{Republican}_i + \alpha_5 \text{Prior}_i + \alpha_6 \text{Republican}_i + \alpha_7 \text{Prior}_i \times \text{Republican}_i + \alpha_8 \mathbf{x}_i + \varepsilon_i$$

where Republican_i is an indicator for self-identifying as a Republican.

¹⁷Since those with accurate pre-treatment beliefs (i.e., 15) should become more confident in their beliefs, which we expected should increase support for pro-black policies, we decided to group them in the same category as those who strictly underestimated racial discrimination.

¹⁸For Experiment 1, we include the following controls: gender (binary), age (in years), two ethnicity indicators (non-Hispanic whites and non-Hispanic blacks); three regional indicators; household size (continuous); log household income (continuous); an indicator for having college degree; and indicator for being employed; and two party affiliation indicators (Republicans and Democrats). For Experiment 2, we also include confidence in prior beliefs as a control (integer from 1 to 5) and, to follow the pre-analysis plan, do not include an indicator for self-identifying as a Democrat.

1.4.2 Do people update their beliefs about racial discrimination?

Experiment 1: Beliefs about the housing market We first examine whether people used the information about racial discrimination in the labor market to update their beliefs about racial discrimination in the housing market.¹⁹ Column 1 shows that treated underestimators increase their estimate of the rejection rate of black-sounding names by 4.2 percentage points ($p < 0.01$). By contrast, treated overestimators decrease their estimate of the rejection rate for black-sounding names by 5.8 percentage points ($p < 0.01$). These estimates are significantly different from each other ($p < 0.01$). Column 2 shows that these results are virtually unaffected by including controls in the regressions. Columns 1 and 2 of Panel B show that there is no significant treatment heterogeneity between Republicans and non-Republicans. One reason for this could be that we incentivized the belief elicitation, making it costly to engage in motivated partisan reasoning.

Experiment 2: Posterior beliefs about the labor market In Experiment 2, we elicited posterior beliefs about racial discrimination in the one-week follow-up. Column 3 shows that treated underestimators increase their estimate of how many times resumes with black-sounding names need to be sent out to get one callback on average by 2.3 resumes ($p < 0.05$). Treated overestimators, by contrast, decrease their estimate by 11 resumes ($p < 0.01$). These estimates are significantly different from each other ($p < 0.01$). Column

¹⁹While respondents were asked about the acceptance rate of black-sounding names (i.e., how many percent of the time they thought reservation requests from black-sounding names were accepted), we recoded the responses such that higher numbers imply more discrimination. The results show beliefs about implied rejection rates instead.

4 shows that the estimates are virtually unaffected by including controls in the regressions. Furthermore, columns 3 and 4 of Panel B show that there is no significant treatment heterogeneity between Republicans and non-Republicans. In Experiment 2, we also elicited confidence in pre-treatment beliefs about racial discrimination in the labor market. Treatment effects on posterior beliefs are stronger for respondents with less confidence in their pre-treatment beliefs (as shown in Table 1.18), consistent with genuine belief updating.

Given all of the estimates discussed above, our next main result can be summarized as follows:

Result 3. *People's beliefs about racial discrimination are responsive to new information. Treated respondents strongly update their beliefs about the extent of racial discrimination in both the labor market and the housing market in response to research evidence from a correspondence study.*

The successful “first stage” on beliefs allows us to investigate whether correcting biases in beliefs about racial discrimination causally affects people's behavior and policy views on pro-black policies.

Table 1.2: Belief updating

	Housing market (NORC)		Labor market (RN)	
	(1)	(2)	(3)	(4)
Panel A: Main specification				
Treatment (a)	4.15*** (1.56)	4.16*** (1.54)	2.25** (1.02)	2.08** (1.02)
Prior × Treatment (b)	-9.94*** (1.91)	-9.91*** (1.90)	-13.27*** (1.62)	-13.08*** (1.62)
Prior	7.66*** (1.54)	7.61*** (1.53)	14.64*** (1.33)	14.00*** (1.34)
N	1366	1366	1701	1701
Controls	No	Yes	No	Yes
Control group mean: Dependent variable	71.1	71.1	19.3	19.3
Control group mean: Prior	0.55	0.55	0.45	0.45
P-value: a + b = 0	0.000	0.000	0.000	0.000
Panel B: Political heterogeneity				
Treatment (a)	2.98 (1.87)	2.91 (1.84)	1.77 (1.22)	1.61 (1.22)
Prior × Treatment (b)	-9.50*** (2.23)	-9.38*** (2.20)	-13.18*** (1.89)	-12.94*** (1.89)
Republican × Treatment (c)	3.94 (3.35)	4.21 (3.34)	1.65 (2.22)	1.66 (2.22)
Prior × Republican × Treatment (d)	0.15 (4.54)	-0.22 (4.55)	0.07 (3.71)	-0.15 (3.65)
Prior	6.62*** (1.79)	6.70*** (1.76)	14.84*** (1.57)	14.21*** (1.57)
Prior × Republican	2.43 (3.63)	2.77 (3.65)	-1.18 (2.97)	-1.04 (2.95)
Republican	-5.18* (2.74)	-4.33 (2.82)	-0.86 (1.47)	-1.48 (1.52)
N	1366	1366	1701	1701
Controls	No	Yes	No	Yes
P-value: a + b = 0	0.000	0.000	0.000	0.000
P-value: a + c = 0	0.013	0.011	0.066	0.082
P-value: b + d = 0	0.018	0.017	0.000	0.000
P-value: a + b + c + d = 0	0.388	0.383	0.000	0.000

Note: The table shows OLS regression results where the dependent variables are post-treatment beliefs about how many percent of the time reservation requests from black-sounding names were rejected on Airbnb (columns 1–2; Experiment 1 with NORC) and post-treatment beliefs about the number of resumes with black-sounding names on average had to be sent out to get one callback on average (columns 3–4; wave 2 of Experiment 2 with Research Now). In even-numbered columns, we include pre-specified controls (including gender, age, race, region, income, education, employment, and political views). “Prior” takes the value one for respondents who overestimate the extent of racial discrimination against blacks in the labor market (i.e., who thought pre-treatment that resumes with black-sounding names had to send out more than 15 resumes to get one callback on average). For post-treatment beliefs about the labor market (columns 3 and 4), we also include confidence in prior beliefs as a control.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

1.4.3 Does the treatment affect donations?

Table 1.3 shows regression results from Experiment 1 on people's real donations to a pro-black civil rights organization.²⁰ In the regression, we z-score the number of donations using the mean and standard deviation of the control group.

Column 1 of Table 1.3 shows that treated underestimators increase their donations to the civil rights organization by 0.17 of a standard deviation ($p < 0.05$).²¹ This effect size corresponds to 29 percent of the Democrat-Republican difference in donations. It also corresponds to about one-half of the difference in donations between those who initially overestimate and underestimate racial discrimination. By contrast, treated respondents who overestimate racial discrimination do not reduce their donations; the treatment effect estimate is close to zero and not statistically significant, ($p = 0.97$), even though respondents in this group changed their beliefs about racial discrimination in the housing market considerably. The interaction effect between pre-treatment beliefs and the treatment is not statistically significant ($p\text{-value} = 0.12$), but goes in the expected direction. Column 2 shows that the estimates are virtually unaffected by including controls in the regressions. These findings suggest that information has most scope to change behavior for people who underestimate racial discrimination. One reason as to why overestimators do not change their behavior could be that the treatment made them more confident that racial discrimination against blacks is a prob-

²⁰We only collected data on donations for respondents in Experiment 1. Respondents could choose between varying amounts of money for themselves or donating \$5 to *The Lawyers' Committee for Civil Rights*, a pro-black civil rights organization.

²¹A subset of respondents only completed a subset of the choices in the multiple price list. Once we restrict the sample to respondents who made all six choices in the multiple price list, the estimated effect sizes are virtually unchanged.

lem, which could offset the fact that they realize that discrimination is less prevalent than their initial estimate.

Columns 3 and 4 of Table 1.3 examine political heterogeneity in treatment effects on donations. We find no significant treatment heterogeneity based people's political affiliation, but generally the data are consistent with stronger treatment effects for non-Republicans and weaker treatment effects for Republicans. Among non-Republicans, treated underestimators increase their donations by 0.23 of a standard deviation ($p < 0.05$), whereas treated overestimators are essentially unaffected by the treatment; these estimates are significantly different from each other ($p < 0.05$). For Republican underestimators, the treatment effect estimate is positive but close to zero and not statistically significant ($p = 0.86$). This estimate is also not significantly different from the effect on non-Republican underestimators ($p = 0.86$). For Republican overestimators, the point estimate is positive but not statistically significant ($p = 0.36$) and also not significantly different from the effect on non-Republican overestimators ($p = 0.51$). The estimated treatment effects are essentially unchanged when we include controls (column 4).²²

Although the treatment substantially narrows the Democrat–Republican gap in beliefs, the Democrat–Republican gap in donations of about 0.6 of a standard deviation is essentially unaffected by the treatment ($p = 0.93$). Our fourth main result is the following:

Result 4. *The provision of information about racial discrimination causally affects donations to an NGO lobbying for blacks in the labor market. The effect differs for people who initially overestimate and underestimate racial discrimination: While the treatment strongly increases donations for underestimators, the treatment has*

²²Table 1.14 shows that results are robust to using a continuous measure of people's pre-treatment beliefs instead of the indicator used in our main specification.

Table 1.3: Treatment effects on donations

	(1)	(2)	(3)	(4)
Treatment (a)	0.174** (0.080)	0.159** (0.075)	0.230** (0.096)	0.213** (0.093)
Prior × Treatment (b)	-0.171 (0.111)	-0.139 (0.107)	-0.259** (0.129)	-0.217* (0.126)
Republican		-0.229*** (0.067)	-0.365*** (0.112)	-0.181 (0.112)
Prior	0.359*** (0.077)	0.269*** (0.075)	0.328*** (0.089)	0.284*** (0.087)
Prior × Republican			-0.087 (0.174)	-0.057 (0.168)
Republican × Treatment (c)			-0.207 (0.160)	-0.191 (0.155)
Prior × Republican × Treatment (d)			0.398 (0.250)	0.325 (0.243)
N	1327	1327	1327	1327
Controls	No	Yes	No	Yes
P-value: a + b = 0	0.97	0.79	0.73	0.96
P-value: a + c = 0			0.86	0.86
P-value: b + d = 0			0.51	0.61
P-value: a + b + c + d = 0			0.35	0.44

Note: The table shows OLS regression results where the dependent variable is the number of donations to the pro-black civil rights organization (the respondents were given a multiple price list where they could choose between money for themselves and \$5 to the pro-black civil rights organization in increments of \$1 from \$0 to \$5). The dependent variable has been z-scored using the mean and standard deviation in the control group). In even-numbered columns, we include the following pre-specified controls: gender, age, race (indicators for blacks and whites), regions (three indicators), household size, income, education (indicator for having at least a two-year college degree), employment (indicator for having for full-time work), and self-reported political affiliation (indicators for Republicans and Democrats). “Prior” takes the value one for respondents who overestimate the extent of racial discrimination against blacks in the labor market (i.e., who thought pre-treatment that resumes with black-sounding names had to send out more than 15 resumes to get one callback on average).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

no effect on overestimators. The effect for treated underestimators is entirely driven by non-Republicans, which means that the treatment fails to narrow Democrat–Republican differences in donations.

1.4.4 Does the treatment affect policy views?

Table 1.4 shows regression results from both experiments on people’s self-reported support for different policies to address racial discrimination in society. Columns 1–4 show results from Experiment 1, while columns 5–8 show results from Experiment 2. In this section, we only report results from the main specification with controls; Table 1.15 shows the corresponding results excluding controls. All outcomes are z-scored and coded such that higher values imply higher support for the policies.

Experiment 1: NORC

Support for pro-black policies Columns 1 and 2 of Panel A of Table 1.4 show support for two “preferential treatment” policies specifically designed to help blacks in the labor market, namely support for giving qualified black candidates preference over equally qualified white candidates in getting a job (column 1) and support for giving qualified black candidates assistance in getting a job (column 2). There is essentially no impact of the treatment on policy views on pro-black policies for either overestimators or underestimators. Moreover, there was no significant heterogeneity between Republicans and non-Republicans in treatment responses on these measures (as shown in Panel B). Our next main result is as follows:

Result 5. *Views on pro-black labor market policies, such as black preference in hiring*

and job assistance programs for blacks, do not change in response to information about the extent of discrimination against blacks in the labor market.

One reason for the lack of treatment effects on support for pro-black policies could be that people have a strong ideological stance on “preferential treatment” policies, making their support for such policies very unresponsive to changes in beliefs.

Support for name-blind recruitment We next analyze treatment effects on support for mandatory name-blind recruitment, i.e., a “non-preferential” policy for hiring in public and private jobs as a way to reduce discrimination in the labor market. The outcome is closely related to our informational treatment, which advised people that employers used names on resumes to discriminate against blacks. From the results shown in Column 4 of Panel A of Table 1.4, we see that the treatment has essentially no impact on underestimators. Overestimators, by contrast, increase their support for name-blind recruitment, but the estimate is not statistically significant ($p=0.45$).

Exploring political heterogeneity in treatment responses (Panel B of Table 1.4), we find significant differences between Republicans and non-Republicans. For non-Republicans, the treatment has a positive but non-significant impact on support for name-blind recruitment among underestimators and essentially no impact among overestimators. For Republicans, by contrast, the treatment decreases support for name-blind recruitment by 0.24 of a standard deviation for underestimators ($p=0.11$) and increases support by 0.36 of a standard deviation for overestimators ($p<0.05$); the increased polarization in attitudes between Republicans who underestimated and overestimated

discrimination is highly significant ($p < 0.01$). One explanation for this finding could be that Republicans have a stronger self-interested motive to oppose name-blind recruitment than non-Republicans.²³

Experiment 2

Support for pro-black policies Columns 5–7 of Panel A in Table 1.4 show treatment effects on support for pro-black policies. While the treatment has essentially no impact on overestimators, it “backfires” for underestimators who significantly reduce their support for pro-black policies when they learn that discrimination was larger than they thought. This backfire effect is entirely driven by Republicans, as shown in Panel B. Treated Republicans who initially underestimate racial discrimination reduce their support for pro-black policies by 0.30 of a standard deviation ($p < 0.01$), an estimate that significantly differs from the treatment effect on non-Republican underestimators ($p < 0.05$). In Experiment 1, we did not observe backfire effects for Republicans. One reason for this difference could be that Republicans in Experiment 1 felt it was not socially acceptable to express very low support for pro-black policies after being informed by the experimenter that discrimination is more prevalent than their initial estimate. This concern does not apply to the same extent in Experiment 2 because of the obfuscation design.

Explaining the backfire effect on support for pro-black policies One potential explanation for why the treatment backfires for Republicans is that it

²³One reason for why Republicans and non-Republicans might differ in their support for name-blind recruitment could be that Republicans are more likely to be white. However, we find similar results and even stronger evidence of polarization in attitudes between Republicans if we restrict the sample to non-Hispanic whites. Results are available upon requests.

simultaneously changes their beliefs about how effective affirmative action programs have been in helping blacks. Among Republicans, we find evidence of strong polarization in beliefs: Treated republican underestimators are 0.36 of a standard deviation more likely to think that affirmative action programs have hurt blacks ($p < 0.01$), whereas Republican overestimators do not significantly change their beliefs in response to the treatment (results are displayed in Column 1 of Table 1.12). For non-Republicans, we observe no treatment effect on beliefs about the effectiveness of affirmative action programs. While these results could reflect genuine updating about the effectiveness of affirmative action, an alternative explanation is that treated Republican underestimators report different beliefs to justify their lower support for pro-black policies.

Support for name-blind recruitment Column 8 of Panel A of Table 1.4 shows treatment effects on support for mandatory name-blind recruitment. The treatment decreases support for name-blind recruitment among underestimators by 0.12 of a standard deviation and increases support among overestimators by 0.13 of a standard deviation. While neither effect is significantly different from zero ($p = 0.09$ and $p = 0.12$, respectively), the estimates are significantly different from each other ($p < 0.01$). In line with the evidence from the first experiment, the negative treatment effect on underestimators is mainly driven by Republicans (Panel B of Table 1.4). While the treatment has essentially no impact on non-Republican underestimators, it decreases support for name-blind recruitment among Republican underestimators by 0.2 of a standard deviation ($p = 0.12$).

Table 1.4: Treatment effects on policy preferences

	Experiment 1 (NORC)				Experiment 2 (Research Now)			
	(1) Black preference	(2) Black assistance	(3) Problack (Index)	(4) Name-blind screening	(5) Black preference	(6) Black assistance	(7) Problack (Index)	(8) Name-blind screening
Panel A: Main specification								
Treatment (a)	-0.028 (0.070)	-0.015 (0.077)	-0.025 (0.071)	0.011 (0.076)	-0.081 (0.059)	-0.136** (0.062)	-0.121** (0.059)	-0.124* (0.064)
Prior × Treatment (b)	-0.037 (0.094)	0.059 (0.099)	0.010 (0.094)	0.079 (0.101)	0.071 (0.087)	0.137 (0.093)	0.116 (0.088)	0.255*** (0.094)
Prior	0.194*** (0.068)	0.234*** (0.071)	0.237*** (0.067)	0.086 (0.073)	-0.077 (0.063)	0.089 (0.065)	0.002 (0.063)	-0.009 (0.066)
N	1377	1374	1371	1378	1720	1720	1720	1720
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
P-value: a + b = 0	0.30	0.49	0.81	0.18	0.88	0.98	0.94	0.06
Panel B: Political heterogeneity								
Treatment (a)	-0.061 (0.084)	-0.072 (0.091)	-0.074 (0.087)	0.114 (0.089)	-0.015 (0.069)	-0.056 (0.071)	-0.039 (0.068)	-0.088 (0.077)
Prior × Treatment (b)	-0.074 (0.109)	0.037 (0.112)	-0.024 (0.109)	-0.079 (0.115)	-0.035 (0.101)	0.012 (0.104)	-0.014 (0.100)	0.209* (0.109)
Republican × Treatment (c)	0.109 (0.150)	0.192 (0.172)	0.166 (0.149)	-0.350** (0.172)	-0.223* (0.136)	-0.268* (0.143)	-0.276** (0.138)	-0.114 (0.141)
Prior × Republican × Treatment (d)	0.275 (0.214)	0.251 (0.253)	0.298 (0.221)	0.666*** (0.244)	0.404** (0.204)	0.471** (0.229)	0.493** (0.211)	0.162 (0.219)
Prior	0.214*** (0.079)	0.273*** (0.076)	0.270*** (0.076)	0.215*** (0.082)	0.018 (0.073)	0.215*** (0.073)	0.125* (0.072)	0.099 (0.076)
Prior × Republican	-0.148 (0.152)	-0.250 (0.190)	-0.219 (0.165)	-0.542*** (0.175)	-0.361** (0.140)	-0.482*** (0.160)	-0.473*** (0.146)	-0.414*** (0.153)
Republican	-0.320*** (0.112)	-0.251** (0.123)	-0.323*** (0.107)	0.120 (0.125)	-0.237** (0.093)	-0.192** (0.093)	-0.244*** (0.092)	-0.043 (0.094)
N	1377	1374	1371	1378	1720	1720	1720	1720
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
P-value: a + b = 0	0.06	0.60	0.15	0.64	0.50	0.56	0.46	0.12
P-value: a + c = 0	0.70	0.41	0.45	0.11	0.04	0.01	0.01	0.09
P-value: b + d = 0	0.27	0.20	0.15	0.01	0.04	0.02	0.01	0.05
P-value: a + b + c + d = 0	0.07	0.02	0.01	0.03	0.33	0.33	0.25	0.26

Note: The table shows OLS regression results. The dependent variables are indicated in each column. In columns 1–4, we present results from Experiment 1; in columns 5–8, we present results from Experiment 2 (wave 2). For the outcomes *Name-blind recruitment* (support for mandatory name-blind recruitment), *Black preference* (support for giving qualified black candidates preference over equally qualified white candidates in getting a job), and *Black assistance* (support for giving qualified black candidates assistance in getting a job), answers were given on a scale from 1 (Strongly oppose) to 5 (Strongly support). These outcome are z-scored using the mean and standard deviation in the control group. *Problack (index)* is the mean of *Black preference* and *Black assistance*; this index was pre-specified. *Prior* takes the value one for respondents who overestimate the extent of racial discrimination against blacks in the labor market. We include pre-specified controls in all regressions (the controls are listed in Table 1.2). * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses.

1.5 Exploring drivers of polarization in policy views

Although the provision of the research evidence strongly reduces political polarization in beliefs about racial discrimination, it does not reduce political polarization in views on pro-black policies and donations. This finding raises the question which other factors drive these differences. In this section, we explore the role that (i) beliefs about differences in work ethic between whites and blacks and (ii) political identity play in driving the partisan gap in attitudes towards pro-black policies.

1.5.1 Beliefs about differences in work ethic

A centuries-old negative stereotype of blacks is the belief that they are “lazy, shiftless, and unambitious” (Gilens, 2009). One reason for why Democrats and Republicans differ in their views on pro-black policies could be that they differ in the extent to which they hold this negative stereotype.²⁴

In Experiment 2, we asked respondents several questions to shed light on mechanisms, including two questions on whether differences in economic outcomes between whites and blacks were primarily the result of “racial discrimination against blacks” or primarily the result of “whites working harder than blacks.” Using data from control group respondents, we show that believing racial inequality is due to “whites working harder than blacks” is, by a large margin, the strongest predictor of attitudes towards pro-black policies (as displayed in Figure 1.10). Agreeing to the statement that racial inequalities are due to “whites working harder than blacks” is associated with

²⁴For a formal model of stereotypes, see Bordalo et al. (2016).

a 0.87 of a standard deviation lower support for black preference in hiring, conditional on controls for demographics and party affiliations ($p < 0.01$). To shed light on whether negative stereotyping of blacks causally affects attitudes towards affirmative action policies, we ran an additional experiment in which we challenge this stereotype with an information intervention.

Experimental design and sample We recruited approximately 3000 American respondents from Amazon Mechanical Turk (MTurk), an online platform commonly used in economic experiments (Cavallo et al., 2016; DellaVigna and Pope, 2018; Horton et al., 2011; Kuziemko et al., 2015). We ran the experiment in October 2018 and submitted a pre-analysis plan to the same AEA RCT Registry trial as the main experiments before we started the data collection.²⁵

In the experiment, we first elicited people's beliefs about which factors they think blacks and whites rate as least important for them in a job. We then randomized respondents in a treatment and control group. Respondents in the treatment group received information that blacks and whites both rate short working hours as the least important characteristic in a job. Respondents in the control group did not receive any information. Subsequently, we measured people's support for pro-black policies using the same self-reported questions as in the main study.

Results In line with negative stereotyping of blacks (Gilens, 2009), the respondents think that whites are 20 percent more likely than blacks to place least weight on short working hours in a job (Table 1.20). Furthermore, only 25 percent have correct beliefs that blacks actually placed the lowest weight

²⁵Instructions are provided in the pre-analysis plan.

on short working hours. But while having incorrect beliefs predicts greater opposition to pro-black policies, the information treatment does not affect support for pro-black policies. The information treatment also does not shift beliefs about whether differences in economic outcomes between blacks and whites are “primarily the result of whites working harder than blacks,” suggesting that the treatment is ineffective in challenging the stereotype of “lazy blacks.” Given our large sample size, we take this as suggestive evidence that beliefs governing racial stereotypes are much less responsive to new information than beliefs about racial discrimination. Furthermore, this result emphasizes that views on pro-black policies are generally very unresponsive to new information.

1.5.2 The role of political identity

During the last four decades, political polarization in beliefs about whether differences in economic outcomes between blacks and whites are “mainly due to discrimination” has strongly increased (Figure 1.9; data from the General Social Survey). This shift in beliefs is part of a broader trend in which American politics has become more polarized along partisan lines than at any point in recent history.²⁶ Since political identity might be a factor that influences both beliefs and attitudes, we decided to run a further experiment to test whether political party identity further polarizes attitudes towards pro-black policies between Republicans and Democrats.

Experimental sample and design We recruited 4000 respondents in collaboration with Research Now, the same market research company as used in

²⁶<http://pewrsr.ch/1mHUL02>, accessed November 30, 2018.

Experiment 2. The sample was constructed to be representative of the US population in terms of age, sex, and region. We ran the experiment in July 2018, and we submitted a pre-analysis plan to the same AEA RCT Registry trial as the main experiments before we started the data collection.²⁷

We randomly assigned respondents into a control group and a treatment group. For respondents in the treatment group, we added the following introductory sentence to the question on whether they support affirmative action in hiring: “In contrast to the Democratic Party, the Republican Party generally opposes all forms of special treatment based on race.” In the main specification, we focused on the 2,737 respondents who self-identify as either Democrats or Republicans. We hypothesized in the pre-analysis plan that this treatment would polarize attitudes by making Democrats more supportive of pro-black policies and Republicans less supportive.

Results The treatment has essentially no impact on attitudes for either Democrats or Republicans (Table 1.19). Given our large sample size, we take this as suggestive evidence that political identity is not a very important driver of pro-black policies.²⁸ This finding underscores the point that views on pro-black policies are hard to move.

²⁷Instructions are provided in the pre-analysis plan.

²⁸While the null result could also reflect that the manipulation was too weak to substantially increase the salience of people’s political identity, we note that a similar manipulation employed by Cappelen et al. (2019) strongly increased political polarization in views on redistribution. We also note that a stronger manipulation would have probably induced too much experimenter demand to be informative about the underlying question.

1.6 Concluding remarks

In this chapter, we provide novel evidence of the determinants of people's support for pro-black policies with a particular focus on the role of beliefs about the extent of racial discrimination against blacks. We first provide representative evidence of people's beliefs about racial discrimination. We document strong heterogeneity in beliefs about the extent of racial discrimination in society and find that people strongly update their beliefs in response to information about the results from a correspondence study (Bertrand and Mullainathan, 2004). However, although the treatment strongly reduces differences in beliefs about racial discrimination between Democrats and Republicans, we do not observe a similar convergence in support for pro-black policies used to combat racial discrimination. Almost three decades ago, Bobo and Kluegel (1993) pointed out "the need to address the denial of contemporary racial discrimination [...] if policies addressing persistent racial inequalities are to be pursued." Our results suggest that correcting people's biases in beliefs about racial discrimination is not sufficient to reduce political differences in support for pro-black policies, and we think more work is needed to better understand the causal drivers of the polarization in support for pro-black policies.

Methodologically, the chapter introduces a new approach of measuring beliefs about discrimination by leveraging correspondence studies to measure beliefs. The advantage of this approach is that it allows for the elicitation of quantitative and incentivized beliefs that are easily comparable across respondents. Furthermore, this approach allows for the provision of research evidence based on clean causal evidence. Our study demonstrates the feasi-

bility of this approach by showing that correspondence studies can easily be explained to and understood by a general population sample. The approach could be useful for researchers who wish to study beliefs about discrimination in other domains, such as discrimination against women. Finally, the approach could be used to measure beliefs about other resume characteristics, such as additional years of education, to measure and change beliefs about the returns to human capital investments with credible research evidence.

Summary of Appendices

Section 3.B provides all the appendix tables. Section 1.A.1 provides summary statistics for Experiment 1 and Experiment 2 as well as evidence of covariate balance and results on attrition. Section 1.A.2 provides treatment effects on some mechanisms questions. Section 1.A.3 provides additional results on robustness and heterogeneity of treatment effects. Section 1.A.4 shows treatment effects from the two additional experiments (Experiment 3 and Experiment 4). Section 1.A.5 provides additional pre-specified tables. Section 1.B provides all the appendix figures. Section 1.C provides screenshots of the consent forms for Experiment 2 and the recruitment email from Research Now.

1.A Appendix tables

1.A.1 Summary statistics, balance and attrition

Table 1.5: Summary statistics: Experiment 1 (NORC)

	Mean	SD	Median	Min.	Max.	Obs.
Respondent age	48.52	16.79	49.00	18.00	92.00	1542
Male	0.46	0.50	0.00	0.00	1.00	1542
Non-Hispanic black	0.11	0.31	0.00	0.00	1.00	1542
Non-Hispanic white	0.66	0.47	1.00	0.00	1.00	1542
Northeast	0.16	0.36	0.00	0.00	1.00	1542
Midwest	0.29	0.45	0.00	0.00	1.00	1542
South	0.33	0.47	0.00	0.00	1.00	1542
Household size	2.69	1.42	2.00	1.00	6.00	1542
Log household income	10.81	0.86	10.92	7.82	12.27	1542
At least some college	0.80	0.40	1.00	0.00	1.00	1542
Paid employee	0.51	0.50	1.00	0.00	1.00	1542
Self-employed	0.10	0.31	0.00	0.00	1.00	1542
Prior (dummy)	0.55	0.50	1.00	0.00	1.00	1382
Prior (continuous)	22.46	21.15	20.00	1.00	100.00	1382
Republican	0.24	0.43	0.00	0.00	1.00	1542
Democrat	0.36	0.48	0.00	0.00	1.00	1542

Notes: This table displays summary statistics for Experiment 1 (NORC). “Prior (dummy)” takes the value one for respondents who overestimate racial discrimination in the labor market. “Prior (continuous)” refers to the number of times the respondents thought resumes with black-sounding names had to be sent out to get one callback on average.

Table 1.6: Summary statistics: Experiment 2 (Research Now)

	Mean	SD	Median	Min.	Max.	Obs.
Respondent age	47.43	15.53	49.50	21.00	69.50	2073
Male	0.50	0.50	0.00	0.00	1.00	2073
Non-Hispanic black	0.06	0.24	0.00	0.00	1.00	2073
Non-Hispanic white	0.49	0.50	0.00	0.00	1.00	2073
Household size	2.46	1.35	2.00	0.00	10.00	2073
Log household income	10.93	0.83	11.04	8.92	12.32	2073
At least 2-year college degree	0.83	0.38	1.00	0.00	1.00	2073
Prior (dummy)	0.46	0.50	0.00	0.00	1.00	2073
Prior (continuous)	18.74	19.91	15.00	1.00	100.00	2073
Confidence in prior	3.34	1.00	3.00	1.00	5.00	2073
Republican	0.26	0.44	0.00	0.00	1.00	2073
Democrat	0.38	0.48	0.00	0.00	1.00	2073
West	0.23	0.42	0.00	0.00	1.00	2073
South	0.35	0.48	0.00	0.00	1.00	2073
Northeast	0.23	0.42	0.00	0.00	1.00	2073
Midwest	0.19	0.39	0.00	0.00	1.00	2073

Notes: This table displays summary statistics for Experiment 2 (Research Now). “Prior (dummy)” takes the value one for respondents who overestimate racial discrimination in the labor market. “Confidence in prior” (i.e., confidence in the answer to the question of how many times resumes with black-sounding names had to be sent out to get one callback on average) was elicited on a scale from 1 (Very unsure) to 5 (Very Sure).

Table 1.7: Balance: Experiment 1 (NORC)

	Treatment (T)	Control (C)	P-value(T - C)	Observations
Respondent age	49.31	47.71	0.062	1542
Male	0.45	0.48	0.258	1542
Non-Hispanic black	0.11	0.11	0.767	1542
Non-Hispanic white	0.67	0.65	0.514	1542
Northeast	0.16	0.15	0.713	1542
Midwest	0.26	0.31	0.033	1542
South	0.34	0.32	0.586	1542
Household size	2.66	2.73	0.308	1542
Log household income	10.84	10.79	0.214	1542
At least some college	0.82	0.78	0.032	1542
Paid employee	0.52	0.50	0.316	1542
Self-employed	0.10	0.11	0.708	1542
Prior (dummy)	0.54	0.55	0.708	1382
Republican	0.23	0.24	0.825	1542
Democrat	0.36	0.35	0.734	1542

Notes: This table displays covariate means for the treatment and control group for Experiment 1 (NORC). “Prior (dummy)” takes the value one for respondents who overestimate racial discrimination in the labor market. The p-value of a joint F-test of a regression of the treatment indicator on all of the covariates is p=0.164.

Table 1.8: Balance: Experiment 2 (Research Now; baseline survey)

	Treatment (T)	Control (C)	P-value(T - C)	Observations
Respondent age	47.19	47.66	0.493	2073
Male	0.50	0.49	0.844	2073
Non-Hispanic black	0.06	0.05	0.335	2073
Non-Hispanic white	0.49	0.48	0.812	2073
Household size	2.42	2.50	0.228	2073
Log household income	10.92	10.94	0.691	2073
At least 2-year college degree	0.83	0.82	0.609	2073
Prior (dummy)	0.47	0.45	0.350	2073
Confidence in prior	3.31	3.36	0.295	2073
Republican	0.25	0.26	0.643	2073
Democrat	0.38	0.37	0.799	2073
West	0.22	0.24	0.225	2073
South	0.35	0.35	0.947	2073
Northeast	0.24	0.22	0.281	2073
Midwest	0.19	0.19	0.940	2073

Notes: This table displays covariate means for the treatment and control group (wave 1 of Experiment 2 with Research Now). “Prior (dummy)” takes the value one for respondents who overestimate racial discrimination in the labor market. “Confidence in prior” (i.e., confidence in the answer to the question of how many times resumes with black-sounding names had to be sent out to get one callback on average) was elicited on a scale from 1 (Very unsure) to 5 (Very Sure). The p-value of a joint F-test of a regression of the treatment indicator on all of the covariates is $p=0.918$.

Table 1.9: Balance: Experiment 2 (Research Now; obfuscated follow-up)

	Treatment (T)	Control (C)	P-value(T - C)	Observations
Respondent age	47.48	48.05	0.449	1671
Male	0.51	0.51	0.990	1671
Non-Hispanic black	0.07	0.06	0.419	1671
Non-Hispanic white	0.49	0.48	0.863	1671
Household size	2.43	2.46	0.640	1671
Log household income	10.92	10.94	0.716	1671
At least 2-year college degree	0.82	0.82	0.987	1671
Prior (dummy)	0.47	0.45	0.357	1670
Confidence in prior	3.32	3.38	0.218	1670
Republican	0.25	0.27	0.449	1671
Democrat	0.39	0.38	0.642	1671
West	0.22	0.25	0.313	1671
South	0.34	0.35	0.717	1671
Northeast	0.25	0.22	0.286	1671
Midwest	0.19	0.18	0.707	1671

Notes: This table displays covariate means for the treatment and control group (wave 2 of Experiment 2 with Research Now). “Prior (dummy)” takes the value one for respondents who overestimate racial discrimination in the labor market. “Confidence in prior” (i.e., confidence in the answer to the question of how many times resumes with black-sounding names had to be sent out to get one callback on average) was elicited on a scale from 1 (Very unsure) to 5 (Very Sure). The p-value of a joint F-test of a regression of the treatment indicator on all of the covariates is $p=0.961$.

Table 1.10: Experiment 2: Correlates of attrition

	Completed Follow-up	
	(1)	(2)
Treatment	-0.025 (0.017)	-0.027 (0.017)
Republican		0.049** (0.023)
Independent		0.041** (0.021)
Log(Income)		-0.001 (0.012)
College		-0.051** (0.024)
Black		0.036 (0.036)
White		-0.007 (0.019)
Prior (dummy) thisstat24		0.016 (0.018)
Confidence in Prior		0.005 (0.009)
Male		0.042** (0.018)
Age		0.001 (0.001)
Response rate	0.806	0.806
Observations	2073	2073

Notes: The outcome variables takes value one if our respondent completed the follow-up study (wave 2 of Experiment 2 with Research Now). “Treatment” takes value one if the respondent received information about the results from the correspondence study. “Prior (dummy)” takes the value one for respondents who overestimate racial discrimination in the labor market. “Confidence in prior” (i.e., confidence in the answer to the question of how many times resumes with black-sounding names had to be sent out to get one callback on average) was elicited on a scale from 1 (Very unsure) to 5 (Very Sure). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

1.A.2 Mechanisms

Table 1.11: Treatment effects: Views on whether discrimination is a “serious problem”

	Experiment 1 (NORC)		Experiment 2 (RN)	
	(1)	(2)	(3)	(4)
Panel A: Main specification				
Treatment (a)	0.178** (0.083)	0.157** (0.072)	0.127** (0.062)	0.108* (0.056)
Prior × Treatment (b)	-0.046 (0.105)	-0.019 (0.092)	-0.017 (0.086)	-0.001 (0.078)
Prior	0.429*** (0.076)	0.302*** (0.067)	0.326*** (0.060)	0.325*** (0.055)
N	1379	1379	2073	2073
Controls	No	Yes	No	Yes
P-value: a + b = 0	0.040	0.016	0.061	0.049
Panel B: Political heterogeneity				
Treatment (a)	0.170* (0.099)	0.141 (0.090)	0.197*** (0.070)	0.189*** (0.066)
Prior × Treatment (b)	-0.092 (0.119)	-0.042 (0.109)	-0.082 (0.095)	-0.083 (0.090)
Republican × Treatment (c)	0.010 (0.156)	0.051 (0.147)	-0.257* (0.135)	-0.280** (0.127)
Republican × Prior × Treatment (d)	0.283 (0.221)	0.166 (0.212)	0.207 (0.189)	0.283 (0.178)
N	1379	1379	2073	2073
Controls	No	Yes	No	Yes
P-value: a + b = 0	0.242	0.113	0.070	0.090
P-value: a + c = 0	0.137	0.098	0.602	0.403
P-value: b + d = 0	0.303	0.496	0.444	0.191
P-value: a + b + c + d = 0	0.009	0.024	0.575	0.310

Note: The table shows OLS regression results where the dependent variable is agreement to the statement that “racial disagreement against blacks in the labor market is a serious problem.” Columns 1 and 2 show responses from Experiment 1 (NORC), whereas columns 3 and 4 show responses from the first wave of Experiment 2 (Research Now). In both experiments, answers were given from a scale from 1 (Not a problem at all) to 5 (A very serious problem). The outcome has been z-scored by the mean and standard deviation of the control group. “Prior” takes the value one for respondents who overestimate the extent of racial discrimination against blacks in the labor market. Even-numbered columns include pre-specified controls (as listed in Table 1.2).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 1.12: Experiment 2: Treatment effects – mechanism questions

	(1) Affirmative action hurts	(2) Inequality due to effort	(3) Inequality due to disc.	(4) Disc. ser. problem
Panel A: Main specification				
Treatment (a)	0.054 (0.066)	0.015 (0.061)	0.048 (0.062)	-0.022 (0.063)
Prior × Treatment (b)	-0.083 (0.095)	-0.121 (0.087)	-0.081 (0.090)	0.189** (0.089)
Prior	0.022 (0.067)	-0.080 (0.062)	0.465*** (0.064)	0.105* (0.063)
N	1720	1719	1715	1715
Controls	Yes	Yes	Yes	Yes
P-value: a + b = 0	0.669	0.087	0.607	0.008
Panel B: Political heterogeneity				
Treatment (a)	-0.076 (0.074)	-0.046 (0.071)	0.089 (0.076)	0.017 (0.076)
Prior × Treatment (b)	0.080 (0.104)	0.003 (0.100)	-0.099 (0.104)	0.081 (0.105)
Republican × Treatment (c)	0.441*** (0.155)	0.205 (0.139)	-0.137 (0.132)	-0.131 (0.134)
Republican × Prior × Treatment (d)	-0.592** (0.240)	-0.488** (0.204)	0.032 (0.211)	0.440** (0.194)
N	1720	1719	1715	1715
Controls	Yes	Yes	Yes	Yes
P-value: a + b = 0	0.953	0.543	0.891	0.182
P-value: a + c = 0	0.007	0.184	0.664	0.302
P-value: b + d = 0	0.018	0.006	0.716	0.001
P-value: a + b + c + d = 0	0.383	0.013	0.442	0.001

Note: The table shows OLS regression results where the dependent variables are indicated in each column. Responses were elicited in the second wave of Experiment 2 (the obfuscated follow-up study). *Affirmative action hurts* refers to the question of whether “affirmative action programs for the past fifty years have helped blacks blacks” which was elicited on a scale from 1 (Strongly helped) to 7 (Strongly hurt). *Inequality due to effort* refers to the question of whether “differences in economic outcomes between whites and blacks are primarily the result of racial discrimination against blacks” which was elicited on a scale from 1 (Strongly disagree) to 7 (Strongly agree). *Inequality due to disc.* refers to the question of whether “differences in economic outcomes between whites and blacks are primarily the result of whites working harder than blacks” which was elicited on scale from 1 (Strongly disagree) to 7 (Strongly agree). *Disc. ser. problem* refers to the question of whether “racial discrimination against blacks in the labor market is a serious problem” which was elicited on a scale from 1 (Not a problem at all) to 5 (A very serious problem). All responses are z-scored using the mean and the standard deviation of the control group. Controls include gender, age, race, region, income, education, employment, political views, and confidence in prior beliefs. *Prior* takes the value one for respondents who overestimate the extent of racial discrimination against blacks in the labor market.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 1.13: Correlates of willingness to pay for research evidence

	Willingness to pay	
	Raw	z-score
Republican	-0.481** (0.220)	-0.172** (0.079)
Age	0.012* (0.007)	0.004* (0.002)
Log(Income)	0.018 (0.126)	0.006 (0.045)
Black	-0.407 (0.414)	-0.145 (0.148)
White	-0.487** (0.209)	-0.174** (0.075)
College	0.321 (0.255)	0.115 (0.091)
Male	-0.469** (0.192)	-0.167** (0.069)
Prior	0.008* (0.004)	0.003* (0.002)
Confidence in prior	0.026 (0.100)	0.009 (0.036)
Mean	3.318	-0.001
Observations	861	861

Notes: The table show OLS regressions using control group respondents from Experiment 2 (Research Now). We offered control group respondents the option to buy information about the results from the correspondence study by Bertrand and Mullainathan (2004). Willingness to pay to receive the information was elicited using a multiple price list where respondents could choose between receiving the information or varying amounts for themselves (between 10 cents and \$1). “Willingness to pay” is the number of times individuals prefer to receive information over receiving money (on a scale from 0 to 7). Column 1 shows the raw score, whereas column 2 shows the z-score (standardized using the mean and standard deviation of the responses). “Prior” takes the value one for respondents who overestimate the extent of racial discrimination against blacks in the labor market. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

1.A.3 Robustness and additional heterogeneity

Table 1.14: Treatment effects on donations: Robustness with continuous prior

	(1)	(2)	(3)	(4)
Treatment	0.256*** (0.092)	0.246*** (0.089)	0.325*** (0.110)	0.300*** (0.107)
Prior × Treatment	-0.009** (0.004)	-0.009** (0.004)	-0.012*** (0.004)	-0.010** (0.004)
Prior	0.015*** (0.003)	0.012*** (0.003)	0.015*** (0.003)	0.013*** (0.003)
Republican		-0.232*** (0.067)	-0.293** (0.133)	-0.115 (0.134)
Prior × Republican			-0.006 (0.007)	-0.005 (0.007)
Republican × Treatment			-0.263 (0.193)	-0.224 (0.190)
Prior × Republican × Treatment			0.013 (0.010)	0.010 (0.010)
N	1327	1327	1327	1327
Controls	No	Yes	No	Yes

Note: The table shows OLS regression results where the dependent variable is the number of donations to the pro-black civil rights organization (the respondents were given a multiple price list where they could choose between money for themselves and \$5 to the pro-black civil rights organization in \$1 increments from \$0 to \$5). The dependent variable has been z-scored using the mean and standard deviation in the control group. In even-numbered columns, we include the following pre-specified controls: gender, age, race (indicators for blacks and whites), regions (three indicators), household size, income, education (indicator for having at least a two-year college degree), employment (indicator for having for full-time work), and self-reported political affiliation (indicators for Republicans and Democrats). “Prior” refers beliefs about the number of times resumes with black-sounding names had to be sent out to get one callback on average (the question was elicited on a scale from 1 to 100, and in line with the pre-analysis plan we have top-coded responses at 50).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 1.15: Treatment effects on policy preferences: Results without controls

	Experiment 1 (NORC)			Experiment 2 (Research Now)				
	(1) Name-blind recruitment	(2) Black preference	(3) Black assistance	(4) Problack (Index)	(5) Name-blind recruitment	(6) Black preference	(7) Black assistance	(8) Problack (Index)
Panel A: Main specification								
Treatment (a)	0.028 (0.079)	-0.015 (0.077)	0.010 (0.082)	-0.004 (0.079)	-0.101 (0.066)	-0.047 (0.066)	-0.102 (0.065)	-0.083 (0.066)
Prior × Treatment (b)	0.058 (0.106)	-0.072 (0.104)	0.029 (0.106)	-0.026 (0.104)	0.224** (0.098)	0.018 (0.096)	0.088 (0.098)	0.057 (0.096)
Prior	0.170** (0.076)	0.304*** (0.075)	0.354*** (0.075)	0.367*** (0.075)	-0.008 (0.068)	-0.095 (0.067)	0.107 (0.068)	0.000 (0.068)
N	1378	1377	1374	1371	1720	1720	1720	1720
Controls	No	No	No	No	No	No	No	No
P-value: a + b = 0	0.22	0.21	0.57	0.66	0.091	0.67	0.84	0.72
Panel B: Political heterogeneity								
Treatment (a)	0.145 (0.092)	-0.038 (0.092)	-0.037 (0.096)	-0.042 (0.095)	-0.071 (0.078)	0.009 (0.075)	-0.034 (0.074)	-0.013 (0.073)
Prior × Treatment (b)	-0.130 (0.120)	-0.129 (0.119)	-0.014 (0.119)	-0.085 (0.119)	0.193* (0.112)	-0.062 (0.106)	-0.017 (0.107)	-0.046 (0.105)
Republican × Treatment (c)	-0.406** (0.174)	0.065 (0.154)	0.154 (0.175)	0.121 (0.154)	-0.118 (0.144)	-0.231 (0.148)	-0.267* (0.148)	-0.280* (0.149)
Republican × Prior × Treatment (d)	0.803*** (0.248)	0.374* (0.223)	0.339 (0.258)	0.406* (0.230)	0.133 (0.227)	0.346 (0.219)	0.440* (0.234)	0.441** (0.224)
N	1378	1377	1374	1371	1720	1720	1720	1720
Controls	No	No	No	No	No	No	No	No
P-value: a + b = 0	0.844	0.027	0.460	0.078	0.130	0.481	0.502	0.429
P-value: a + c = 0	0.077	0.824	0.427	0.520	0.115	0.082	0.019	0.024
P-value: b + d = 0	0.002	0.193	0.155	0.103	0.100	0.140	0.043	0.046
P-value: a + b + c + d = 0	0.010	0.056	0.012	0.010	0.387	0.667	0.458	0.496

Note: The table shows OLS regression results. The dependent variables are indicated in each column. In columns 1–4, we present results from Experiment 1; in columns 5–8, we present results from Experiment 2. For the outcomes *Name-blind recruitment* (support for mandatory name-blind recruitment), *Black preference* (support for giving qualified black candidates preference over equally qualified white candidates in getting a job), and *Black assistance* (support for giving qualified black candidates assistance in getting a job), answers were given on a scale from 1 (Strongly oppose) to 5 (Strongly support). These outcomes are z-scored using the mean and standard deviation in the control group. *Problack (index)* is the mean of *Black preference* and *Black assistance*; this index was pre-specified. *Prior* takes the value one for respondents who overestimate the extent of racial discrimination against blacks in the labor market.

* p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses.

Table 1.16: Treatment effects on policy preferences: Results with continuous prior

	Experiment 1 (NORC)				Experiment 2 (Research Now)			
	(1) Name-blind recruitment	(2) Black preference	(3) Black assistance	(4) Problack (Index)	(5) Name-blind recruitment	(6) Black preference	(7) Black assistance	(8) Problack (Index)
Panel A: Main specification								
Treatment	0.044 (0.088)	0.090 (0.080)	0.055 (0.087)	0.081 (0.081)	-0.136* (0.074)	-0.109 (0.070)	-0.153** (0.072)	-0.147** (0.070)
Prior × Treatment	0.000 (0.003)	-0.007** (0.003)	-0.002 (0.003)	-0.006* (0.003)	0.008** (0.003)	0.004 (0.003)	0.005 (0.003)	0.005 (0.003)
Prior	0.007*** (0.002)	0.012*** (0.002)	0.012*** (0.002)	0.014*** (0.002)	0.001 (0.002)	-0.002 (0.002)	0.004 (0.002)	0.001 (0.002)
N	1378	1377	1374	1371	1720	1720	1720	1720
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Political heterogeneity								
Treatment	0.130 (0.100)	0.032 (0.095)	-0.039 (0.100)	-0.003 (0.096)	-0.113 (0.087)	-0.033 (0.080)	-0.069 (0.082)	-0.057 (0.080)
Prior × Treatment	-0.003 (0.004)	-0.007** (0.004)	-0.001 (0.003)	-0.005 (0.003)	0.007* (0.004)	0.000 (0.004)	0.001 (0.004)	0.001 (0.004)
Republican × Treatment	-0.356* (0.207)	0.175 (0.177)	0.290 (0.210)	0.258 (0.179)	-0.064 (0.169)	-0.280* (0.160)	-0.320* (0.171)	-0.338** (0.165)
Prior × Republican × Treatment	0.018** (0.009)	0.003 (0.008)	0.001 (0.010)	0.002 (0.008)	0.001 (0.009)	0.015* (0.008)	0.016* (0.009)	0.017** (0.008)
N	1378	1377	1374	1371	1720	1720	1720	1720
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table shows OLS regression results. The dependent variables are indicated in each column. In columns 1–4, we present results from Experiment 1; in columns 5–8, we present results from Experiment 2. For the outcomes *Name-blind recruitment* (support for mandatory name-blind recruitment), *Black preference* (support for giving qualified black candidates preference over equally qualified white candidates in getting a job), and *Black assistance* (support for giving qualified black candidates assistance in getting a job), answers were given on a scale from 1 (Strongly oppose) to 5 (Strongly support). These outcomes are z-scored using the mean and standard deviation in the control group. *Problack (index)* is the mean of *Black preference* and *Black assistance*; this index was pre-specified. *Prior* refers beliefs about the number of times resumes with black-sounding names had to be sent out to get one callback on average (the question was elicited on a scale from 1 to 100, and in line with the pre-analysis plan we have top-coded responses at 50).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 1.17: Treatment effects with probability weights (Experiment 1; NORC)

	(1) Disc.: housing	(2) Donations to NGO	(3) Name-blind recruitment	(4) Black preference	(5) Black assistance	(6) Disc. ser. problem
Panel A: Man specification						
Treatment (a)	2.413 (2.087)	0.158 (0.180)	0.065 (0.099)	0.020 (0.101)	-0.009 (0.108)	0.231** (0.114)
Prior × Treatment (b)	-7.313*** (2.523)	-0.001 (0.252)	-0.021 (0.134)	-0.064 (0.132)	0.099 (0.134)	-0.081 (0.148)
Prior	5.157*** (1.925)	0.290 (0.177)	0.070 (0.093)	0.124 (0.087)	0.096 (0.096)	0.260** (0.103)
N	1366	1327	1378	1377	1374	1379
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	71.69	1.93	3.47	2.70	3.41	3.18
P-value: a + b = 0	0.000	0.371	0.627	0.598	0.280	0.100
Panel B: Political heterogeneity						
Treatment (a)	1.515 (2.418)	0.212 (0.222)	0.234** (0.111)	-0.009 (0.117)	-0.134 (0.125)	0.243* (0.141)
Prior × Treatment (b)	-7.022** (2.811)	-0.145 (0.296)	-0.295** (0.149)	-0.079 (0.150)	0.115 (0.151)	-0.158 (0.176)
Republican × Treatment (c)	3.035 (4.769)	-0.184 (0.368)	-0.579** (0.230)	0.097 (0.230)	0.422* (0.232)	-0.048 (0.237)
Republican × Prior × Treatment (d)	-0.017 (6.354)	0.655 (0.579)	1.145*** (0.331)	0.129 (0.302)	0.148 (0.332)	0.376 (0.327)
N	1366	1327	1378	1377	1374	1379
Controls	Yes	Yes	Yes	Yes	Yes	Yes
P-value: a + b = 0	0.000	0.732	0.536	0.367	0.830	0.393
P-value: a + c = 0	0.269	0.925	0.086	0.658	0.146	0.304
P-value: b + d = 0	0.219	0.306	0.004	0.849	0.370	0.431
P-value: a + b + c + d = 0	0.521	0.180	0.020	0.401	0.010	0.034

Note: The table shows OLS regressions with probability weights where the dependent variable is indicated in each column (applying probability weights was not pre-specified). *Disc. housing* refers to beliefs about the rejection rate of black-sounding names in the housing market (elicited on a scale from 0 to 100). *Donations to the NGO* refers to the number of times the respondents preferred money to the pro-black civil rights organization over money for themselves (responses range from 0 to 6). For the outcomes *Name-blind recruitment* (support for mandatory name-blind recruitment), *Black preference* (support for giving qualified black candidates preference over equally qualified white candidates in getting a job), and *Black assistance* (support for giving qualified black candidates assistance in getting a job), answers were given on a scale from 1: “Strongly oppose” to 5: “Strongly support.” *Disc. ser. problem* refers to the question of whether “racial discrimination against blacks in the labor market is a serious problem” which was elicited on a scale from 1 (Not a problem at all) to 5 (A very serious problem). The outcomes in columns 2–6 are z-scored using the mean and standard deviation in the control group. Controls are listed in Table 1.3. *Prior* takes the value one for respondents who overestimate the extent of racial discrimination against blacks in the labor market.

* p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses.

Table 1.18: Belief updating: Heterogeneity by confidence in prior beliefs

	Labor market	
	(1)	(2)
Panel A: Main specification		
Treatment	2.25** (1.02)	2.10** (1.02)
Prior × Treatment	-13.27*** (1.62)	-13.09*** (1.62)
Prior	14.64*** (1.33)	14.09*** (1.34)
N	1701	1701
Controls	No	Yes
Panel B: Heterogeneity by confidence		
Treatment	11.20*** (4.17)	11.94*** (4.12)
Prior × Treatment	-22.85*** (6.23)	-23.35*** (6.13)
Confidence × Treatment	-2.63** (1.22)	-2.89** (1.20)
Prior × Confidence × Treatment	2.81 (1.86)	3.01* (1.82)
Prior	19.61*** (5.03)	19.02*** (5.01)
Prior × Confidence	-1.47 (1.50)	-1.45 (1.49)
Confidence	1.22 (0.94)	1.35 (0.94)
N	1701	1701
Controls	No	Yes

Note: The table shows OLS regression results where the dependent variable is post-treatment beliefs about the number of resumes with black-sounding names on average had to be sent out to get one callback on average (wave 2 of Experiment 2 with Research Now). In column 2, we include pre-specified controls (including gender, age, race, region, income, education, employment, and political views). “Prior” takes the value one for respondents who overestimate the extent of racial discrimination against blacks in the labor market (i.e., who thought pre-treatment that resumes with black-sounding names had to send out more than 15 resumes to get one callback on average). “Confidence” refers to confidence in pre-treatment beliefs (measured instantly after the belief elicitation) and was elicited on a scale from 1 (Very unsure) to 5 (Very sure).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

1.A.4 Results from follow-up experiments

Table 1.19: Experiment 3: Treatment effects of a political party prime

	(1)	(2)	(3)	(4)
Treatment	0.04 (0.05)	0.03 (0.05)	-0.04 (0.05)	-0.03 (0.05)
Republicans	-0.61*** (0.05)	-0.61*** (0.05)	-0.20*** (0.05)	-0.17*** (0.05)
Treatment × Republicans	-0.06 (0.07)	-0.03 (0.07)	0.02 (0.07)	0.03 (0.07)
Democrats			0.41*** (0.05)	0.44*** (0.05)
Treatment × Democrats			0.08 (0.07)	0.06 (0.07)
N	2737	2737	4000	4000
Controls	No	Yes	No	Yes

Note: The table shows OLS regressions from Experiment 3 (Research Now). The dependent variable is support for “government and private programs that give qualified black and other racial minority candidates preference over equally qualified white candidates in getting a job.” Answers were given on a scale from 1 (Strongly oppose) to 5 (Strongly support). We have z-scored the responses by the mean and standard deviation in the control group. The treatment was a political party prime, where we reminded respondents about party views on affirmative action as follows: “In contrast to the Democratic Party, the Republican Party generally opposes all forms of special treatment based on race.” In even-numbered columns, we include the following pre-specified controls: gender, age, and education. In line with the pre-analysis, we exclude Independents from the regression in columns 1–2 as the treatment was tailored to affect attitudes for Republicans and Democrats. In columns 3–4, add interaction terms between the treatment and Democrats and add Independents to the regressions. The sample was recruited from Research Now and is representative of the US population on the following observable characteristics: age, gender, and region.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 1.20: Experiment 4: Treatment effects of information about racial stereotypes

	(1) Black preference	(2) Black assistance	(3) Problack (Index)	(4) Inequality: effort
Panel A: Main specification				
Treatment	-0.001 (0.032)	0.012 (0.033)	0.006 (0.028)	0.040 (0.032)
Panel B: Heterogeneity				
Treatment (a)	0.02 (0.04)	0.03 (0.04)	0.02 (0.03)	0.04 (0.04)
Prior × Treatment (b)	-0.09 (0.07)	-0.06 (0.07)	-0.07 (0.06)	0.01 (0.07)
Prior	0.18*** (0.05)	0.11** (0.05)	0.15*** (0.05)	-0.13*** (0.05)
N	2999	2999	2999	2999
Controls	Yes	Yes	Yes	Yes
P-value: a + b = 0	0.29	0.61	0.37	0.43

Note: The table shows OLS regression results from Experiment 4 (MTurk). The dependent variables are indicated in each column. For the outcomes *Black preference* (support for giving qualified black candidates preference over equally qualified white candidates in getting a job) and *Black assistance* (support for giving qualified black candidates assistance in getting a job), answers were given on a scale from 1: “Strongly oppose” to 5: “Strongly support.” These outcome are z-scored using the mean and standard deviation in the control group. *Problack (index)* is the mean of *Black preference* and *Black assistance*; this index was pre-specified. For the outcome “Inequality: effort” (agreement to the statement that differences in economic outcomes between blacks and whites are due to whites working harder than blacks), answers were given on a scale from 1 (Strongly disagree) to 7 (Strongly agree) and then z-scored. *Prior* is indicator taking the value one for respondents who thought that blacks were most likely to rank “Working hours are short, lots of free time” as the least important characteristic in a job. Controls were pre-specified and include the prior, two racial indicators (black and white), a gender indicator, a college indicator, age, log income, and two indicators for political status (Democrats and Republicans).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

1.A.5 Additional pre-specified tables

Table 1.21: Pre-specified regressions: Experiment 1 (NORC)

	Racial disc. is a ser. prob.	Preference for blacks	Assistance for blacks	Pro-black policy index	Name-blind screening	Racial disc.: housing	Donation NGO
Panel A: Main Effect							
Treatment	0.147*** (0.045)	-0.049 (0.047) [1.000]	0.019 (0.050) [1.000]	-0.015 (0.042)	0.054 (0.050)	-0.065 (0.047)	0.082 (0.053)
Observations	1379	1377	1374	1371	1378	1366	1327
Panel B: Prior							
Treatment × (A) Prior > 15	-0.019 (0.092)	-0.037 (0.094)	0.057 (0.099)	0.009 (0.084)	0.080 (0.101)	-0.501*** (0.096)	-0.137 (0.107)
Treatment (B)	0.157** (0.072)	-0.029 (0.070)	-0.012 (0.078)	-0.020 (0.064)	0.010 (0.076)	0.210*** (0.078)	0.157** (0.076)
Pr(A+B)=0 Observations	0.016 1379	0.299 1377	0.474 1374	0.842 1371	0.178 1378	0.000 1366	0.790 1327
Panel C: Republican							
Treatment × Republican (A)	0.126 (0.103)	0.238** (0.106)	0.289** (0.124)	0.265*** (0.097)	-0.051 (0.123)	0.285** (0.115)	-0.016 (0.118)
Treatment (B)	0.118** (0.052)	-0.104* (0.055)	-0.048 (0.055)	-0.077 (0.048)	0.066 (0.057)	-0.131** (0.053)	0.086 (0.062)
Pr(A+B)=0 Observations	0.006 1379	0.143 1377	0.031 1374	0.026 1371	0.894 1378	0.133 1366	0.489 1327

Notes: For the outcome **Racial discrimination serious problem**, answers were given from a scale from 1: “Not a problem” at all to 5: “A very serious problem”. For the outcomes **Support preference for blacks**, **Support assistance for blacks**, and **Support name-blind recruitment**, answers were given on a scale from 1: “Strongly oppose” to 5: “Strongly support”. Policy preference index is an unweighted mean of people’s (z-scored) support for giving blacks (i) preference in the hiring process and (ii) assistance programs for blacks. For **Racial discrimination — housing market**, answers were given on a scale from 0 to 100 (higher values imply more discrimination). For **Donation NGO**, we count the number of times the respondent preferred money for the NGO over money for self we count the number of times the respondent preferred money for the NGO over money for self (scale 0–6). The outcome variables are z-scored using the mean and standard deviation in the control group. “Treatment” takes value 1 if the respondent received information about the results from the correspondence study. “Prior > 15” takes value one if our respondents overestimate the extent of racial discrimination. “Republican” takes value 1 if our respondent identifies as a Republican. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 1.22: Pre-specified regressions: Experiment 2 (Research Now)

	Racial discr. serious problem		Preference for blacks		Assistance for blacks		Pro-black policy index		Name-blind screening		Posterior: Belief		Racial Inequality due to		Affirmative action hurts	
	main	follow-up	for blacks	for blacks	for blacks	for blacks	policy index	screening	Belief	Effort	Discrimination	action hurts				
Panel A: Main Effect																
Treatment	0.110** (0.039)	0.068 (0.044)	-0.050 (0.043) [0.284]	-0.073 (0.046) [0.284]	-0.061 (0.039)	-0.004 (0.047)	-3.982*** (0.815)	-0.036 (0.043)	0.007 (0.045)	0.025 (0.048)	1721	1716	1720	1716	1721	1721
Observations	2073	1716	1721	1721	1721	1721	1702	1720	1716	1721	1716	1720	1716	1721	1721	1721
Panel B: Prior																
Treatment × (A) Prior > 15	-0.004 (0.077)	0.200** (0.088)	0.082 (0.087)	0.142 (0.093)	0.112 (0.077)	0.257*** (0.094)	-13.030*** (1.630)	-0.126 (0.086)	-0.097 (0.090)	-0.093 (0.096)	1721	1721	1720	1716	1721	1721
Treatment (B)	0.111** (0.055)	-0.024 (0.062)	-0.087 (0.059)	-0.139** (0.062)	-0.113** (0.052)	-0.122* (0.064)	2.044** (1.018)	0.022 (0.060)	0.051 (0.062)	0.068 (0.066)	1721	1721	1720	1716	1721	1721
Pr(A+B)=0 Observations	0.045 2073	0.005 1716	0.931 1721	0.961 1721	0.985 1721	0.052 1721	0.000 1702	0.093 1720	0.487 1716	0.713 1721	1721	1720	1716	1721	1721	1721
Panel C: Republican																
Treatment × Republican (A)	-0.153* (0.091)	0.038 (0.098)	-0.064 (0.102)	-0.087 (0.112)	-0.075 (0.093)	-0.071 (0.108)	2.642 (1.798)	0.014 (0.102)	-0.119 (0.103)	0.191 (0.119)	1721	1721	1720	1716	1721	1721
Treatment (B)	0.149*** (0.044)	0.059 (0.052)	-0.033 (0.049)	-0.051 (0.052)	-0.042 (0.043)	0.014 (0.054)	-4.672*** (0.951)	-0.039 (0.049)	0.037 (0.052)	-0.025 (0.052)	1721	1721	1720	1716	1721	1721
Pr(A+B)=0 Observations	0.959 2073	0.247 1716	0.278 1721	0.164 1721	0.152 1721	0.546 1721	0.188 1702	0.773 1720	0.362 1716	0.120 1721	1721	1720	1716	1721	1721	1721

Notes: For the outcome **Racial discrimination serious problem**, answers were given from a scale from 1: "Not a problem" at all to 5: "A very serious problem". For the outcomes **Support preference for blacks**, **Support assistance for blacks**, and **Support name-blind recruitment**, answers were given on a scale from 1: "Strongly oppose" to 5: "Strongly support". Policy preference index is an unweighted mean of people's (z-scored) support for giving blacks (i) preference in the hiring process and (ii) assistance programs for blacks. "Racial inequality due to effort" is people's agreement to the following statement: "Differences in economic outcomes between whites and blacks are primarily the result of racial discrimination against blacks." "Posterior belief" is people's estimate of the number of times a resume with black-sounding name had to be sent to get one callback. "Racial inequality due to discrimination" is people's agreement to the following statement: To what extent do you agree with the following statement: "Differences in economic outcomes between whites and blacks are primarily the result of whites working harder than blacks." Responses to these questions are on a 7-point scale where (1) means "strongly disagree" and (7) means "strongly agree". "Affirmative action hurts" is people's response to the question: "Overall, do you think affirmative action programs for the past fifty years have helped blacks, hurt them, or had no effect one way or the other?" People answer this question on a 7-point scale ranging from (1) strongly helped to (7) strongly hurt. The outcome variables are z-scored using the mean and standard deviation in the control group. "Treatment" takes value 1 if the respondent received information about the results from the correspondence study. "Prior > 15" takes value one if our respondents overestimate the extent of racial discrimination. "Republican" takes value 1 if our respondent identifies as a Republican. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

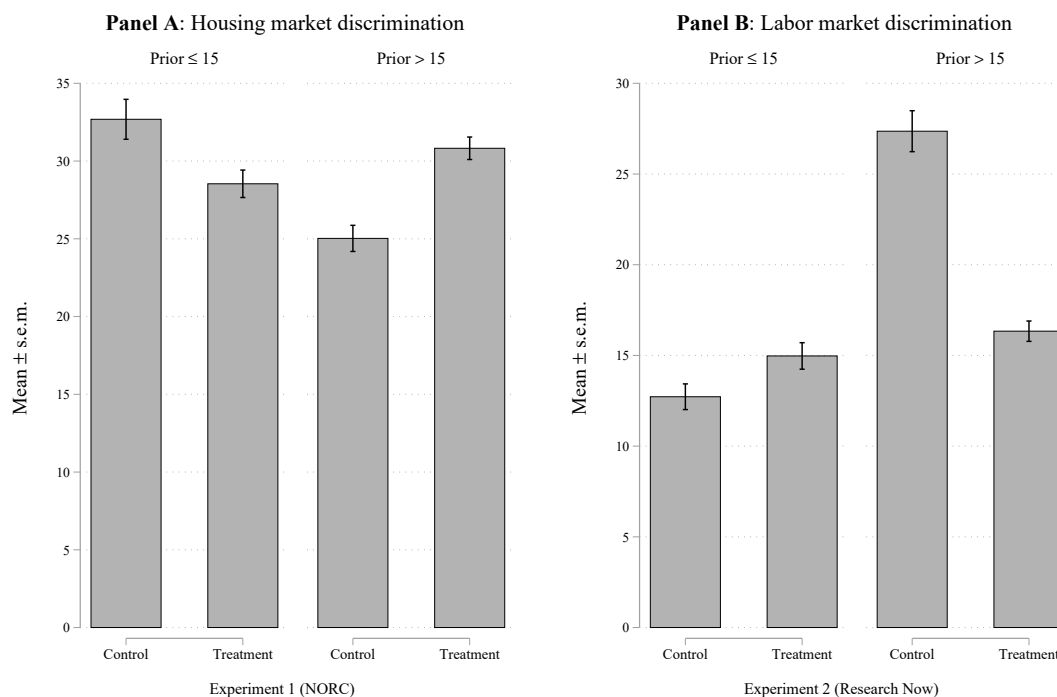
Table 1.23: Pre-specified regressions II: Experiment 2 (Research Now)

	Racial discr: serious problem		Preference	Assistance	Pro-black	Name-blind	Posterior:
	main	follow-up	for blacks	for blacks	policy index	screening	Belief
Panel A:							
Treatment × (A)	0.000	0.006*	0.003	0.005	0.004	0.007**	-0.573***
Prior (continuous)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.068)
Treatment (B)	0.110*	-0.027	-0.103	-0.148**	-0.125**	-0.128*	6.085***
	(0.063)	(0.072)	(0.069)	(0.072)	(0.062)	(0.074)	(1.268)
Pr(A+B)=0	0.069	0.754	0.134	0.038	0.041	0.089	0.000
Observations	2073	1715	1720	1720	1720	1720	1701
Panel B:							
Treatment × (A)	-0.167**	-0.020	0.092	0.035	0.064	-0.183*	-1.145
Male	(0.077)	(0.088)	(0.086)	(0.092)	(0.077)	(0.094)	(1.625)
Treatment (B)	0.192***	0.079	-0.096	-0.091	-0.094*	0.088	-3.404***
	(0.054)	(0.064)	(0.059)	(0.063)	(0.053)	(0.064)	(1.134)
Pr(A+B)=0	0.652	0.334	0.948	0.404	0.593	0.164	0.000
Observations	2073	1715	1720	1720	1720	1720	1701
Panel C:							
Treatment × (A)	-0.099**	-0.020	0.046	0.008	0.027	-0.075	-1.021
Confidence in prior	(0.042)	(0.047)	(0.048)	(0.050)	(0.043)	(0.053)	(0.931)
Treatment (B)	0.439***	0.135	-0.203	-0.101	-0.152	0.245	-0.568
	(0.143)	(0.161)	(0.163)	(0.174)	(0.147)	(0.180)	(3.149)
Pr(A+B)=0	0.001	0.320	0.182	0.464	0.240	0.188	0.483
Observations	2073	1716	1721	1721	1721	1721	1702

Notes: For the outcome **Racial discrimination serious problem**, answers were given from a scale from 1: “Not a problem” at all to 5: “A very serious problem”. For the outcomes **Support preference for blacks**, **Support assistance for blacks**, and **Support name-blind recruitment**, answers were given on a scale from 1: “Strongly oppose” to 5: “Strongly support”. “Racial inequality due to effort” is people’s agreement to the following statement: “Differences in economic outcomes between whites and blacks are primarily the result of racial discrimination against blacks.” “Posterior belief” is people’s estimate of the number of times a resume with black-sounding name had to be sent to get one callback. The outcome variables are z-scored using the mean and standard deviation in the control group. “Treatment” takes value 1 if the respondent received information about the results from the correspondence study. “Prior > 15” takes value one if our respondents overestimate the extent of racial discrimination. “Republican” takes value 1 if our respondent identifies as a Republican. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

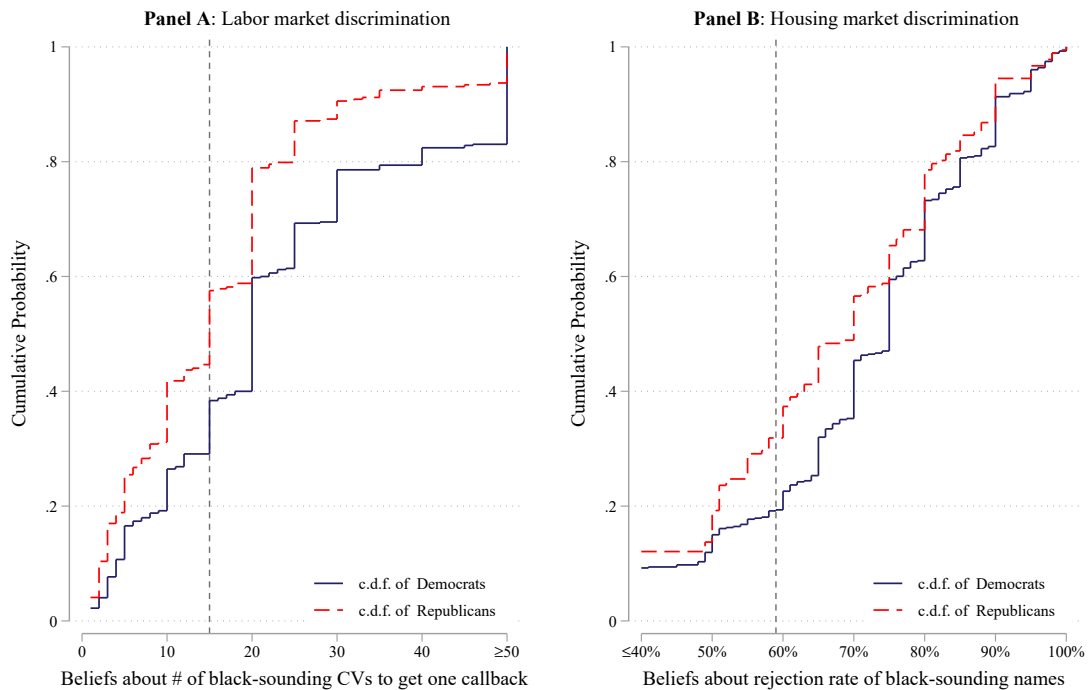
1.B Appendix figures

Figure 1.5: Belief updating in response to the research evidence



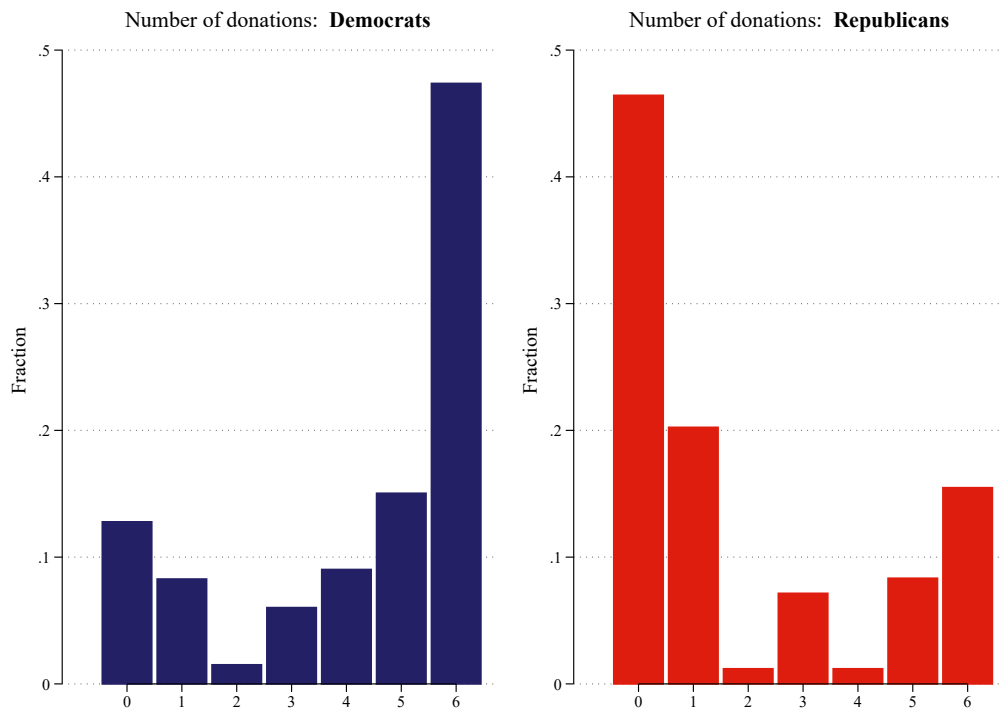
Notes: In **Panel A**, which uses data from Experiment 1 (NORC), answers are given on a scale from 0 to 100 and indicate beliefs about the acceptance rate of black candidates (higher values imply less discrimination). In **Panel B**, which uses data from Experiment 2 (Research Now), answers are given on a scale from 1 to 100 and indicate people's beliefs about the number of resumes with black-sounding resumes had to be sent to get one callback (higher values imply more discrimination). The errors bars indicate the standard error of the mean.

Figure 1.6: Republican–Democrat differences in beliefs about racial discrimination



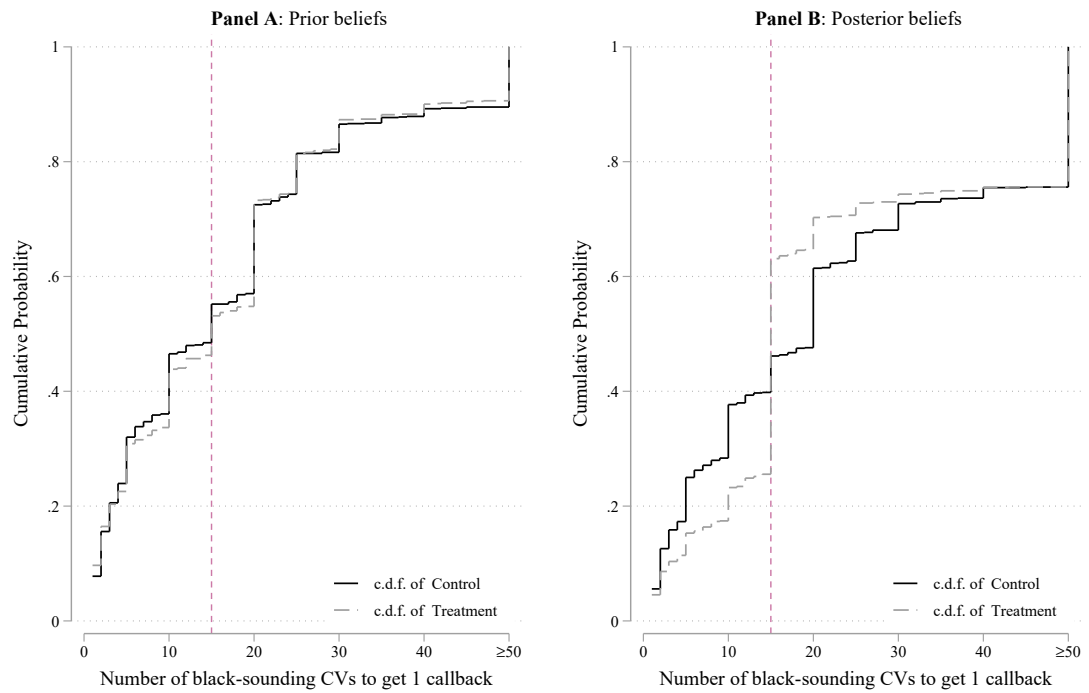
Notes: This figure uses data from Experiment 1 (the NORC sample). **Panel A** shows, separately for Republicans and Democrats, data on beliefs about how many times resumes with black-sounding names on average had to be sent out to get one callback for an interview. Respondents were informed that the corresponding number for resumes with white-sounding names was ten (as found in the study by Bertrand and Mullainathan, 2004). **Panel B** shows, separately for Republicans and Democrats, using only control group respondents, beliefs about the rejection rate on reservation requests sent from accounts with black-sounding names. Respondents were initially asked about the percent rate of acceptances of reservation requests for black-sounding names on Airbnb (true rate is 41 percent, as found in the study by Edelman et al., 2017). They were told that the corresponding number for white-sounding names was 49. We have recoded the values to implied rejection rates by subtracting each estimate from 100. In both panels, the dashed vertical lines indicate the correct answer.

Figure 1.7: Republican–Democrat differences in donations behavior



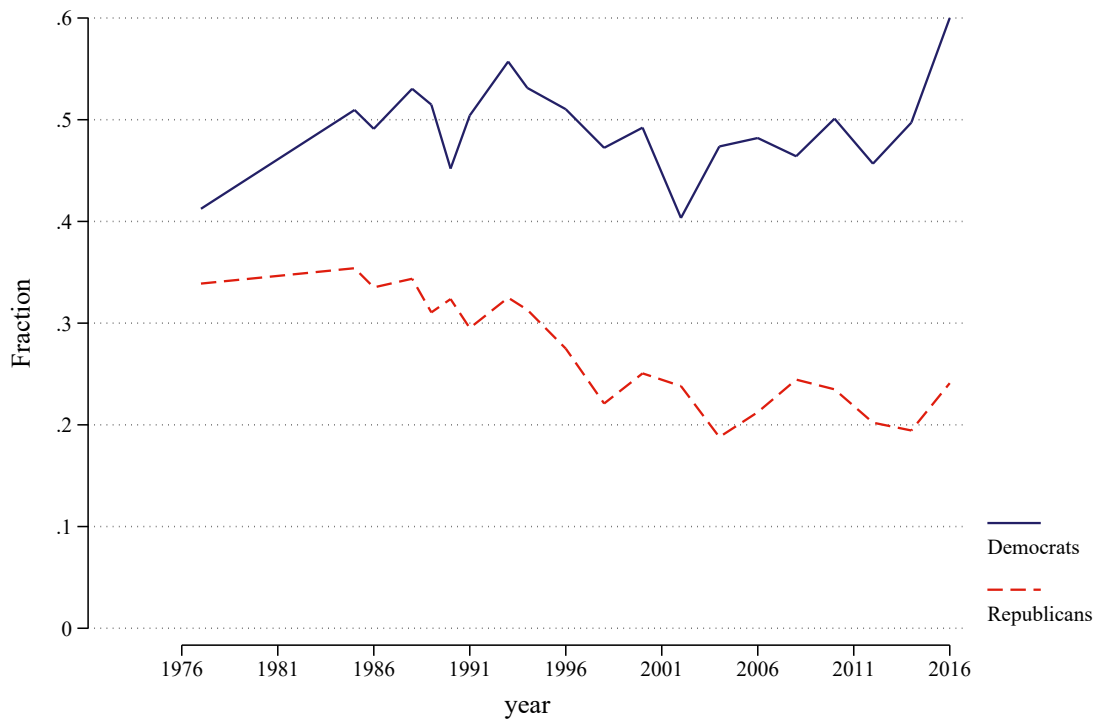
Notes: The figure, which uses data from control group respondents in Experiment 1 (NORC), shows distributions of the number of donations to the pro-black civil rights organization for self-identified Democrats and Republicans separately (the respondents were given a multiple price list where they could choose between money for themselves and \$5 to the pro-black civil rights organization in increments of \$1 from \$0 to \$5). The figure only includes respondents who completed all choices in the multiple price list.

Figure 1.8: Prior and posterior beliefs about the number of resumes sent to get one interview



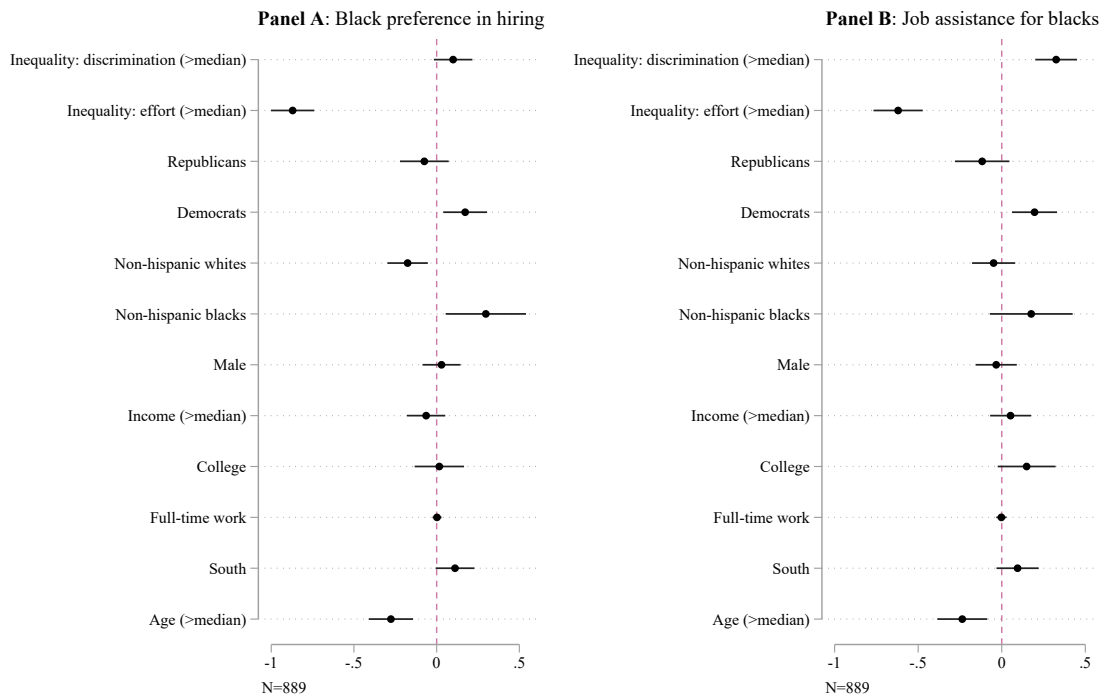
Notes: The figure uses data from Experiment 2 (Research Now). Respondents were asked how many times they thought resumes with black-sounding names on average had to be sent out to get one callback for an interview. Respondents were informed that the corresponding number for resumes with white-sounding names was ten. **Panel A** shows pre-treatment beliefs asked in wave 1 separately for the treatment and control group, whereas **Panel B** shows posterior beliefs asked in wave 2 approximately one week later. The vertical dashed line indicates the correct answer from the study by Bertrand and Mullainathan (2004).

Figure 1.9: Political polarization in beliefs about racial discrimination



Notes: The figure shows data from the General Social Survey, <http://gss.norc.org/get-the-data>. Respondents were asked whether differences the fact that blacks have “worse jobs, income, and housing than white people” is “mainly due to discrimination”; the figure shows the fraction of Democrats and Republicans who agree to this statement.

Figure 1.10: Correlates of attitudes towards pro-black policies



Notes: This figure uses data from Experiment 2 (Research Now). The dots indicate the mean values of the estimated multiple regression coefficients. The dependent variable in **Panel A** is support for giving black candidates preference over equally qualified white candidates in getting a job. The dependent variable in **Panel B** is support for giving qualified black candidates assistance in getting a job. Both outcomes are z-scored. "Inequality: discrimination" and "Inequality: effort" are agreements to the statements that differences in economic outcomes between blacks and whites are primarily the result of, respectively, "discrimination against blacks" and "whites working harder than blacks." Lines indicate 95 percent confidence intervals.

1.C Screenshots

Figure 1.11: Invitation emails sent out for the experiments with Research Now

Hi John,

You have an opportunity waiting!

Topic: Personal Opinion

Incentive: \$2.5 in e-Rewards® Currency

Length: 10 minutes

LET'S BEGIN

Figure 1.12: Consent form in wave 1 of Experiment 2 (Research Now)

This study has received ethics clearance by the Oxford University Institutional Review Board.

If subjects have questions about this study or their rights, or if they wish to lodge a complaint or concern, they may contact us at the following email: christopher.roth@economics.ox.ac.uk

Next >>

0% 100%

Consent form

- I have read the information provided on the previous page.
- I have had the opportunity to ask questions about the study.
- I understand that I may withdraw from the study at any time.
- I understand how to raise a concern or make a complaint.
- I understand that I can only participate in this experiment once.
- **I understand that close attention to the survey is required for my responses to count.**

If you are 18 years of age or older, agree with the statements above, and freely consent to participate in the study, please click on the "I Agree" button to begin the experiment.

I agree I disagree

Next >>

0% 100%

Figure 1.13: Consent form in wave 2 of Experiment 2 (Research Now)

This survey is conducted by a researcher from NHH Norwegian School of Economics.

In this survey, you will be asked questions on a broad range of different topics. Please pay close attention to all questions.

By continuing this survey, you acknowledge your consent to participate and that you are at least 18 years of age.



Chapter 2

Labor Market Concerns and Support for Immigration

Ingar K. Haaland and Christopher Roth*

Abstract

Do labor market concerns affect people's support for immigration? Using a large, representative sample of the US population, we first elicit beliefs about the labor market impact of immigration. To generate exogenous variation in beliefs, we then provide respondents in the treatment group with research evidence showing no adverse labor market impacts of immigration. Treated respondents update their beliefs and become more supportive of immigration, as measured by self-reported policy views and petition signatures. Treatment effects also persist in a follow-up study designed to mitigate experimenter demand. Our results demonstrate that labor market concerns causally affect people's support for immigration. (*JEL* C91, D83, F22, J15)

*Haaland: Department of Economics, NHH Norwegian School of Economics; Roth: Institute on Behavior & Inequality. We thank Alexander W. Cappelen, Simon Quinn, and Bertil Tungodden for extremely generous guidance. We also thank seminar audiences in Bergen (University of Bergen, NHH, and ECBE), Berlin, Essex, Lofoten, Munich, New York (NY Fed), Nottingham, Oxford, Richmond (ESA), Naples, San Diego, Stanford (SITE), Trondheim, and Warwick for valuable comments. The experiment is registered in the AEA RCT Registry as trial 2247, <https://www.socialscisceregistry.org/trials/2247>. This work was partially supported by the Research Council of Norway through its Centre of Excellence Scheme, FAIR project No 262675. IRB approval was obtained from the University of Oxford.

2.1 Introduction

There is currently a heated debate about immigration in Western countries. Although arguments about the adverse labor market impact of immigration are prominent in this debate, the current consensus in the academic literature is that labor market concerns are not an important determinant of people's attitudes towards immigration (Hainmueller and Hopkins, 2014b). In this chapter, we take a new approach to study the relevance of labor market concerns by experimentally manipulating beliefs about the labor market impact of immigration. This allows us to provide the first causal evidence of whether labor market concerns affect people's support for immigration.

In a pre-registered experiment with a large and representative sample of the US population, we first elicit beliefs about the labor market impact of the Mariel boatlift, which is known as the "one historical event that has most shaped how economists view immigration" (Clemens, 2017). During the Mariel boatlift, which was an unexpected mass immigration of Cubans to the United States, the low-skilled workforce in Miami increased by 20 percent over the course of a few months. To generate exogenous variation in beliefs, we provide a random subsample of our respondents with information about the results from a widely cited research study showing no adverse labor market impacts of the Mariel boatlift on wages and unemployment in Miami (Card, 1990). We then measure our respondents' support for immigration using both self-reported attitudes on preferred immigration levels as well as behavioral measures: two anonymous real online petitions proposing changes to the annual cap on visas for low-skilled guest workers to the US. Finally, we conduct an obfuscated follow-up study one week later in

which we hide the connection between the follow-up and the main study. The obfuscation allows us to address concerns about experimenter demand (de Quidt et al., 2018; Zizzo, 2010).

The main finding of the chapter is that beliefs about the labor market impact of immigration are an important causal driver of people's support for immigration. Treated respondents update their beliefs when provided with research evidence showing no adverse labor market impacts. This exogenous shift in beliefs leads to increased support for immigration as measured by both self-reported policy views and real online petitions. Treated respondents increase their support for admitting more low-skilled immigrants by 0.14 of a standard deviation ($p < 0.01$). This effect size corresponds to about one quarter of the Democrat–Republican difference in policy views. Treated respondents are also 69.2 percent more likely to sign a real online petition in favor of increasing the annual cap on low-skilled guest workers to the US, compared to a control group mean of 3.9 percentage points ($p < 0.01$). Moreover, we show robustness of treatment effects in the follow-up study designed to mitigate experimenter demand.

Exploring heterogeneity in treatment responses, we find that respondents with pessimistic pre-treatment beliefs about the labor market impact of the Mariel boatlift become more optimistic and vice versa. Further exploring mechanisms, we find that the treatment does not significantly affect beliefs unrelated to the labor market, such as beliefs about the cultural impact of immigration. These results corroborate our interpretation that the treatment operates through changes in beliefs about the labor market impact of immigration.

Our work contributes to several strands of the literature. First, we contribute

to the literature on labor market concerns and attitudes towards immigration (Card et al., 2012; Citrin et al., 1997; Facchini et al., 2009; Hainmueller et al., 2015; Iyengar et al., 2013; Mayda, 2006; Scheve and Slaughter, 2001). While some studies find correlations which suggest that labor market concerns influence attitudes towards immigration (Gerber et al., 2017; Mayda, 2006; Scheve and Slaughter, 2001), correlational studies are vulnerable to omitted variable bias and reverse causality (Hainmueller and Hopkins, 2014b).

To overcome the challenges with observational data, researchers have used experiments which measure support for hypothetical immigrants with randomly assigned characteristics, such as their education levels and whether they plan to find work. Such experiments can causally identify which characteristics of immigrants people value, but do not allow us to identify the underlying motivations for why certain characteristics are valued. For instance, an experiment by Hainmueller and Hopkins (2014a) finds that Americans are concerned about low-skilled immigrants who do not plan to find work. This finding could reflect economic considerations about the fiscal burden posed by these immigrants, but it could also reflect concerns about how low-skilled immigrants who do not plan to find work fit in culturally in the US.

We employ a different approach by experimentally manipulating our respondents' beliefs about the economic impact of immigration.² Since we find that our experimental manipulation does not significantly affect our respondents' beliefs about factors that are unrelated to the labor market, we are able to cleanly identify the role of labor market concerns in driving support for

²Methodologically, we relate to the literature that tries to understand the determinants of people's policy preferences by experimentally manipulating beliefs (Alan and Ertac, 2017; Alesina et al., 2018b; Cruces et al., 2013; Gilens, 2001; Karadja et al., 2017; Kuklinski et al., 2000; Kuziemko et al., 2015). Thematically, we also relate to the literature on how immigration affects voting outcomes (Dehdari, 2018; Halla et al., 2017; Mayda et al., 2018; Tabellini, 2018). Our results complement this literature by shedding light on the underlying motivations on voters.

immigration. In contrast to the current consensus, our results show that labor market concerns significantly affect people's support for immigration.³

In the context of immigration policy, our results differ from previous experiments showing muted responses of policy preferences to factual information about the fraction of immigrants (Hopkins et al., 2018; Sides and Citrin, 2007), or their characteristics (Alesina et al., 2018a; Grigorieff et al., 2018; Lergetporer et al., 2017). A potential explanation for why we find stronger responses to new information than most previous studies could be that we give information that is easy to connect with public policy. Consistent with this explanation, Facchini et al. (2017) use online experiments in Japan to show that fictitious news articles about how immigration helps solve social and economic problems, such as the ageing of the population, are effective in changing self-reported attitudes towards immigrants.

Our findings are also related to the literature on whether people are open to persuasion on political issues. Several influential studies claim that behavioral biases, such as confirmation bias, make people unwilling to revise their political beliefs in response to disconfirming information (Lord et al., 1979; Nyhan and Reifler, 2010; Taber and Lodge, 2006). Our results challenge this claim by showing that an information treatment based on research evidence can be effective in changing beliefs and policy views for Republicans and Democrats alike, even on a highly contested issue such as immigration.

More broadly, our findings contribute to a long-standing debate in the social sciences which discusses the relative importance of consequential, ideological, and social motives in driving people's political behavior (Bursztyn et

³In a review article on attitudes towards immigration, Hainmueller and Hopkins (2014b) conclude as follows: "As an explanation of mass attitudes toward immigration, the labor market competition hypothesis has repeatedly failed to find empirical support, making it something of a zombie theory."

al., 2016; DellaVigna et al., 2016; Downs, 1957; Gerber et al., 2009). Our results support a consequential view on political behavior by highlighting that changes in beliefs about the economic consequences of a policy can significantly affect political behavior.

2.2 Experimental design and sample

Our experiment has two parts: A main experiment and an obfuscated follow-up study performed seven days after the main experiment. In the following, we describe the structure of the main experiment and the obfuscated follow-up study. Figure 2.2 provides a summary of the structure.⁴

2.2.1 Main experiment

In the main experiment we first ask questions about demographics, political affiliation and self-perceived skill levels, as well as eliciting our respondents' pre-treatment beliefs about the labor market impact of immigration. We then expose half of our respondents to the information treatment. Subsequently, we measure our respondents' support for immigration using self-reported policy views and signatures on real online petitions. Finally, we elicit post-treatment beliefs about the labor market impact of immigration.

⁴Full instructions for the main experiment and the follow-up are provided in pre-analysis plan. The Qualtrics survey for the main experiment is available on the following link: https://nhh.eu.qualtrics.com/jfe/form/SV_8Am0WWUZiq4u2ax. The Qualtrics survey for the obfuscated follow-up is available on the following link: https://cessoxford.eu.qualtrics.com/jfe/form/SV_d71YFolo6Dw9Ump.

Pre-treatment beliefs about the Mariel boatlift

We first elicit our respondents' beliefs about the labor market impact of the Mariel boatlift. To familiarize our respondents with the context, we present them with the following text:

In 1980, Cuba's then President, Fidel Castro, suddenly announced that Cubans wishing to emigrate to the United States were free to do so. This led to an unexpected mass immigration to Miami, Florida, where most of the Cuban immigrants arrived by boat.

With the arrival of the new Cuban immigrants, Miami's workforce grew by 55,000, or 8 percent, almost at once. The new immigrants were mostly low-skilled, which meant that the low-skilled workforce increased by 20 percent.

The large, unexpected addition of 55,000 new immigrants to the Miami workforce has allowed researchers to study the impact of immigration on the labor market. To do so, the researchers studied wage and unemployment changes in Miami after the mass immigration relative to other US cities that, because of geographic distance, were not affected by the mass immigration of Cubans.

Thereafter, we ask our respondents how they think "the mass immigration of Cubans" affected wages and unemployment in Miami for both low- and high-skilled workers. We elicit these beliefs on 5-point Likert scales.

Research evidence of the Mariel boatlift

Following the belief elicitation, we inform respondents in the treatment group about the results from a seminal study about the labor market consequences of the Mariel boatlift (Card, 1990). Specifically, we present the following text to respondents in the treatment group (Figure 2.14 provides a screenshot):

The researchers who analyzed the short- and long-term effects of the mass immigration of Cubans to Miami concluded that, for both high-skilled and low-skilled workers, the mass immigration had **virtually no effect on wages and virtually no effect on unemployment.**

According to the researchers, the mass immigration had virtually no effect on wages and unemployment because the new Cuban immigrants increased the overall demand for goods and services, which created more jobs.

Respondents in the control group do not receive any information and proceed directly from the belief elicitation questions to the outcome questions. There are several reasons for why we chose to focus on the Mariel boatlift in our experiment. First, the Mariel boatlift boatlift has strongly shaped how economists view immigration (Clemens, 2017) and it is straightforward to explain the setting to a general audience. Second, Card (1990) found that the boatlift had no adverse labor market impacts. Since most people think that immigration has negative labor market impacts, the provision of research evidence showing no adverse labor market impacts is essential to create a strong “first stage” of changes in beliefs.⁵ While the validity of our

⁵That is, our focus is different from studies that try to correct people’s biases in beliefs

approach only depends on whether the treatment successfully changed our respondents' beliefs, we did not want to deceive respondents by giving them false information.⁶

Measuring support for immigration: self-reported policy views

To measure how the treatment affects support for immigration, we first investigate self-reported attitudes. Although we give people information about the labor market impact of low-skilled immigration, respondents could also use this information to update their beliefs about the labor market impact of high-skilled immigration. We therefore ask questions about both *low-skilled* and *high-skilled* immigrants. Furthermore, it is possible that the causal effect of beliefs on attitudes depends on the immigrants' cultural characteristics. To fix beliefs about the immigrants' cultural characteristics, we also differentiate between immigrants who are *highly familiar* and *not familiar* with American values and traditions. All respondents are asked whether the US should allow more or less immigrants to come and live in the US. We asked this question for all four types of immigrants, randomizing the order of the questions between respondents.⁷ Respondents report their answer on a 5-point Likert scale from (1) "Allow a lot less of these immigrants" to (5) "Allow a lot more of these immigrants."

about immigrants (Grigorieff et al., 2018; Hopkins et al., 2018).

⁶Although a recent paper by Borjas (2017) claims that the boatlift had negative impacts on the least skilled workers in Miami, Clemens and Hunt (2017) argue that this result was spurious.

⁷We find no evidence of any order effects.

Measuring support for immigration: political behavior

After measuring self-reported attitudes, we give our respondents the opportunity to sign real online petitions with concrete policy proposals to maximize external validity. We inform our respondents that Congress is debating whether to change the annual cap on non-agricultural guest workers to the US, the H-2B visa program. We chose to focus on the H-2B visa program because it was debated in Congress at the time of the experiment and because of the close connection between our informational treatment, which highlighted the labor market impact of low-skilled immigration, and the H-2B visa program, which is a program to bring low-skilled foreign nationals to the US.

To make sure that the debate surrounding the H-2B visa program is meaningful to the respondents, we suggest some arguments in favor of both increasing and decreasing the annual cap. Respondents are then told that they will be given the opportunity to sign one of two petitions related to this debate. The first petition suggests to increase the annual cap from 66,000 to 99,000, whereas the second petition suggests to decrease the annual cap from 66,000 to 33,000. We randomized the order of the petitions between participants.

We ask our respondents whether they want to sign one of the two petitions. Respondents who say that they want to sign one of the petitions are provided with a link to a real petition that we created on the White House web page, petitions.whitehouse.gov (Figure 2.10 provides a screenshot). To identify treatment differences in actual signatories, we provide respondents in the treatment and control group with different links to identical petitions.

Petitions on the White House web page have some noteworthy features. First, our petitions never became public and could only be reached through the links provided in our experiment. This was important to avoid contamination by people from outside the experiment. Second, the White House requires an email confirmation for petitions to count, thus making signings more costly. Third, the petition signatures were anonymous meaning that only the White House could observe the names and emails of the signatories. This anonymity mitigates concerns about experimenter demand because respondents cannot use the petitions to signal that they conform to the experimenter's wishes. Fourth, it takes several hours for the petition pages to update the number of signatures. Eventual differences in the number of signatures between the treatment and control petition page could be a confound as people may be more likely to sign a petition which already has more signatures. We measured the number of signatures over time and do not find that the treatment effects get stronger after the number of signatures on the petition pages gets updated (results available upon request).

Post-treatment beliefs about the impact of immigration

To explore mechanisms and to confirm that we successfully managed to induce exogenous variation in beliefs, we examine people's perceptions about how increasing the number of low-skilled or high-skilled immigrants to the United States would affect labor market outcomes and other theoretically relevant dimensions over the next five years. We randomize whether respondents answer the questions about low-skilled or high-skilled immigrants in order to reduce the risk of survey fatigue.

We elicit beliefs about both the impact of immigration on both the respon-

dents' *own household* and on *most Americans*. To assess whether the treatment shifted beliefs about the labor market impact of immigration, we ask respondents how they think increased immigration affects wages and job opportunities and job security. To assess whether the treatment changed beliefs not related to the labor market, we also ask how they think increased immigration affects taxes and how it affects American culture and society as a whole. We elicit responses to all of these questions on 5-point Likert scales.

2.2.2 Obfuscated follow-up study

A potential concern with the evidence from our main experiment is that treatment effects could be biased due to experimenter demand effects. While recent evidence suggests that this bias is not quantitatively important (de Quidt et al., 2018; Mummolo and Peterson, 2018), we take additional steps to address this concern by performing an obfuscated follow-up study with the same participants about one week after the main study.⁸ We had about one week between the follow-up and the main study to strike a balance between greater obfuscation and minimizing attrition.

The follow-up study is presented as an independent study to the participants. Since no treatment is administered in the follow-up study, differential experimenter demand between the treatment and control group is unlikely to be a concern unless respondents nonetheless realize that the follow-up is connected to the main study. While previous studies also have tried to hide the connection between two related surveys from respondents (e.g., Hainmueller and Hopkins, 2014a), we take additional steps to hide the connection be-

⁸The actual number of days between the main study and the follow-up study varied between one and fourteen days for all subjects. The average difference was seven days.

tween our two studies. First, we choose to collaborate with a market research company where respondents regularly receive invitations to participate in surveys. When sending out these invitations, the company uses generic invitations that only contain information about pay and expected completion time (Figure 2.11 provides a screenshot). Second, we use different consent forms for the two studies: In the first study, respondents are forwarded to a survey with a consent form from the Norwegian School of Economics, while the second study presents a consent form from the University of Oxford (Figure 2.12 and Figure 2.13 provide screenshots). We also use different layouts for the two surveys. Third, to make the follow-up seem like an independent study, we first ask respondents a series of questions about their demographics. Fourth, to further obfuscate the purpose of the follow-up study, we ask several questions about government spending, taxation, and redistribution before we ask any questions about immigration.

At the end of the follow-up study we ask three questions about immigration regarding support for low-skilled immigration, support for high-skilled immigration, and beliefs about the labor market impact of immigration. Since three questions about immigration may send a signal that we are interested in immigration, thus increasing the chance that respondents realize that the two studies are connected, we ask each question on a separate page with the most important outcome question (preference for low-skilled immigration) on the first of these three pages. To minimize the chance that respondents realize the relationship between the two studies, we use different wordings for the questions on immigration in the follow-up compared to the main study.⁹

⁹In the follow-up study, we drop the distinction about familiarity with American values and ask: "In your view, should immigration of workers with little to no education be kept at its present level, increased, or decreased?"

2.2.3 Sample

We recruited respondents using Research Now, an online market research company in the US that is regularly used by researchers to conduct academic studies (e.g., de Quidt et al., 2018). We recruited 3130 respondents who are representative of the adult US population on some important observable characteristics. All respondents who finished the main study were invited to participate in the follow-up study, for which we received 2075 respondents. The experiment was run in late May and early June 2017. We submitted a pre-analysis plan to the AEA RCT Registry prior to the data collection, specifying the sample size, empirical specifications, and our hypotheses: <https://www.socialscienceregistry.org/trials/2247>.

By construction, our sample is representative of the US population in terms of some important observable characteristics (age, region, gender, and household income; see Table 2.4 and Table 2.5). Furthermore, the treatment and control group are balanced in terms of observables both in the main study and the follow-up (Table 2.6 and Table 2.7) and there is no differential attrition in the response rates to the follow-up (Table 2.8).

2.3 Results

This section presents our main results. While our results on self-reported policy views and petition signatures were pre-specified, we also report some results that were not pre-specified, but which naturally follow from the pre-analysis plan. In Section 2.B of the appendix, we discuss a few minor deviations from the pre-analysis plan.

2.3.1 Pre-treatment beliefs about the Mariel boatlift

To aid interpretation of our main results, we first investigate which pre-treatment beliefs our respondents hold about the labor market impact of the Mariel boatlift (Figure 2.3). The large majority of our respondents think that the boatlift negatively affected wages and unemployment for low-skilled workers in Miami. By contrast, the large majority of our respondents think that the boatlift had no effect on wages and unemployment for high-skilled workers in Miami. Since most of the Cuban immigrants were low-skilled, these results suggest that our respondents believe that immigration mainly affects labor market outcomes for native workers with similar skill levels as the immigrants.

We find that our respondents' pre-treatment beliefs about the Mariel boatlift vary systematically by their background characteristics. Differences in beliefs between self-identified Republicans and Democrats are especially pronounced. We also observe significant correlations between pre-treatment beliefs and college education, race, work status, age, and income (Figure 2.4).

2.3.2 Do beliefs respond to the treatment?

To investigate whether the treatment successfully affects our respondents' beliefs about the labor market impact of immigration, we estimate the following equation using OLS:¹⁰

$$y_i = \alpha_0 + \alpha_1 T_i + \alpha_2 \mathbf{x}_i + \varepsilon_i$$

¹⁰The results are robust to employing ordered response models (results available upon request).

where y_i is the outcome of interest; T_i is an indicator for whether subject i received the research evidence; x_i is a vector of pre-specified controls¹¹; and ε_i is an individual-specific error term. We use robust standard errors for all specifications.

Table 2.1 presents regression results for post-treatment beliefs about the labor market impact of increased immigration on most Americans. In the regressions, we z-score all outcomes using the mean and standard deviation of the control group. Columns 1 to 3 show results on beliefs about the labor-market impact of low-skilled workers. The treatment increases people's optimism about the labor market impact of low-skilled immigration by 0.16 of a standard deviation ($p < 0.01$). This effect size corresponds to approximately one-half of the Democrat–Republican difference in beliefs.

Columns 4 to 6 show that the treatment also increases people's optimism about the labor market impact of immigration of high-skilled immigration by 0.23 of a standard deviation ($p < 0.01$). This effect is not significantly different from the treatment effect on beliefs about low-skilled immigrants ($p = 0.32$). The similar size of treatment effects could reflect that we gave treated respondents a reason for why the Mariel boatlift did not adversely affect labor market outcomes, namely that “the immigrants increased the overall demand for goods and services, which created more jobs,” which is not specific to low-skilled immigrants. Furthermore, there was equal scope to change people's beliefs about the labor impact of high-skilled and low-skilled immigrants: as illustrated by Figure 2.5, control group respondents are about equally pessimistic about the labor market impact of increased

¹¹The pre-specified controls include gender, age, ethnicity, region, household size, household income, education, employment status, party affiliation, whether the respondent was born in the US, whether the subject's parents were born in the US, self-perceived skill-level, and pre-treatment beliefs about the labor market impact of low-skilled (or high-skilled) immigration. The controls are coded as described in the pre-analysis plan.

high-skilled and low-skilled immigration on the US today.

Table 2.1: Beliefs about the labor market impact of immigration (post-treatment)

	Low-skilled			High-skilled			Follow-up
	Wages	Employment	Index	Wages	Employment	Index	Overall
Panel A: With controls							
Treatment	0.163 (0.046)	0.156 (0.047)	0.159 (0.042)	0.197 (0.048)	0.264 (0.048)	0.230 (0.043)	0.117 (0.040)
Observations	1474	1469	1469	1476	1470	1470	2075
Panel B: Without controls							
Treatment	0.169 (0.048)	0.164 (0.049)	0.166 (0.044)	0.194 (0.049)	0.259 (0.049)	0.228 (0.044)	0.139 (0.043)
Observations	1474	1469	1469	1476	1470	1470	2075

Notes: The table shows OLS regression results where the dependent variables are beliefs about the labor market impact of immigration. In the main study, respondents were asked how they thought admitting more low-skilled/high-skilled immigrants would affect wages and unemployment for “most Americans.” We randomized whether respondents answered these questions for low-skilled or high-skilled immigrants. In the follow-up, respondents were asked whether they thought increased immigration would hurt American workers. All questions were answered on 5-point Likert scales where higher values indicate more optimistic views on the effect of immigration. The outcome variables are z-scored using the mean and standard deviation in the control group. We include controls as described in the pre-analysis plan (also listed in Table 2.2). Robust standard errors in parentheses.

Previous literature has suggested that beliefs about the labor market impact of immigration may affect beliefs about (i) the fiscal burden of immigration and (ii) the cultural impact of immigration (e.g., because immigrants who find work are complying with American work-related norms). We find that the treatment did not significantly affect beliefs about the fiscal burden of immigration or beliefs about the cultural impact of immigration (columns 5–7 of Table 2.11).

Overall, we find a significant effect of our informational treatment on beliefs about the labor market impact of immigration. Furthermore, we do not find evidence that the treatment significantly affected beliefs not related to the

labor market. This first stage allows us to investigate whether changes in beliefs about the labor market impact of immigration have a causal impact on support for immigration.

2.3.3 Do policy preferences respond to the treatment?

Table 2.2 presents regression results for our main outcome measures on self-reported policy views. We use a pre-specified index to assess self-reported policy views on low-skilled immigration. The index is defined as the average of the standardized responses to the following two questions: support for increasing the number of low-skilled immigrants that are (i) *highly familiar* with American values and traditions and (ii) *not familiar* with American values and traditions. We use an analogous index to assess attitudes towards high-skilled immigration. Column 3 of Table 2.2 shows our first main result:

Result 1. *Beliefs about the labor market impact of immigration causally affect attitudes towards immigration. The treatment increases support for low-skilled immigration by 0.14 of a standard deviation ($p < 0.01$). This effect size corresponds to one fifth of the Democrat–Republican gap in policy preferences.*

Column 5 shows that the treatment also increases support for high-skilled immigration by 0.07 of a standard deviation ($p < 0.05$). The treatment effect on support for low-skilled immigrants is significantly larger than the effect on high-skilled immigrants ($p < 0.01$). The lower treatment effect on support for high-skilled immigration could reflect that the treatment had less scope to change attitudes towards high-skilled immigrants. As illustrated in Figure 2.5, control group respondents are about one quarter of a standard deviation more supportive of high-skilled immigration than low-skilled immigration.

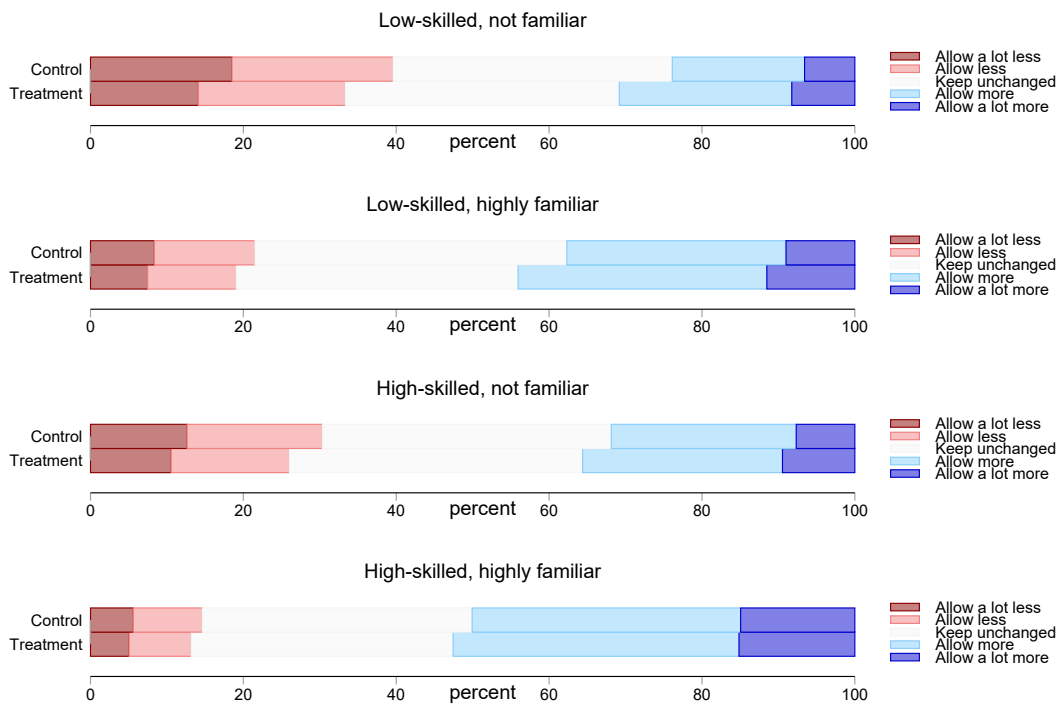
Table 2.2: Self-reported attitudes towards immigration (post-treatment)

	Low-skilled (Main Study)			High-skilled (Main Study)			Follow-up	
	Not familiar	Familiar	Index	Not familiar	Familiar	Index	Low-skilled	High-skilled
Panel A: With controls								
Treatment	0.170 (0.033) [0.001]	0.112 (0.034) [0.001]	0.141 (0.031)	0.105 (0.033) [0.003]	0.040 (0.034) [0.135]	0.072 (0.031)	0.096 (0.040)	0.064 (0.042)
Adjusted p-value								
Observations	3130	3130	3130	3130	3130	3130	2075	2075
Panel B: Without controls								
Treatment	0.168 (0.036) [0.001]	0.117 (0.036) [0.001]	0.143 (0.033)	0.108 (0.036) [0.005]	0.046 (0.035) [0.108]	0.077 (0.032)	0.108 (0.044)	0.070 (0.043)
Adjusted p-value								
Observations	3130	3130	3130	3130	3130	3130	2075	2075

Notes: The table shows OLS regression results where the dependent variables are attitudes to the different types of immigrants. The answers were given on a five point scale from 1: "Allow a lot less of these immigrants" to 5: "Allow a lot more of these immigrants." The question order was randomized (statistical tests show no order effects). The outcomes are z-scored using the mean and standard deviation in the control group. The indices are created by taking the mean of the responses to immigrants with different familiarity with American values and traditions for each skill level. Adjusted p-values are in brackets. "Treatment" is an indicator equal to 1 if respondents received the research evidence. Controls include gender, age, ethnicity, region, household size, household income, education, employment status, party affiliation, whether the respondent was born in the US, whether the subject's parents were born in the US, self-perceived skill-level, and pre-treatment beliefs about the labor market impact of low-skilled (or high-skilled) immigration and are coded as described in the pre-analysis plan. Robust standard errors in parentheses.

Does the treatment mainly increase support for immigration or mainly decrease opposition to immigration? Figure 2.1, which shows the distribution of responses for both control and treatment group respondents, highlights that the treatment makes people both less likely to support decreased immigration and more likely to support increased immigration. For instance, the treatment increases the share of respondents who say that they want to “allow more” or “allow a lot more” of low-skilled immigrants that are not familiar with American values and traditions by 29 percent and decreases the share saying that they want to “allow less” or “allow a lot less” of these immigrants by 15.7 percent (see also Figure 2.6).

Figure 2.1: Distribution of attitudes towards immigrants: Treatment vs. control



Notes: The figure shows the distribution of attitudes towards low-skilled/high-skilled immigrants that are highly familiar/not familiar with American values and traditions, disaggregated by the treatment and control group.

Interpreting effect sizes: IV What do our results imply about the quantitative importance of labor market concerns in driving support for low-skilled immigration? To answer this question, we examine how beliefs about the labor market impact of immigration affects people’s self-reported support for low-skilled immigration by using the treatment assignment as an instrument for beliefs. Specifically, we estimate the following IV regression:

$$y_i = \beta_0 + \beta_1 \widehat{\text{labor_concerns}}_i + \beta_2 \mathbf{x}_i + \varepsilon_i$$

where the first-stage equation is

$$\text{labor_concerns}_i = \gamma_0 + \gamma_1 T_i + \gamma_2 \mathbf{x}_i + \epsilon_i$$

and where y_i is support for low-skilled immigration; T_i is the treatment indicator; labor_concerns_i is an unweighted index of people’s beliefs about the effect of low-skilled immigration on (i) wages for most Americans and (ii) job opportunities or job security for most Americans; and \mathbf{x}_i is a vector of controls.

To satisfy monotonicity of the instrument, we exclude respondents who pre-treatment thought that the Mariel boatlift had either positive or no effects on wages and unemployment in Miami. The first-stage F statistic with the restricted sample is above 20, which confirms instrument relevance. The exclusion restriction—that the treatment only affects support for low-skilled immigration through changes in beliefs about its labor market impact—cannot be directly tested. However, as discussed in Section 2.3.2, we do not find evidence that the treatment significantly affected beliefs not related to the labor market.¹²

¹²The exclusion restriction also rules out that people develop more positive feelings

The instrumental variables estimates show that a one standard deviation change in beliefs about the labor market impact of low-skilled immigration translates into a 0.62 standard deviation change in support for low-skilled immigration (Table 2.9; $p < 0.01$). This demonstrates a quantitatively important role for labor market concerns in driving support for low-skilled immigration.¹³

How externally valid are our IV estimates? To shed light on this question, we compare our IV estimates to the corresponding OLS estimates in which we regress support for low-skilled immigrants on our measure of labor market concerns, labor_concerns_i using control group respondents. The OLS estimates are statistically significant in the same direction as the IV estimates, but are only about half as large in magnitude. One reason for this could be that possible confounding factors in the OLS regressions, such as measurement error in beliefs, are downward biasing the estimated relationship between labor market concerns and support for immigration. Alternatively, it could be that the IV estimates are higher because they capture the local average treatment effect for the subsample of respondents who change their beliefs about the labor market impact of immigration after receiving the research information. It is possible that these respondents have a weaker ideological stance on immigration and are therefore more willing than others to change their policy views.

towards immigrants as a result of the treatment. While we cannot test this assumption with our data, we argue that concerns about temporary changes in, e.g., empathy towards immigrants are mitigated in the follow-up study.

¹³IV results for high-skilled immigration are similar (as shown in Table 2.10). As a robustness check, Panel B of Table 2.9 shows that the IV results are barely affected when we control for post-treatment beliefs about (i) the impact of immigration on wages and employment opportunities of the respondents' own household, (ii) the tax burden created by immigrants for most Americans and the respondents' own household, and (iii) the cultural impact of immigration.

Obfuscated follow-up Column 7 of Table 2.2 shows statistically significant treatment effects on support for low-skilled immigration in the obfuscated follow-up study. The treatment effect corresponds to 0.10 of a standard deviation ($p < 0.01$). There is also some persistence of treatment effects for high-skilled immigrants (column 8), but the point estimate of 0.06 of a standard deviation is not statistically significant ($p = 0.13$). It is also not statistically different from the effect on support for low-skilled immigration ($p = 0.43$). Column 5 of Table 2.1 shows persistent treatment effects on beliefs about the overall labor market impact of immigration ($p < 0.01$).¹⁴

Since we use different questions in the follow-up and the main study, the treatment effects observed in the follow-up are not directly comparable. Nevertheless, comparing magnitudes, the effect sizes we observe in the follow-up are not significantly different from those observed in the main study ($p = 0.44$ and $p = 0.94$, respectively).¹⁵ Overall, the results from the obfuscated follow-up demonstrate that our respondents genuinely changed their attitudes towards immigration and that it is unlikely that demand effects or the increased salience of labor market concerns associated with the treatment severely bias the treatment effects observed in the main experiment.

2.3.4 Behavioral measures

To provide evidence of a more externally valid outcome measure, we analyze whether the treatment affects people's willingness to sign real online petitions regarding the cap on non-agricultural guest workers to the US, the H-2B visa

¹⁴We explored whether there was any heterogeneous treatment responses by number of days between the main study and the follow-up, but did not find any systematic differences based on this measure.

¹⁵Results on treatment effects for the sample of respondents who also complete the follow-experiment can be seen in Table 2.13.

program.

First, we analyze how the treatment affects respondents' stated willingness to sign one of the petitions. Table 2.3 presents the results.¹⁶ Column 1 shows that the treatment increases the share of respondents who say that they would sign the petition to increase the annual cap on H-2B visas by 4.8 percentage points ($p < 0.01$), which corresponds to a 16.7 percent increase from the control group mean of 28.6 percentage points. Similarly, column 2 shows that the treatment decreases the share of respondents who say that they want to sign the petition to decrease the annual gap by 6.1 percentage points ($p < 0.01$), corresponding to a 18.9 percent decrease from the control group mean of 32.2 percentage points.

Next, we investigate whether the observed changes in intentions to sign the petitions are reflected in actual petition signatures. Column 4 demonstrates that this is the case for the petition suggesting an increase in the annual cap:

Result 2. *The treatment increases the share of respondents signing the petition in favor of increasing the annual cap by 2.7 percentage points ($p < 0.01$). This corresponds to a 69.2 percent increase from the control group mean of 3.9 percentage points.*

Column 5 shows that the treatment decreases the share actually signing the petition in favor of reducing the annual cap by 0.4 percentage points, which corresponds to a 8.8 percent decrease in signatures from the control group mean of 4.5 percentage points. This difference, however, is not statistically significant from zero ($p = 0.57$).

¹⁶In this section, we focus on the results without controls since controls are not available for actual signings of the petitions. However, including controls for the intentions to sign the petitions yields identical results.

Table 2.3: Online petitions on H-2B visas (post-treatment)

	Intention: H2B Visas		Actual signatures H2B Visas	
	Increase	Decrease	Net support	Decrease
Panel A: With controls				
Treatment	0.048 (0.016)	-0.058 (0.016)	0.135 (0.034)	-
Observations	3130	3130	3130	-
Panel B: Without controls				
Treatment	0.048 (0.017)	-0.061 (0.016)	0.141 (0.036)	-0.004 (0.007)
Control Mean	0.286	0.322	-0.000	0.045
Observations	3130	3130	3130	3130

Notes: The three first columns show regression results where the dependent variable is intention to sign the petitions. "Increase" ("Decrease") is an indicator equal to 1 if a respondent wanted to sign the petition suggesting to increase (decrease) the annual cap on the H-2B visa program. "Net support" is a z-scored transformation of a variable taking value 1 (-1) if a respondent wanted to sign the petition to increase (decrease) the annual cap on the H-2B visa program and 0 otherwise. The two last columns show actual signatures. Since we only observe actual signatures on the treatment group level, we cannot include controls and run regressions for these outcomes. To do testing, we calculate standard errors using the standard formula for proportion tests. "Treatment" is an indicator equal to 1 if respondents received information about the labor market impact of the Mariel boatlift. We use the same controls as in Table 2.2. For the questions on intention to sign the petitions, we apply robust standard errors in parentheses.

2.3.5 Heterogeneous treatment effects

We hypothesized in the pre-analysis plan that treatment responses would depend on people's pre-treatment beliefs about the labor market impact of the Mariel boatlift.

Post-treatment beliefs We find negative and significant interaction effects of the treatment and pre-treatment beliefs about how the Mariel boatlift affected low-skilled workers on people's post-treatment beliefs about how most Americans are affected by low-skilled immigration (Panel A of Table 2.16). We also observe negative interaction effects between the treatment and pre-treatment beliefs on post-treatment beliefs about the effect of immigration on people's own household, but these effects are not statistically significant.¹⁷

While we document significant treatment heterogeneity by pre-treatment beliefs about how the Mariel boatlift affected low-skilled workers, we do not find any evidence of treatment heterogeneity by pre-treatment beliefs about how the Mariel boatlift affected high-skilled workers. This could reflect the fact that the large majority of our respondents thought that the Mariel boatlift had no impact on high-skilled workers.

¹⁷One concern is that heterogeneous treatment effects by pre-treatment beliefs may be driven by correlations between pre-treatment beliefs and other characteristics, such as political views, race, work status, and income. We therefore decompose the total variation in pre-treatment beliefs into a component predicted by the pre-specified observables we use as control variables throughout the chapter, and the residual component of pre-treatment beliefs that is not explained by these observables. Reassuringly, we find very similar results using the variation in pre-treatment beliefs that is not explained by our pre-specified covariates. By contrast, the variation in pre-treatment beliefs explained by our pre-specified covariates does not predict heterogeneous responses (results available upon request). This suggests that the observed interaction effects between the treatment and pre-treatment beliefs are indeed driven by genuine changes in beliefs.

Policy preferences We find a negative interaction effect between the treatment and pre-treatment beliefs about how the Mariel boatlift affected low-skilled workers on support for low-skilled immigration, but the estimated coefficient is only marginally significant (Panel A of Table 2.17). That we find a less strong interaction effect for policy preferences than for beliefs could reflect a lack of statistical power as we observe smaller average treatment effects on policy preferences than on beliefs. As Panel A of Figure 2.8 shows, we find large and significant treatment effects conditional on having pessimistic pre-treatment beliefs, and imprecisely estimated null effects conditional on having optimistic pre-treatment beliefs. On support for high-skilled immigration, we do not observe any significant treatment heterogeneity by pre-treatment beliefs. However, statistical power is lower since the average treatment effect on support for high-skilled immigration was significantly lower than on support for low-skilled immigration.

Heterogeneity by political affiliation and skill-level We also pre-specified to examine heterogeneous responses on self-reported policy views by people's political affiliation and self-perceived skill levels. We find no evidence of heterogeneity based on self-identifying as Republican (Panel B of Table 2.17). This result could reflect different mechanisms going in opposite directions. While Republicans have more pessimistic pre-treatment beliefs than non-Republicans—suggesting there is more scope to change their beliefs with the research evidence—they may also be more likely to engage in partisan motivated reasoning to dismiss the research evidence. We also find no evidence of heterogeneity based on people's self-perceived skill level (Panel C of Table 2.17). The lack of heterogeneity by skill level is consistent with people caring more about how immigration affects most Americans than

their own household.

Machine-learning approaches to heterogeneity The selection of pre-specified covariates for the heterogeneity analysis in the previous section was motivated by theoretical considerations. As a supplement to this analysis, we employ Classification and Regression Trees (CART). This machine-learning method allows us to consider all covariates in our data set and select those which predict maximum differences in the magnitude of treatment effects for different sub-populations, while simultaneously controlling for multiple hypothesis testing (Athey and Imbens, 2016). Specifically, the algorithm constructs a tree by sequentially dividing the data set into two groups with the target of minimizing the mean squared error of the estimated treatment effects. To deal with over-fitting, the algorithm uses a cross-validation set to reduce the depth of the tree.¹⁸

Figure 2.9 presents the trees obtained by the CART algorithm for the outcome variables of interest. People's pre-treatment beliefs about the wage impact of the Mariel boatlift, or their confidence in these beliefs, appear at the top of all of the trees presented in the figure. That is, the CART algorithm confirms that pre-treatment beliefs about the labor market impact of the Mariel boatlift are most powerful at predicting differences in responses to the information treatment both in terms of policy views on immigration and post-treatment beliefs about the labor market impact of immigration. Proxies for economic status (such as income and employment status) and ethnicity appear in the lower end of the trees, suggesting that economic and cultural considerations

¹⁸We specify the bin size of the final nodes to be at least 200 and use 100 cross-validation sets for the out-of-sample predictions; i.e., we randomly partition the data into equally-sized sub-samples. One of the folds is used for evaluation, while the remaining folds are utilized for fitting the tree.

also shape people’s responsiveness to the information treatment. Overall, we find it encouraging that the data-driven machine learning approach singles out pre-treatment beliefs about the Mariel boatlift when assessing which factors shape responses to the information.

2.3.6 Discussion

Our causal estimates demonstrate that labor market concerns are a quantitatively important driver of attitudes towards immigration. Do labor market concerns mostly operate through self-interested concerns about own labor market outcomes or through concerns about how immigration affects the national labor market? While our treatment changed beliefs about both dimensions, the lack of heterogeneous treatment effects by self-perceived skill levels documented in Section 2.3.5 may suggest that the treatment mainly operates through concerns about how immigration affects the national labor market.

To test whether this explanation is consistent with correlational evidence, we run regressions with control group respondents simultaneously including beliefs about the labor market impact of increased immigration on “most Americans” as well on the respondents’ own household. We find that beliefs about the labor market impact on “most Americans” are stronger predictors of support for immigration than beliefs about the labor market impact on people’s own household (Table 2.14). While a one standard deviation change in beliefs about the wage impact of immigration on most Americans is associated with a 0.12 to 0.21 standard deviation change in attitudes towards different types of immigrants, the corresponding point estimates for beliefs about the wage impact on people’s own household are either zero or negative.

The same pattern holds after controlling for beliefs about the cultural impact of immigration.¹⁹ These estimates suggest an important role for nationwide labor market concerns in the formation of immigration attitudes.

This finding may explain why our main results differ from the current consensus that labor market concerns are not a quantitatively important driver of attitudes towards immigration (Hainmueller and Hopkins, 2014b). This consensus is formed by studies showing that people’s policy preferences on immigration vary little with their own labor market position. While this suggests that self-interested labor market concerns are not a quantitatively important driver of attitudes towards immigration, it does not rule out an important role for labor market concerns about the nationwide labor market effect of immigration.

2.4 Concluding remarks

In this chapter, we present evidence that labor market concerns are an important causal driver of public support for immigration. An exogenous shift in beliefs about the labor market impact of immigration affects people’s support for immigration both in terms of self-reported attitudes and signatures on real online petitions. Immigration is now said to rival economics as “the driving force in Western politics,” and may continue to dominate the political discussion for decades to come.²⁰ While natural experiments studying the impact of immigration on voting outcomes cannot identify the underlying motivations of voters, our findings suggest that labor market concerns may

¹⁹We also replicate these correlational patterns using data from a probability-based sample of the US population (see Table 2.15).

²⁰Rachman, Gideon. “Migration will drive western politics for decades to come,” *Financial Times*, May 8, 2018. <https://www.ft.com/content/7f4c6222-4f94-11e8-9471-a083af05aea7>

be an important mechanism at play.

We believe that our approach of using the results from natural experiments to measure and shock people's beliefs about the expected costs and benefits of economic policies could be applied in many different settings. A large literature has investigated whether people hold accurate beliefs about policy-relevant facts. The consensus from this literature is that people suffer from widespread political misperceptions, which undermine their ability to form meaningful opinions (Flynn et al., 2017). However, to form meaningful opinions about topics such as immigration, taxation, and monetary policy, it is arguably equally important for voters to engage in counterfactual policy analysis. We think that a promising avenue for future research is to assess the economic competence of voters by investigating two related questions: (i) which mental models people use to assess the economic implications of different policies and (ii) how people use new evidence to update their mental models of the economy.

Summary of the Appendices

Section 2.A.1 provides additional figures. Figure 2.2 shows an overview of the structure of the experiment. Figure 2.3 shows the distribution of the pre-treatment beliefs our respondents hold about the labor market impact of the Mariel boatlift. Figure 2.4 displays the correlates of demographics with people's pre-treatment beliefs about the wage and employment impact of the Mariel boatlift. Figure 2.5 displays the distribution of beliefs about the labor market impact of immigration on most Americans versus own household. Figure 2.6 shows treatment effects in absolute and relative changes. Figure 2.7 shows heterogeneous updating of beliefs about the labor market effects of immigrants. Figure 2.8 shows heterogeneous treatment effects on support for low-skilled immigration by people's pre-treatment beliefs about the Mariel boatlift. Figure 2.9 shows the causal trees obtained by the CART algorithm.

Section 2.A.2 provides evidence of covariate balance and describes the sample. Tables 2.4 and 2.5 provide descriptive statistics. Table 2.6 shows covariate balance for the main sample, while Table 2.7 provides evidence of covariate balance for the sample in the follow-up study. Table 2.8 examines correlates of attrition in the follow-up study.

Table 2.9 and Table 2.10 show IV estimates on people's support for low-skilled and high-skilled immigration, respectively.

Section 2.A.3 provides descriptives and further evidence of the mechanisms. Table 2.11 summarizes treatment effects on people's beliefs about how their own household's and most American's wages, employment, tax burden and American culture are affected by low-skilled immigration. Table 2.12

shows the main outcome questions for people with pessimistic pre-treatment beliefs. Table 2.13 shows the main results on policy preferences restricting the sample to those who also completed the follow-up survey. Tables 2.14 and 2.15 examine the relative importance of self-interested vs. group-level concerns in shaping immigration preferences.

Section 2.A.4 examines heterogeneity in treatment effects. Table 2.16 examines heterogeneous treatment effects on people's beliefs about the labor market impact of immigration. Table 2.17 and Table 2.18 examine heterogeneity for our main self-reported outcome questions by people's self-perceived skill level, their political affiliation, and their pre-treatment belief about the effect of the Mariel boatlift. Table 2.19 examines heterogeneous treatment effects along these dimensions on people's intention to sign the petition. Section 2.B lists some minor deviations from the pre-analysis plans.

Section 2.C provides screenshots of the online petition, the invitation email for the follow-up study, and the consent forms in the main study and the obfuscated follow-up study.

2.A Additional figures and tables

2.A.1 Additional figures

Figure 2.2: Overview of the experiment

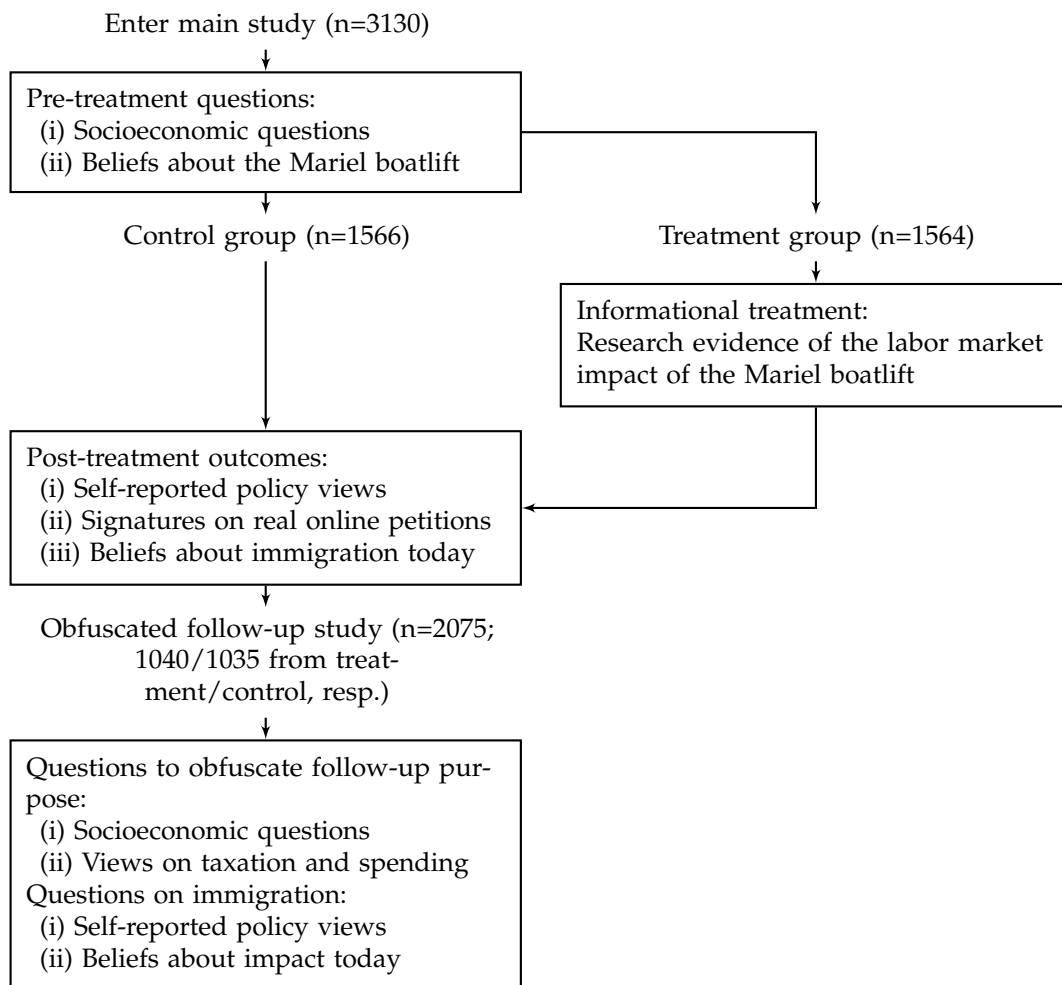
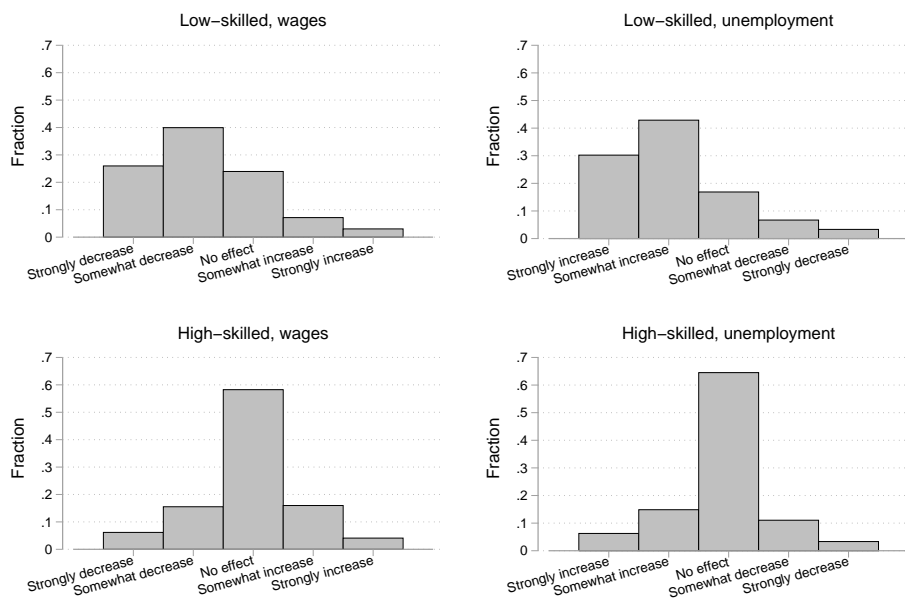
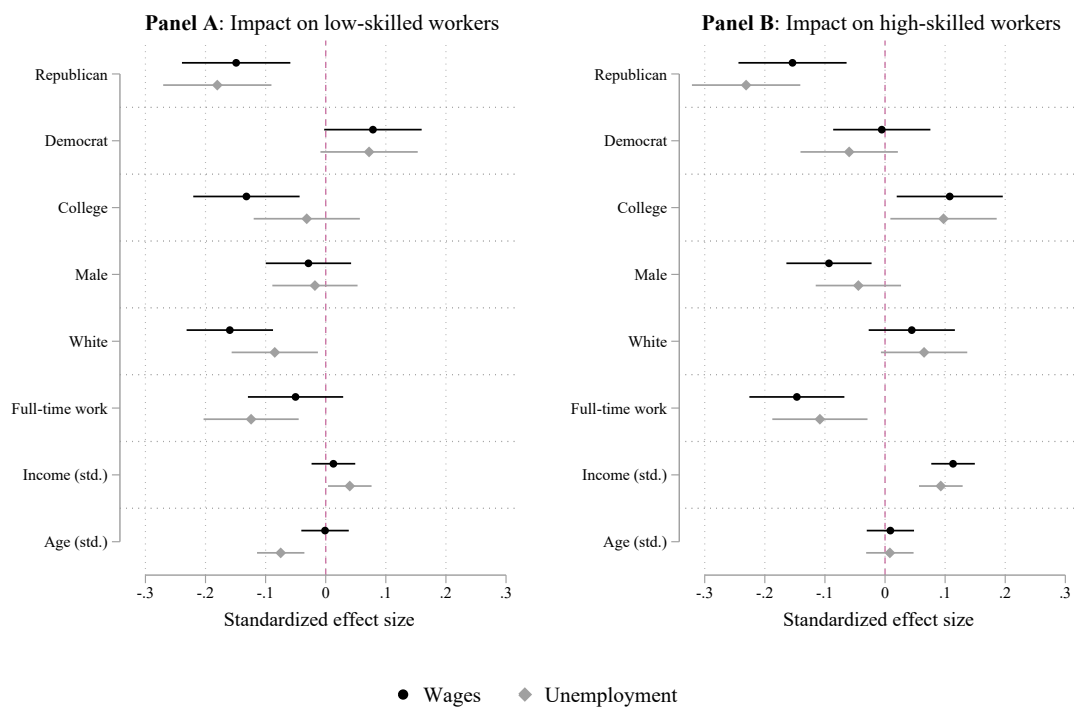


Figure 2.3: Pre-treatment beliefs about the labor market impacts of the Mariel boatlift



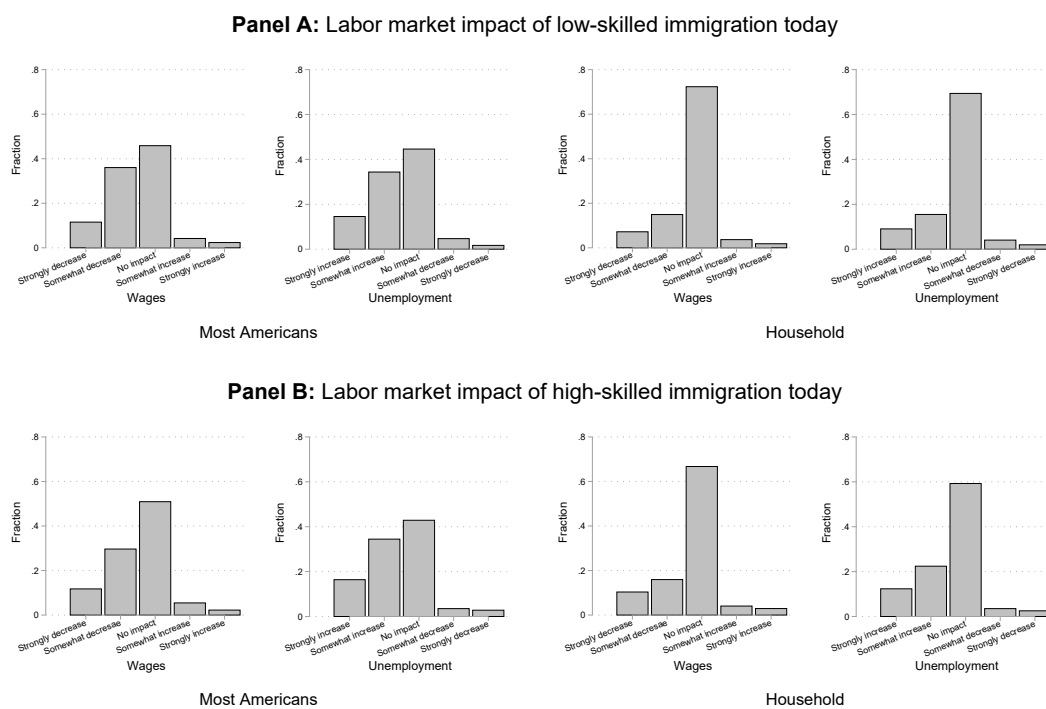
Notes: The figure shows the distribution of pre-treatment beliefs about the impact of the Mariel boatlift on wages (left panels) and unemployment (right panels) for low-skilled workers (top panels) and high-skilled workers (bottom panels). Respondents are asked two questions on wages: “In the five-year period after 1980, how do you think wages of low-skilled (high-skilled) workers in Miami were affected by the mass immigration of Cubans?” The responses are on a five-point scale ranging from (1) Strongly decrease to (5) Strongly increase. They are also asked two questions about unemployment: “In the five-year period after 1980, how do you think unemployment among low-skilled (high-skilled) workers in Miami was affected by the mass immigration of Cubans?” The responses are on a five-point scale ranging from (1) Strongly increase to (5) Strongly decrease.

Figure 2.4: Correlates of pre-treatment beliefs about the Mariel boatlift



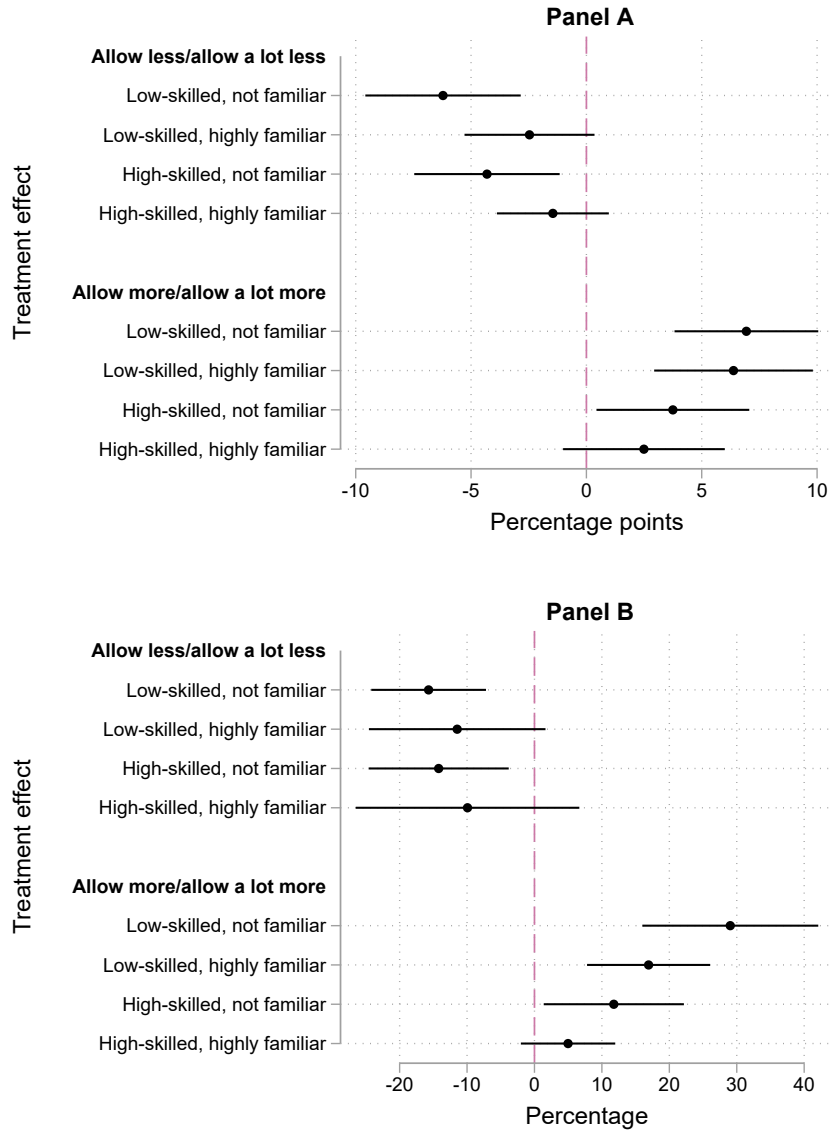
Notes: The dots indicate the mean values of the estimated multiple regression coefficients. The lines indicate 95 percent confidence interval of the mean. In **Panel A**, the outcome variables are people's beliefs about the labor market impact of the Mariel boatlift on low-skilled workers; in **Panel B**, the outcome variables are beliefs about the labor market impact on high-skilled workers.

Figure 2.5: Beliefs about the labor market impact of immigration: Impact on “most Americans” versus own household



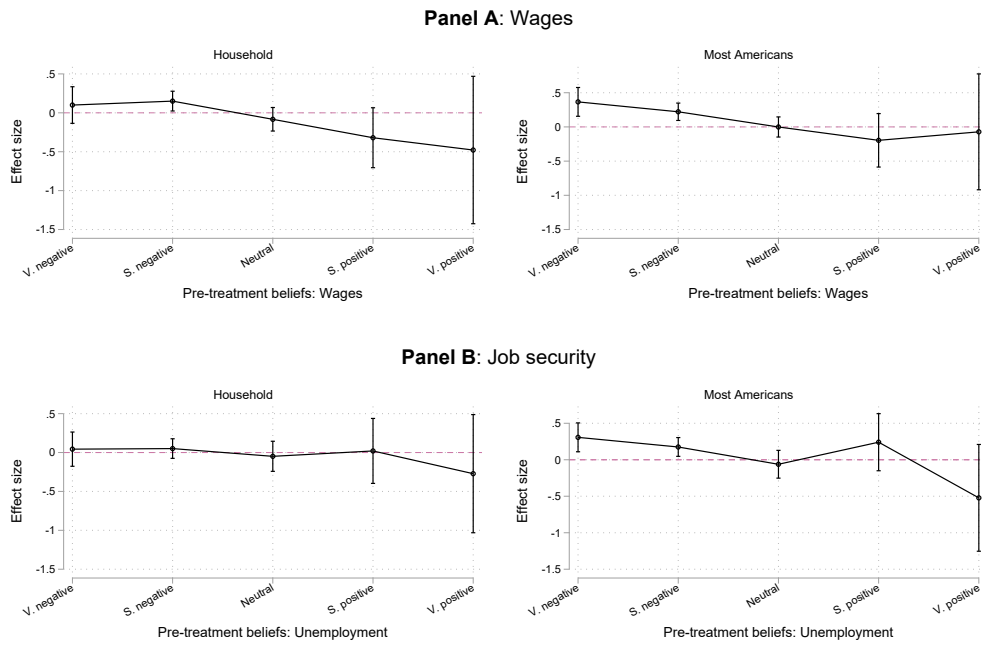
Notes: **Panel A** shows the distribution of beliefs about the labor market impact of low-skilled immigrants on *most Americans* as well as on the respondents’ own household. **Panel B** shows the corresponding distributions for beliefs about high-skilled immigrants. Both panels only include responses from respondents in the control group.

Figure 2.6: Treatment effects on attitudes towards immigrants



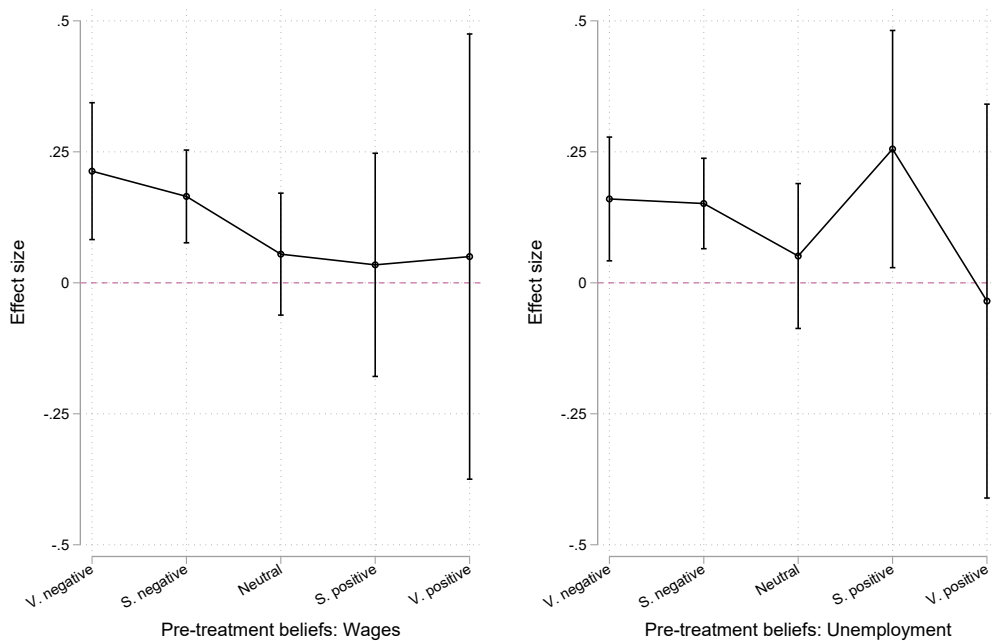
Notes: The figure shows treatment effects on the fraction of respondents who answer that they want to allow more/allow a lot more and allow less/a lot less of low-skilled/high-skilled immigrants that are highly familiar/not familiar with American values and traditions. **Panel A** shows the treatment effects in percentage point changes. **Panel B** shows the treatment effects in percent changes. Lines indicate 95 percent confidence intervals.

Figure 2.7: Heterogeneity in belief updating: Labor market impact of low-skilled immigration



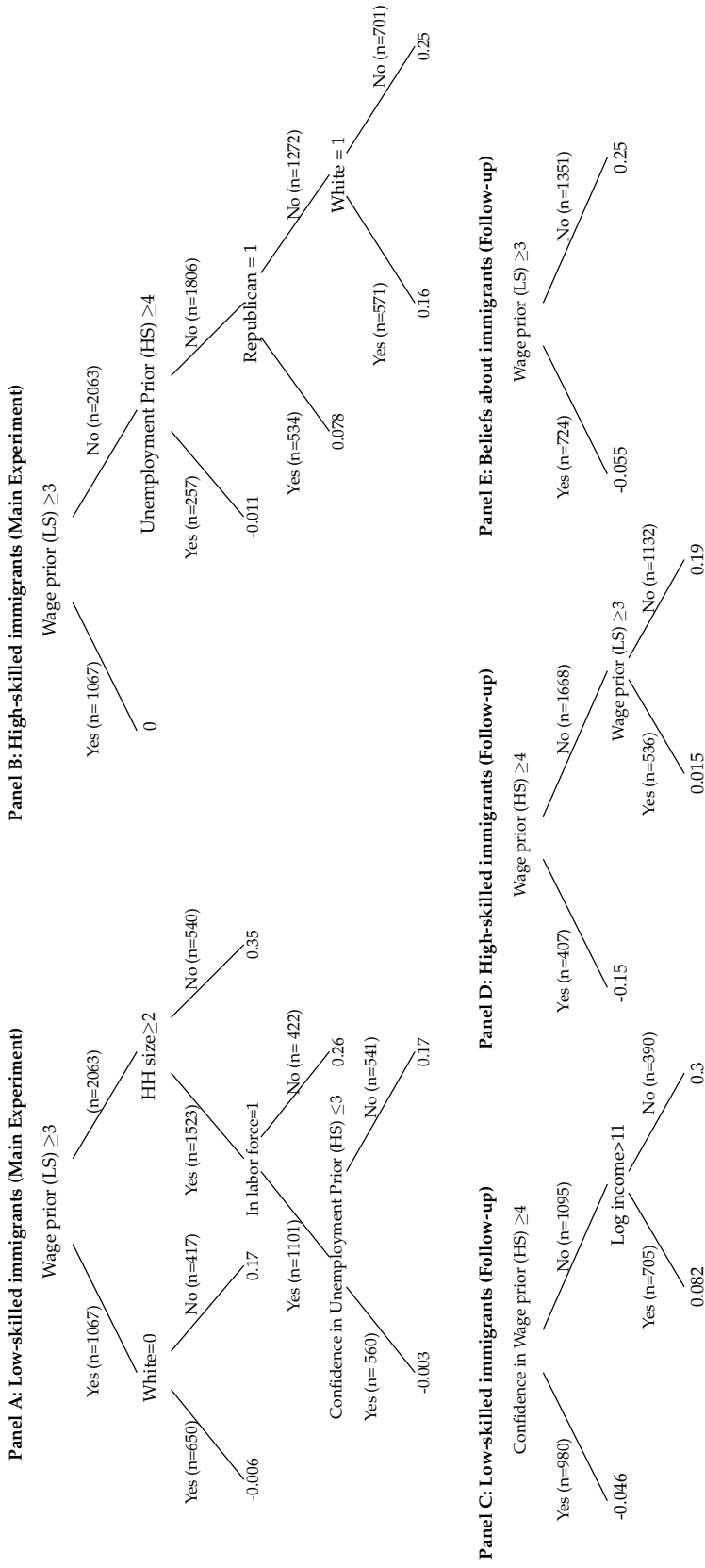
Notes: The figure shows standardized treatment effects on beliefs about the wage and unemployment impact of low-skilled immigration by their pre-treatment beliefs about the wage and unemployment impact of the Mariel boatlift. The outcomes are based on the follow questions: how do you think admitting more low-skilled immigrants would affect (i) “wages,” and (ii) “job opportunities and job security” for their own household as well as for most Americans. The regressions include pre-specified controls (listed in Table 2.2). 95 percent confidence intervals are indicated.

Figure 2.8: Support for low-skilled immigration, treatment heterogeneity by pre-treatment beliefs



Notes: The figure shows heterogeneous treatment effects on support for low-skilled immigration based on people’s pre-treatment beliefs about the low-skilled wage and unemployment impact of the Mariel boatlift. The regressions include pre-specified controls (listed in Table 2.2). 95 percent confidence intervals are indicated.

Figure 2.9: Heterogeneous treatment effects: Machine Learning Classification and Regression Trees



Notes: This figure displays causal trees using Classification and Regression Trees (CART; Athey and Imbens, 2016). “Wage prior (LS)” measures people’s beliefs about the impact of the Mariel boatlift on wages of low-skilled workers in Miami. “Wage prior (HS)” measures people’s beliefs about the impact of the Mariel boatlift on wages of high-skilled workers in Miami. The responses for these variables are on a five-point scale ranging from (1) Strongly decrease to (5) Strongly increase. “Unemployment prior (HS)” measures people’s beliefs about the impact of the Mariel boatlift on unemployment of high-skilled workers in Miami. The responses for these variables are on a five-point scale ranging from (1) Strongly increase to (5) Strongly decrease. “Confidence in Wage prior (HS)” measures people’s confidence in their prior beliefs about the wage effects of the Mariel boatlift on high-skilled workers. Responses are measured on a five-point scale ranging from (1) Very unsure to (5) Very sure. “Confidence in Unemployment prior (HS)” measures people’s confidence in their prior beliefs about the employment effects of the Mariel boatlift. Responses are measured on a five-point scale ranging from (1) Very unsure to (5) Very sure. White takes value one for respondents identifying as Whites, and zero otherwise. “HH size” denotes people’s household size. “In labor force” takes value one for respondents who self-report to be in the labor force. Log income denotes the log of respondents’ self-reported household income in US dollars (log income of 11 corresponds to an annual household income of approximately 60,000 USD). The numbers at the final nodes denote the z-scored treatment effects for the different subgroups defined by the causal tree. We include all variables displayed in the balance Table 2.6 in the machine learning algorithm, and we set the minimum number of the final node as 200 and the number of cross validation sets as 100.

2.A.2 Summary statistics, balance and attrition

Table 2.4: Summary statistics

	Mean	SD	Median	Min.	Max.	Obs.
Gender	0.48	0.50	0.00	0.00	1.00	3130
What is your age?	46.64	15.59	49.50	21.00	69.50	3130
Non-Hispanic White	0.46	0.50	0.00	0.00	1.00	3130
African American/Black	0.06	0.23	0.00	0.00	1.00	3130
Republican	0.25	0.43	0.00	0.00	1.00	3130
Democrat	0.37	0.48	0.00	0.00	1.00	3130
Independent	0.36	0.48	0.00	0.00	1.00	3130
Northeast	0.22	0.41	0.00	0.00	1.00	3130
Midwest	0.18	0.39	0.00	0.00	1.00	3130
West	0.24	0.43	0.00	0.00	1.00	3130
Household size	2.36	1.49	2.00	0.00	10.00	3130
Total household income	72871.41	50712.13	62500.00	7500.00	225000.00	3130
Education	4.89	2.11	5.00	0.00	9.00	3130
Self-perceived skill level: High-skilled	0.75	0.43	1.00	0.00	1.00	3130
Full-time employee	0.45	0.50	0.00	0.00	1.00	3130
Part-time employee	0.08	0.27	0.00	0.00	1.00	3130
Self-employed or small business owner	0.06	0.23	0.00	0.00	1.00	3130
Unemployed and looking for work	0.05	0.21	0.00	0.00	1.00	3130
Student	0.03	0.16	0.00	0.00	1.00	3130
Not in labor force	0.26	0.44	0.00	0.00	1.00	3130
College	0.78	0.42	1.00	0.00	1.00	3130
Beliefs about effect of immigration on wages of low-skilled workers	2.21	1.00	2.00	1.00	5.00	3130
Beliefs about effect of immigration on wages of high-skilled workers	2.96	0.85	3.00	1.00	5.00	3130
Beliefs about effect of immigration on unemployment of low-skilled workers	2.10	1.01	2.00	1.00	5.00	3130
Beliefs about effect of immigration on unemployment of high-skilled workers	2.90	0.80	3.00	1.00	5.00	3130
Confidence in beliefs about wages of low-skilled workers	3.49	0.97	3.00	1.00	5.00	3130
Confidence in beliefs about unemployment of low-skilled workers	3.55	0.97	4.00	1.00	5.00	3130
Confidence in beliefs about wages of high-skilled workers	3.52	0.97	3.00	1.00	5.00	3130
Confidence in beliefs about unemployment of high-skilled workers	3.50	0.96	3.00	1.00	5.00	3130

Notes: This table displays the summary statistics for our sample.

Table 2.5: Characteristics of our sample compared to the US Census

	Mean: Online sample	Mean: Online sample – follow-up	Mean: ACS
Male	0.48	0.50	0.51
What is your age?	46.6	47.4	47.1
Northeast	0.22	0.23	0.179
Midwest	0.18	0.19	0.211
West	0.24	0.23	0.24
Total household income	72871	73499	82433

Notes: This table summarizes the characteristics of our sample in the main survey as well as the follow-up survey along targeted dimensions as well as the characteristics of the 2015 American Community Survey.

Table 2.6: Balance across the treatment and control group

	Treatment	Control	P-value(Treatment - Control)	Observations
Gender	0.48	0.48	0.994	3130
What is your age?	45.82	46.54	0.211	3130
Non-Hispanic White	0.48	0.45	0.184	3130
African American/Black	0.05	0.06	0.101	3130
Republican	0.26	0.24	0.452	3130
Democrat	0.37	0.38	0.392	3130
Independent	0.36	0.36	0.919	3130
Northeast	0.23	0.22	0.390	3130
Midwest	0.18	0.18	0.968	3130
West	0.23	0.25	0.302	3130
Household size	2.53	2.59	0.245	3130
Log household income	10.91	10.91	0.936	3130
Self-perceived skill level: High-skilled	0.76	0.75	0.681	3130
Full-time employee	0.49	0.48	0.530	3130
Part-time employee	0.09	0.08	0.324	3130
Self-employed or small business owner	0.06	0.06	0.295	3130
Unemployed and looking for work	0.05	0.05	0.523	3130
Student	0.03	0.03	0.364	3130
Not in labor force	0.27	0.29	0.124	3130
College	0.85	0.83	0.081	3130
Beliefs about effect of immigration on wages of low-skilled workers	2.20	2.23	0.493	3130
Beliefs about effect of immigration on wages of high-skilled workers	2.96	2.97	0.673	3130
Beliefs about effect of immigration on unemployment of low-skilled workers	2.12	2.08	0.301	3130
Beliefs about effect of immigration on unemployment of high-skilled workers	2.90	2.91	0.890	3130
Confidence in beliefs about wages of low-skilled workers	3.43	3.52	0.024	3130
Confidence in beliefs about unemployment of low-skilled workers	3.50	3.56	0.099	3130
Confidence in beliefs about wages of high-skilled workers	3.49	3.50	0.657	3130
Confidence in beliefs about unemployment of high-skilled workers	3.44	3.51	0.058	3130

Notes: This table examines covariate balance based on the sample of respondents from the main study. The p-value of an F-test testing for the joint significance of all covariates in predicting treatment status is 0.2302.

Table 2.7: Balance across the treatment and control group in the follow-up

	Treatment	Control	P-value(Treatment - Control)	Observations
Gender	0.49	0.50	0.809	3130
What is your age?	46.92	47.97	0.124	3130
Non-Hispanic White	0.48	0.49	0.675	3130
African American/Black	0.05	0.07	0.017	3130
Republican	0.26	0.26	0.891	3130
Democrat	0.37	0.38	0.816	3130
Independent	0.36	0.35	0.791	3130
Northeast	0.23	0.23	0.952	3130
Midwest	0.18	0.20	0.118	3130
West	0.24	0.23	0.462	3130
Household size	2.43	2.48	0.408	3130
Log household income	10.93	10.92	0.760	3130
Self-perceived skill level: High-skilled	0.75	0.74	0.734	3130
Full-time employee	0.48	0.45	0.143	3130
Part-time employee	0.09	0.08	0.323	3130
Self-employed or small business owner	0.05	0.06	0.154	3130
Unemployed and looking for work	0.05	0.05	0.980	3130
Student	0.02	0.02	0.442	3130
Not in labor force	0.29	0.33	0.055	3130
College	0.83	0.83	0.855	3130
Beliefs about effect of immigration on wages of low-skilled workers	2.23	2.24	0.729	3130
Beliefs about effect of immigration on wages of high-skilled workers	2.95	2.95	0.974	3130
Beliefs about effect of immigration on unemployment of low-skilled workers	2.12	2.10	0.522	3130
Beliefs about effect of immigration on unemployment of high-skilled workers	2.91	2.91	0.968	3130
Confidence in beliefs about wages of low-skilled workers	3.42	3.51	0.043	3130
Confidence in beliefs about unemployment of low-skilled workers	3.50	3.55	0.212	3130
Confidence in beliefs about wages of high-skilled workers	3.48	3.50	0.579	3130
Confidence in beliefs about unemployment of high-skilled workers	3.45	3.52	0.077	3130

Notes: This table examines covariate balance for the follow-up sample. The p-value of an F-test testing for the joint significance of all covariates in predicting treatment status is 0.4428.

Table 2.8: Attrition in the follow-up study

	Completed Follow-up Survey	Completed Follow-up Survey
Treatment	-0.002 (0.017)	-0.004 (0.016)
Republican		0.008 (0.021)
Independent		-0.026 (0.019)
Log(Income)		-0.020 (0.011)
High-skilled		-0.114 (0.020)
Employed Full-Time		-0.010 (0.023)
Employed Part-Time		0.005 (0.035)
Unemployed		0.044 (0.043)
Self-Employed		-0.043 (0.039)
Student		-0.096 (0.058)
High Education		-0.045 (0.032)
Male		0.042 (0.017)
Age		0.002 (0.001)
Observations	3130	3130

Notes: The outcome variables take value 1 for respondents who completed the follow-up study. "Treatment" is an indicator equal to 1 if respondents received the research evidence. Robust standard errors in parentheses.

Table 2.9: The causal effect of labor market concerns on support for low-skilled immigrants: Subsample of respondents with pessimistic pre-treatment beliefs

	Labor Market Concerns	Support	Support	
	OLS (reduced form)	OLS (reduced form)	IV	OLS
Panel A: Controls				
Treatment	0.222 (0.043)	0.148 (0.033)		
Labor Market Concerns			0.622 (0.212)	0.318 (0.042)
Observations	1218	2599	1218	624
Panel B: Additional controls				
Treatment	0.185 (0.033)	0.125 (0.044)		
Labor Market Concerns			0.677 (0.249)	0.282 (0.052)
Observations	1203	1203	1203	613

Notes: These regressions are based on the subsample of respondents who thought that the Mariel boatlift had either negative or no effects on wages and unemployment. Column 4 only uses control group respondents, while Columns 1 to 3 use both treatment and control group respondents. The dependent variable in columns 2 to 4 is an index for attitudes towards low-skilled immigrants. The dependent variable in column 1 is “Labor Market Concerns” which is an unweighted index of people’s beliefs about the effect of low-skilled immigration on (i) “wages for most Americans” and (ii) “job opportunities or job security for most Americans.” In Column 3 we instrument “Labor Market Concerns” with the treatment indicator. In Columns 1, 2, and 4, we run OLS regressions. We randomized whether people were asked about the impact of low-skilled or high-skilled immigrants. All variables have been standardized. All regressions include the controls described in Table 2.2. Panel B additionally controls for people’s beliefs about the impact of immigration on wages and employment of the own household, as well as beliefs about the tax burden created by immigrants for most Americans and the own household, as well as beliefs about the cultural impact of immigration. Robust standard errors in parentheses.

Table 2.10: The causal effect of labor market concerns on support for high-skilled immigrants: Subsample of respondents with pessimistic priors

	Labor Market Concerns	Support	Support	
	OLS (reduced form)	OLS (reduced form)	IV	OLS
Panel A: Controls				
Treatment	0.282 (0.044)	0.086 (0.033)		
Labor Market Concerns			0.388 (0.163)	0.371 (0.050)
Observations	1228	2599	1228	600
Panel B: Additional controls				
Treatment	0.144 (0.031)	0.039 (0.045)		
Labor Market Concerns			0.273 (0.309)	0.177 (0.059)
Observations	1211	1211	1211	592

Notes: These regressions are based on the subsample of respondents who thought that the Mariel boatlift had either negative or no effects on wages and unemployment. Column 4 only uses control group respondents, while Columns 1 to 3 use both treatment and control group respondents. The dependent variable in columns 2 to 4 is an index for attitudes towards low-skilled immigrants. The dependent variable in column 1 is “Labor Market Concerns” which is an unweighted index of people’s beliefs about the effect of low-skilled immigration on (i) “wages for most Americans” and (ii) “job opportunities or job security for most Americans.” In Column 3 we instrument “Labor Market Concerns” with the treatment indicator. In Columns 1, 2 and 4, we run OLS regressions. We randomized whether people were asked about the impact of low-skilled or high-skilled immigrants. All variables have been standardized. All regressions include the controls described in Table 2.2. Panel B additionally controls for people’s beliefs about the impact of immigration on wages and employment of the respondents’ own household, as well as beliefs about the tax burden created by immigrants for most Americans and the own household, as well as beliefs about the cultural impact of immigration. Robust standard errors in parentheses.

2.A.3 Additional tables

Table 2.11: Beliefs about the effects of low-skilled and high-skilled (post-treatment)

	Wages			Employment		Fiscal burden		Culture
	Own household	Most Americans	Own household	Most Americans	Own household	Most Americans	Most Americans	
Panel A: Effect of low-skilled								
Treatment	0.022 (0.049)	0.163 (0.046)	0.015 (0.049)	0.156 (0.047)	0.024 (0.049)	0.030 (0.048)	0.042 (0.050)	
Observations	1474	1474	1469	1469	1464	1464	1464	1452
Panel B: Effect of high-skilled								
Treatment	0.073 (0.047)	0.197 (0.048)	0.155 (0.048)	0.264 (0.048)	0.058 (0.047)	0.081 (0.047)	0.052 (0.049)	
Observations	1476	1476	1470	1470	1464	1464	1464	1449

Notes: The table shows OLS regression results where the dependent variables are beliefs about the economic and cultural impact of different types of immigrants. Respondents were asked how they thought admitting more low-skilled/high-skilled immigrants would affect (i) “wages,” (ii) “job opportunities and job security,” and (iii) “taxes” for their own household as well as for most Americans. They were also asked about how they thought admitting more low-skilled/high-skilled immigrants “would affect American culture and society as a whole.” We randomized whether respondents answered these questions for low-skilled or high-skilled immigrants. All questions were answered on 5-point Likert scales where higher values indicate more optimistic views on the effect of immigration. The outcome variables are z-scored using the mean and standard deviation in the control group. We use the same controls as described in Table 2.2. Robust standard errors in parentheses.

Table 2.12: Self-reported attitudes towards immigration (post-treatment) – with pessimistic priors

	Low-skilled (Main Study)			High-skilled (Main Study)			Follow-up	
	Not familiar	Familiar	Index	Not familiar	Familiar	Index	Low-skilled	High-skilled
Panel A: With controls								
Treatment	0.172 (0.036) [0.001]	0.128 (0.037) [0.001]	0.150 (0.033)	0.121 (0.036) [0.001]	0.057 (0.037) [0.067]	0.089 (0.033)	0.092 (0.044)	0.078 (0.046)
Adjusted p-value								
Observations	2599	2599	2599	2599	2599	2599	1724	1724
Panel B: Without controls								
Treatment	0.175 (0.039) [0.001]	0.142 (0.039) [0.001]	0.159 (0.036)	0.130 (0.039) [0.001]	0.068 (0.038) [0.040]	0.099 (0.035)	0.114 (0.047)	0.098 (0.048)
Adjusted p-value								
Observations	2599	2599	2599	2599	2599	2599	1724	1724

Notes: The table shows OLS regression results where the dependent variables are attitudes to the different types of immigrants. The answers were given on a five point scale from 1: "Allow a lot less of these immigrants" to 5: "Allow a lot more of these immigrants." The question order was randomized (statistical tests show no order effects). The outcomes are z-scored using the mean and standard deviation in the control group. The indices are created by taking the mean of the responses to immigrants with different familiarity with American values and traditions for each skill level. Adjusted p-values are in brackets. "Treatment" is an indicator equal to 1 if respondents received the research evidence. Controls include gender, age, ethnicity, region, household size, household income, education, employment status, party affiliation, whether the respondent was born in the US, whether the subject's parents were born in the US, self-perceived skill-level, and pre-treatment beliefs about the labor market impact of low-skilled (or high-skilled) immigration and are coded as described in the pre-analysis plan. Robust standard errors in parentheses.

Table 2.13: Self-reported attitudes towards immigration (post-treatment): Sample of respondents who completed the follow-up

	Low-skilled (Main Study)				High-skilled (Main Study)				Follow-up	
	Not familiar		Familiar		Not familiar		Familiar		Low-skilled	High-skilled
	Index	Index	Index	Index	Index	Index	Index	Index	Index	Index
Panel A: With controls										
Treatment	0.145 (0.040) [0.001]	0.104 (0.042) [0.006]	0.125 (0.037)	0.091 (0.041) [0.051]	0.043 (0.042) [0.183]	0.067 (0.037)	0.096 (0.040)	0.064 (0.042)		
Adjusted p-value										
Observations	2075	2075	2075	2075	2075	2075	2075	2075	2075	2075
Panel B: Without controls										
Treatment	0.157 (0.044) [0.001]	0.119 (0.044) [0.003]	0.138 (0.040)	0.109 (0.043) [0.024]	0.052 (0.043) [0.129]	0.080 (0.039)	0.108 (0.044)	0.070 (0.043)		
Adjusted p-value										
Observations	2075	2075	2075	2075	2075	2075	2075	2075	2075	2075

Notes: The table shows OLS regression results where the dependent variables are attitudes to the different types of immigrants and uses respondents who completed both the initial survey and the follow-up survey. The answers were given on a five point scale from 1: "Allow a lot less of these immigrants" to 5: "Allow a lot more of these immigrants." The question order was randomized (statistical tests show no order effects). The outcomes are z-scored using the mean and standard deviation in the control group. The indices are created by taking the mean of the responses to immigrants with different familiarity with American values and traditions for each skill level. Adjusted p-values are in brackets. "Treatment" is an indicator equal to 1 if respondents received the research evidence. Controls include gender, age, ethnicity, region, household size, household income, education, employment status, party affiliation, whether the respondent was born in the US, whether the subject's parents were born in the US, self-perceived skill-level, and pre-treatment beliefs about the labor market impact of low-skilled (or high-skilled) immigration and are coded as described in the pre-analysis plan. Robust standard errors in parentheses.

Table 2.14: Self-interested labor market concerns vs. group-level labor market concerns

	Low-skilled		High-skilled		Low-skilled		High-skilled	
	Not familiar	Familiar	Not familiar	Familiar	Not familiar	Familiar	Not familiar	Familiar
Effect of immigrants on own household's wage	-0.113 (0.067)	-0.022 (0.069)	-0.057 (0.061)	0.016 (0.063)	-0.137 (0.066)	-0.032 (0.069)	-0.069 (0.059)	0.003 (0.061)
Effect of immigrants on most Americans' wages	0.207 (0.059)	0.115 (0.060)	0.145 (0.057)	0.139 (0.059)	0.157 (0.057)	0.062 (0.060)	0.062 (0.056)	0.060 (0.058)
Effect of immigrants on own household's employment	-0.122 (0.063)	-0.108 (0.065)	0.098 (0.061)	0.098 (0.062)	-0.138 (0.062)	-0.122 (0.065)	0.060 (0.059)	0.069 (0.061)
Effect of immigrants on most Americans' employment	0.406 (0.054)	0.335 (0.056)	0.251 (0.055)	0.185 (0.057)	0.334 (0.053)	0.279 (0.056)	0.202 (0.053)	0.140 (0.055)
Effect of immigrants on culture					0.257 (0.036)	0.213 (0.037)	0.286 (0.037)	0.267 (0.038)
Observations	762	762	714	714	750	750	704	704

Notes: This table uses data from respondents in the control group. The table shows OLS regression results where the dependent variables are attitudes to the four different types of immigrants: (i) low-skilled immigrants not familiar with American values and traditions, (ii) low-skilled immigrants highly familiar with American values and traditions, (iii) high-skilled immigrants not familiar with American values and traditions, and (iv) high-skilled immigrants highly familiar with American values and traditions. The answers were given on a five point scale from 1: "Allow a lot less of these immigrants" to 5: "Allow a lot more of these immigrants." The question order was randomized (statistical tests show no order effects). The outcomes are z-scored using the mean and standard deviation in the control group. The independent variables are beliefs about the impact of low-skilled and high-skilled immigrants on the wages and employment of (i) people's own household and (ii) most Americans. On top of that, people's beliefs about the effect of immigrants on American culture are included on columns (5) to (8). All questions were answered on 5-point Likert scales where higher values indicate more optimistic views regarding the effect of immigration. Robust standard errors in parentheses.

Table 2.15: Self-interested vs. nationwide labor market concerns: Evidence from the General Social Survey

	Support for immigration
Belief: Most Americans	0.331 (0.031)
Belief: own household	0.031 (0.028)
Observations	1263

Notes: This table uses data from the 1994 wave of the General Social Survey. *Support for immigration* is the standardized response as to whether “the number of immigrants to America nowadays” should be “decreased a lot” to “increased a lot.” Self-interested labor market concerns are measured with people’s answer to the following question: “What about immigrants? Is it very likely, somewhat likely, somewhat unlikely, or very unlikely that you or anyone in your family won’t get a job or promotion while an equally or less qualified immigrant employee receives one instead?” We proxy for people’s group-level labor market concerns with people’s response to the following question: “How much do you agree or disagree with each of the following statements? Immigrants take jobs away from people who were born in America.”

2.A.4 Heterogeneous effects

Table 2.16: Heterogeneous treatment effects by pre-treatment beliefs: Labor market concerns

	Wages		Employment	
	Own household	Most Americans	Own household	Most Americans
Panel A: Effect of low-skilled				
Treatment × Prior: Low skill	-0.102 (0.062)	-0.140 (0.059)	-0.069 (0.058)	-0.179 (0.056)
Treatment × Prior: High skill	-0.108 (0.081)	-0.071 (0.075)	0.037 (0.090)	0.129 (0.083)
Treatment	0.573 (0.269)	0.695 (0.238)	0.056 (0.288)	0.155 (0.263)
Observations	1483	1483	1478	1478
Panel B: Effect of high-skilled				
Treatment × Prior: Low skill	-0.170 (0.056)	-0.188 (0.055)	-0.141 (0.051)	-0.049 (0.053)
Treatment × Prior: High skill	-0.041 (0.073)	-0.009 (0.068)	-0.041 (0.078)	0.051 (0.077)
Treatment	0.566 (0.251)	0.629 (0.237)	0.562 (0.248)	0.204 (0.239)
Observations	1487	1487	1481	1481

Notes: The dependent variables are beliefs about the economic impact of low-skilled and high-skilled immigrants. Respondents were asked how they thought admitting more low-skilled/high-skilled immigrants would affect (i) “wages” and (ii) “job opportunities and job security.” We randomized whether respondents answered these questions with respect to low-skilled or high-skilled immigrants. All questions were answered on 5-point Likert scales where higher values indicate more optimistic views regarding the effect of immigration. “Treatment” is an indicator equal to 1 if respondents received the research evidence. “Prior: Low-skill” (“Prior: High-skill”) is people’s pre-treatment belief about the wage and employment effects of the Mariel boatlift on low-skilled (high-skilled) workers. The regressions include pre-specified controls (listed in Table 2.2). Robust standard errors in parentheses.

Table 2.17: Heterogeneous treatment effects: Self-reports

	Low-skilled				High-skilled			
	Not familiar	Familiar	Index	Follow-up	Not familiar	Familiar	Index	Follow-up
Panel A: Prior Belief								
Treatment × Prior: Low-skilled	-0.067 (0.043)	-0.074 (0.045)	-0.071 (0.040)	-0.039 (0.051)	-0.074 (0.044)	-0.034 (0.045)	-0.054 (0.040)	-0.019 (0.056)
Treatment × Prior: High-skilled	0.068 (0.047)	0.053 (0.049)	0.060 (0.044)	0.003 (0.055)	0.043 (0.047)	0.029 (0.049)	0.036 (0.044)	-0.082 (0.059)
Treatment	0.169 (0.033)	0.112 (0.034)	0.140 (0.031)	0.096 (0.040)	0.104 (0.033)	0.040 (0.034)	0.072 (0.030)	0.064 (0.041)
Observations	3130	3130	3130	3130	3130	3130	3130	3130
Panel B: Republican								
Treatment × (a) Republican	-0.058 (0.075)	0.002 (0.082)	-0.028 (0.071)	0.055 (0.095)	-0.006 (0.078)	-0.045 (0.083)	-0.025 (0.073)	0.011 (0.100)
Treatment (b)	0.185 (0.038)	0.111 (0.038)	0.148 (0.035)	0.082 (0.045)	0.106 (0.038)	0.052 (0.038)	0.079 (0.034)	0.061 (0.046)
Pr(a+b)=0	0.053	0.123	0.055	0.104	0.147	0.925	0.406	0.418
Observations	3130	3130	3130	2075	3130	3130	3130	2075
Panel C: High-skill								
Treatment × (a) High-skill	-0.038 (0.075)	-0.062 (0.078)	-0.050 (0.069)	-0.021 (0.090)	0.073 (0.076)	-0.017 (0.080)	0.028 (0.070)	-0.015 (0.095)
Treatment (b)	0.199 (0.065)	0.158 (0.067)	0.178 (0.059)	0.112 (0.077)	0.050 (0.065)	0.053 (0.070)	0.051 (0.060)	0.076 (0.081)
Pr(a+b)=0	0.000	0.016	0.000	0.053	0.002	0.359	0.025	0.215
Observations	3130	3130	3130	2075	3130	3130	3130	2075

Notes: The table shows OLS regression results where the dependent variables are attitudes to the four different types of immigrants: (i) low-skilled immigrants not familiar with American values and traditions, (ii) low-skilled immigrants highly familiar with American values and traditions, (iii) high-skilled immigrants not familiar with American values and traditions, and (iv) high-skilled immigrants highly familiar with American values and traditions. The answers were given on a five point scale from 1: “Allow a lot less of these immigrants” to 5: “Allow a lot more of these immigrants.” The question order was randomized (statistical tests show no order effects). The outcomes are z-scored using the mean and standard deviation in the control group. The indices are created by taking the mean of the responses to immigrants with different familiarity with American values and traditions for each skill level. “Treatment” is an indicator equal to 1 if respondents received the research evidence. “Prior: Low-skill” (“Prior: High-skill”) is people’s pre-treatment belief about the wage and employment effects of the Mariel boatlift on low-skilled (high-skilled) workers. “Republican” takes value 1 if our respondent self-identifies as a Republican and zero otherwise. “High-skill” takes values 1 if our respondent self-identifies as high-skilled and zero otherwise. We use the same controls as described in Table 2.2. Robust standard errors in parentheses.

Table 2.18: Heterogeneous treatment effects: Self-reports

	Low-skilled				High-skilled			
	Not familiar	Familiar	Index	Follow-up	Not familiar	Familiar	Index	Follow-up
Panel A: Prior Belief								
Treatment × Prior	-0.056 (0.043)	-0.063 (0.045)	-0.059 (0.040)	-0.044 (0.051)	0.034 (0.046)	0.026 (0.049)	0.030 (0.044)	-0.079 (0.057)
Treatment	0.170 (0.033)	0.111 (0.034)	0.141 (0.031)	0.096 (0.040)	0.105 (0.033)	0.040 (0.034)	0.073 (0.031)	0.063 (0.041)
Observations	0.033 3130	0.383 3130	0.099 3130	0.401 2075	0.011 3130	0.237 3130	0.041 3130	0.811 2075
Panel B: Republican								
Treatment × (a) Republican	-0.058 (0.075)	0.002 (0.082)	-0.028 (0.071)	0.055 (0.095)	-0.006 (0.078)	-0.045 (0.083)	-0.025 (0.073)	0.011 (0.100)
Treatment (b)	0.185 (0.038)	0.111 (0.038)	0.148 (0.035)	0.082 (0.045)	0.106 (0.038)	0.052 (0.038)	0.079 (0.034)	0.061 (0.046)
Pr(a+b)=0 Observations	0.053 3130	0.123 3130	0.055 3130	0.104 2075	3130	3130	3130	2075
Panel C: High-skill								
Treatment × (a) High-skill	-0.038 (0.075)	-0.062 (0.078)	-0.050 (0.069)	-0.021 (0.090)	0.073 (0.076)	-0.017 (0.080)	0.028 (0.070)	-0.015 (0.095)
Treatment (b)	0.199 (0.065)	0.158 (0.067)	0.178 (0.059)	0.112 (0.077)	0.050 (0.065)	0.053 (0.070)	0.051 (0.060)	0.076 (0.081)
Pr(a+b)=0 Observations	0.000 3130	0.016 3130	0.000 3130	0.053 2075	3130	3130	3130	2075

Notes: The table shows OLS regression results where the dependent variables are attitudes to the four different types of immigrants: (i) low-skilled immigrants not familiar with American values and traditions, (ii) low-skilled immigrants highly familiar with American values and traditions, (iii) high-skilled immigrants not familiar with American values and traditions, and (iv) high-skilled immigrants highly familiar with American values and traditions. The answers were given on a five point scale from 1: "Allow a lot less of these immigrants" to 5: "Allow a lot more of these immigrants." The question order was randomized (statistical tests show no order effects). The outcomes are z-scored using the mean and standard deviation in the control group. The indices are created by taking the mean of the responses to immigrants with different familiarity with American values and traditions for each skill level. "Treatment" is an indicator equal to 1 if respondents received the research evidence. "Prior: Low-skilled" ("Prior: High-skilled") is people's pre-treatment belief about the wage and employment effects of the Mariel boatlift on low-skilled (high-skilled) workers. "Republican" takes value 1 if our respondent self-identifies as a Republican and zero otherwise. "High-skill" takes values 1 if our respondent self-identifies as high-skilled and zero otherwise. We use the same controls as described in Table 2.2. Robust standard errors in parentheses.

Table 2.19: Heterogeneous treatment effects: Intention to sign petitions

	Intention: H2B Visas		
	Increase	Decrease	Net support
Panel A: Prior Belief			
Treatment \times (a) Prior	-0.004 (0.019)	0.011 (0.019)	-0.027 (0.042)
Treatment (b)	0.047 (0.016)	-0.058 (0.016)	0.135 (0.034)
Pr(a+b)=0	0.095	0.041	0.047
Observations	3130	3130	3130
Panel B: Republican			
Treatment \times (a) Republican	-0.032 (0.033)	-0.017 (0.038)	-0.014 (0.076)
Treatment (b)	0.056 (0.019)	-0.054 (0.017)	0.138 (0.040)
Pr(a+b)=0	0.391	0.039	0.056
Observations	3130	3130	3130
Panel C: High-skill			
Treatment \times (a) High-skill	-0.010 (0.035)	-0.014 (0.037)	0.018 (0.079)
Treatment (b)	0.055 (0.030)	-0.048 (0.032)	0.121 (0.068)
Pr(a+b)=0	0.016	0.001	0.000
Observations	3130	3130	3130

Notes: The three first columns show regression results where the dependent variable is intention to sign the petitions. “Increase” (“Decrease”) is an indicator equal to 1 if a respondent wanted to sign the petition suggesting to increase (decrease) the annual cap on the H-2B visa program. “Net support” is a z-scored transformation of a variable taking value 1 (-1) if a respondent wanted to sign the petition to increase (decrease) the annual cap on the H-2B visa program and 0 otherwise. “Treatment” is an indicator equal to 1 if respondents received the research evidence. “Prior” is people’s pre-treatment belief about the wage and employment effects of the Mariel boatlift on low-skilled (high-skilled) workers. “Republican” takes value 1 if our respondent self-identifies as a Republican and zero otherwise. “High-skill” takes values 1 if our respondent self-identifies as high-skilled and zero otherwise. We use the same controls as described in Table 2.2. Robust standard errors in parentheses.

2.B Deviations from the pre-analysis plan

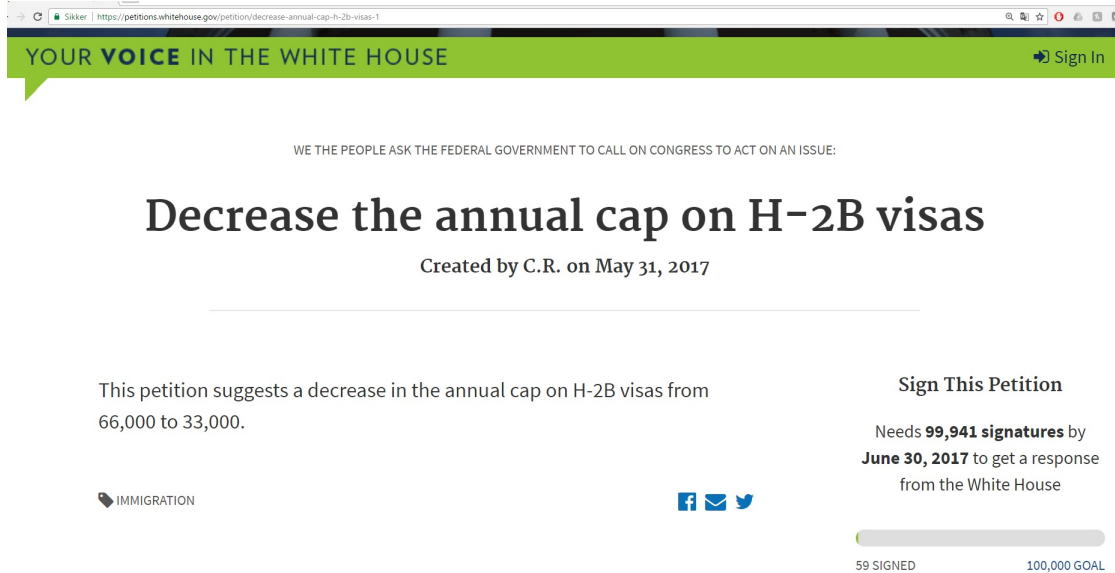
We posted the original pre-analysis plan on May 31, 2017 before starting the data collection. We uploaded an updated pre-analysis plan June 7, before starting collecting the data collection for the obfuscated follow-up, that was identical to the original pre-analysis plan in all aspects except for some minor changes in the instructions for the obfuscated follow-up study. The reader should consult the updated pre-analysis plan to get the actual instructions used in the experiment. Some minor deviations from the pre-analysis plan are listed below:

- We pre-specified a sample size of 3000 respondents. The survey provider delivered a sample of 3130 respondents who completed the main outcome variables of interest. Out of those, 2883 respondents completed all questions in the main study.
- Because 247 respondents did not complete all the demographic questions asked at the end of the main survey, we have some missing observations for these questions. For all questions with missing values, we included a dummy for missing observations to account for this. We also supplemented some missing values from the background questions in the obfuscated follow-up survey.
- We specified applying a proportion test to the difference in the proportion of signatures on the petition in favor of increasing the annual cap and the petition in favor of decreasing the annual cap. We realized that the proportion test can only be applied to binary variables, so we applied it for each of the two petitions separately.

- We did not pre-specify the results presented on belief updating (Section 2.3.2), the IV estimation (Section 3.3) and the machine learning approaches to heterogeneity (Section 3.5).
- When analyzing heterogeneous treatment effects on self-reported attitudes by pre-treatment beliefs, we pre-specified to only include the pre-treatment beliefs about the effect of the Mariel boatlift on low-skilled (high-skilled) workers when analyzing support for low-skilled (high-skilled) immigration. We later concluded that it was more informative to include interaction terms for both beliefs in both specifications. The pre-specified table is still included in the appendix (Table 2.18).

2.C Screenshots

Figure 2.10: Petition



The screenshot shows a web browser window displaying a petition page. The browser's address bar shows the URL: <https://petitions.whitehouse.gov/petition/decrease-annual-cap-h-2b-visas-1>. The page header features the text "YOUR VOICE IN THE WHITE HOUSE" and a "Sign In" button. Below the header, a sub-header reads "WE THE PEOPLE ASK THE FEDERAL GOVERNMENT TO CALL ON CONGRESS TO ACT ON AN ISSUE:". The main title of the petition is "Decrease the annual cap on H-2B visas", created by C.R. on May 31, 2017. The petition text states: "This petition suggests a decrease in the annual cap on H-2B visas from 66,000 to 33,000." To the right, there is a "Sign This Petition" section with the text: "Needs 99,941 signatures by June 30, 2017 to get a response from the White House". Below this, a progress bar shows "59 SIGNED" out of a "100,000 GOAL". Social media sharing icons for Facebook, Email, and Twitter are also visible.

Figure 2.11: Invitation in the email sent out for the obfuscated follow-up study

Hi John,

You have an opportunity waiting!

Topic: Personal Opinion

Incentive: \$2.5 in e-Rewards® Currency

Length: 10 minutes

LET'S BEGIN

Figure 2.12: Consent form in the main study

This study is conducted by The Choice Lab at NHH Norwegian School of Economics. You must be a US citizen of at least 18 years of age to participate in this study. If you do not fulfill these requirements, please do not continue any further.

You are not allowed to participate in this study more than once. If you experience a technical error or problem, do not try to restart or retake the study. Rather, send us an email with a description of your problem and we will get back to you. If you have any questions regarding this study, please email thechoicelab@nhh.no

I have read and understood the above and want to participate in this study.

Yes

No

>>

Figure 2.13: Consent form in the follow-up study

This study has received ethics clearance by the Oxford University Institutional Review Board.

If subjects have questions about this study or their rights, or if they wish to lodge a complaint or concern, they may contact us at the following email: christopher.roth@economics.ox.ac.uk

Next >>

0% 100%

Consent form

- I have read the information provided on the previous page.
- I have had the opportunity to ask questions about the study.
- I understand that I may withdraw from the study at any time.
- I understand how to raise a concern or make a complaint.
- I understand that I can only participate in this experiment once.
- I understand that close attention to the survey is required for my responses to count.

If you are 18 years of age or older, agree with the statements above, and freely consent to participate in the study, please click on the "I Agree" button to begin the experiment.

I agree

I disagree

Next >>

0% 100%

Figure 2.14: Screenshot of the informational treatment

NHH



The researchers who analyzed the short- and long-term effects of the mass immigration of Cubans to Miami concluded that, for both high-skilled and low-skilled workers, the mass immigration had **virtually no effect on wages** and **virtually no effect on unemployment**.

According to the researchers, the mass immigration had virtually no effect on wages and unemployment because the new Cuban immigrants increased the overall demand for goods and services, which created more jobs.

Chapter 3

Beliefs about Behavioral Responses to Taxation

Alexander W. Cappelen Ingar K. Haaland
Bertil Tungodden*

Abstract

We conduct an experiment to study how beliefs about behavioral responses to taxation and preferences over equality–efficiency trade-offs relate to the political disagreement on redistribution. We use a novel method to elicit incentivized beliefs from a sample of 13,900 Americans about how taxes affect people’s effort choices, and we elicit incentivized equality–efficiency preferences. We find that Democrats and Republicans have virtually identical beliefs about behavioral responses to taxation. Furthermore, we find that beliefs about behavioral responses to taxation fail to predict people’s support for equalization of incomes in society. Equality–efficiency preferences, by contrast, strongly predict both people’s political affiliation and their support for equalization of incomes in society. We also explore the role of motivated beliefs and identity politics by priming respondents about the political disagreement

* Affiliation of all authors: Department of Economics, NHH Norwegian School of Economics. We would like to thank Alberto Alesina, Roland Bénabou, Edward Glaeser, Olof Johansson-Stenman, David Laibson, Matthew Rabin, Christopher Roth, Rupert Sausgruber, Andrei Schleifer, Klaus Schmidt, Daniel Schunk, Stefanie Stantcheva, Matthias Sutter, Guido Tabellini, Jean-Robert Tyran, Jonas Tungodden, and numerous seminar and conference participants for their helpful comments and discussions. This work was partially supported by the Research Council of Norway through its Centre of Excellence Scheme, FAIR project No 262675. The experiment is registered in the AEA RCT Registry as trial 2186. The usual disclaimer applies.

on redistribution. The treatments increase political polarization in preferences, but do not polarize beliefs. Our findings suggest that the political divide on redistribution relates more to people's preferences than to their beliefs about the behavioral responses to taxation. (*JEL C91, D83, H20*)

3.1 Introduction

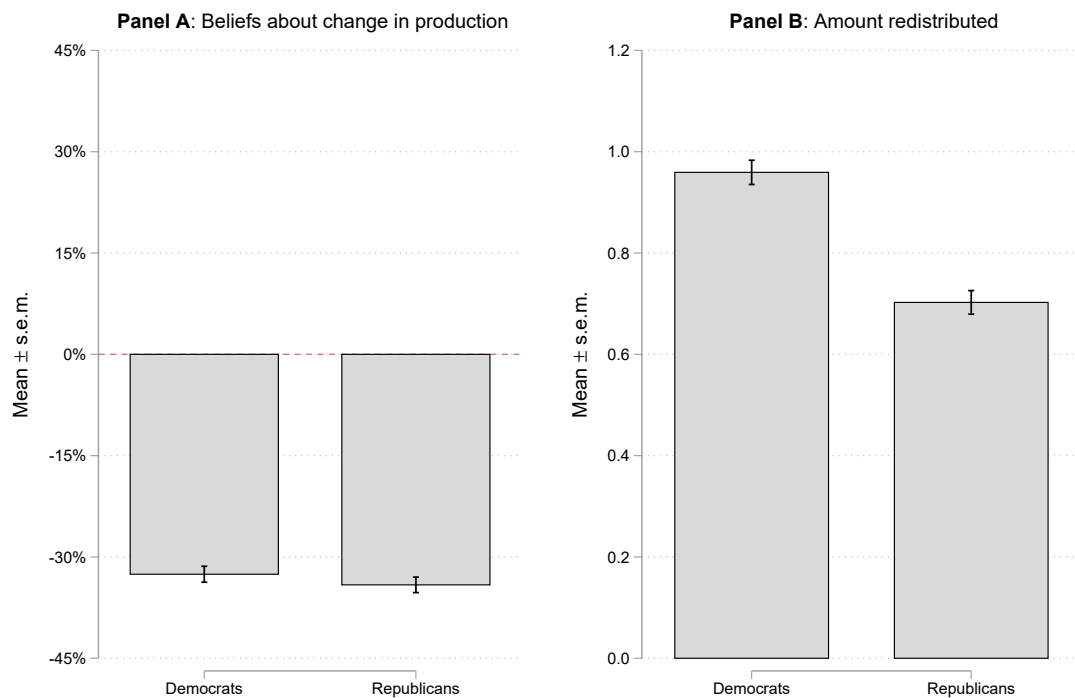
Behavioral responses to taxation are of fundamental importance for the debate on the redistribution of income and wealth. Graduate textbooks in economics emphasize that behavioral responses to taxation determine both the optimal progressivity of the tax-and-transfer system as well as the optimal size of the government (Saez et al., 2012). Because economists have different estimates of behavioral responses to taxation, they also have different policy recommendations when it comes to tax policy (Diamond and Saez, 2011; Feldstein, 2006). The disagreement about redistribution also extends to voters. In the US, for instance, 80 percent of Democrats think that the government should implement "heavy taxes on the rich," while only 22 percent of Republicans think the same (Newport, 2016). To explain why voters disagree about redistribution, some previous research has highlighted the importance of beliefs about the efficiency cost of redistribution. For instance, Piketty (1995) writes that "voters have conflicting views about redistributive taxation because they estimate its incentive costs differently" and Alesina and Giuliano (2011) write that "right wingers tend to believe that the elasticity of labor supply to taxes is high and the other way around." We test the empirical validity of this explanation by eliciting incentivized beliefs about behavioral responses to taxation from a representative sample of 13,900 Americans.

In the study, we employ a spectator–worker design where spectators estimate how much workers produce in a real-effort task under different payment schemes. The design allows us to elicit beliefs in a tightly controlled environment to obtain quantitative and incentivized estimates of the spectator’s beliefs about behavioral responses to taxation. In the experiment, we inform the spectators that we have recruited workers from an online labor market to work on a task for one hour. We tell the spectators that the workers were offered different bonus schemes. In the main treatment, we inform the spectators about how much the workers produced under a 20 cents piece rate with no taxes on earnings. We then incentivize the spectators to estimate how much workers produce under a 20 cents piece rate with a 50 percent tax to the US government.

While beliefs about behavioral responses may be an important source of political disagreement about redistribution, people could also differ in their views on redistribution because they have different social preferences (Almås et al., 2016; Fisman et al., 2015; Saez and Stantcheva, 2016). To elicit people’s equality–efficiency preferences, we gave the spectators an opportunity to redistribute earnings at a cost between two receivers who had been allocated unequal earnings after completing the same assignment. In this setting, the spectators had identical beliefs about the redistribution cost and had to make a trade-off between implementing equality and efficiency.

This chapter offers two main findings, which are summarized in Figure 3.1. First, in contrast to predictions from prominent political economy models of redistribution (Piketty, 1995), we find no systematic differences between Republicans and Democrats in their beliefs about behavioral responses to taxation. Second, we find large political differences in equality–efficiency prefer-

Figure 3.1: Political differences in beliefs and preferences



Notes: The bars indicate the mean values by political affiliation. The lines indicate the standard error of the mean. In **Panel A**, the outcome is people's beliefs about the percentage change in production under High Pay or Government Tax treatments. In **Panel B**, the outcome is the amount redistributed in the spectator's decision. Panels include respondents from the Government Tax treatment in both experiments. The lines indicate the standard error of the mean.

ences: e.g., Republicans are 15 percentage points less likely than Democrats to redistribute earnings from the lucky to the unlucky receiver. This result suggests that the partisan divide on redistribution is primarily driven by differences in preferences and not by beliefs.

Over two experiments, we introduce several treatments to explore mechanisms and test for robustness. In the first experiment, we find that people's beliefs are not in line with the standard model in economics according to which workers only care about their personal incentives. For instance, Democrats and Republicans alike believe that taxes paid to the US government are more detrimental to worker production than paying workers a lower wage with the same personal incentives for the workers. In the second experiment, we replicate the main results from the first experiment and further explore the role of motivated beliefs and group identity by priming the respondents about the political debate on taxation or party views on behavioral responses to taxation. We find that the priming treatments do not lead to political polarization in beliefs, but they do increase political polarization in equality–efficiency preferences and support for redistributive taxation. These findings suggest an important role for motivated reasoning and identity politics in the debate on redistribution.

Finally, we use our incentivized measures of beliefs and preferences to explore whether preferences or beliefs are more strongly related to people's views on redistributive policies. While equality—efficiency preferences are strongly associated with people's policy views on redistribution, we find that beliefs are only correlated with policy views for Democrats. These findings provide further evidence that people's policy views on redistribution are primarily driven by differences in equality-efficiency preferences.

By investigating the roles of beliefs about behavioral responses to taxation and equality–efficiency preferences in forming attitudes towards redistribution, our results contribute to a rich literature on the determinants of people’s redistributive preferences (Alesina and Angeletos, 2005; Bénabou and Ok, 2001; Bénabou and Tirole, 2006; Kuziemko et al., 2015; Meltzer and Richard, 1981; Persson and Tabellini, 2000; Piketty, 1995). In particular, we relate to studies showing that people’s fairness preferences may be instrumental in forming their views on redistributive policies (Alesina and La Ferrara, 2005; Alesina and Angeletos, 2005; Almås et al., 2016; Bénabou and Tirole, 2006; Di Tella et al., 2017). More broadly, our results relate to the literature on social preferences and what motivates effort (Almås et al., 2016; Bolton and Ockenfels, 2000; Charness and Rabin, 2002; DellaVigna and Pope, 2018; Fehr and Schmidt, 1999; Fisman et al., 2015; Kessler and Norton, 2016) and the public finance literature on optimal tax policy and behavioral responses to taxation (Diamond and Saez, 2011; Mankiw et al., 2009; Piketty et al., 2014).

This chapter proceeds as follows: Section 3.2 describes the design and sample for the first experiment. Section 3.3 presents the theoretical frameworks to guide interpretation of the results. Section 3.4 outlines the empirical strategy for the first experiment. Section 3.5 reports the results from the first experiment. Section 3.6 describes the design and reports results from the second experiment. Section 3.7 relates beliefs and equality-efficiency preferences to views on redistributive policies. Finally, Section 3.8 concludes.

3.2 Experiment 1: Design and participants

In the first experiment, we collected data for three types of participants: *workers*, *spectators* and *receivers*. We were primarily interested in the specta-

tors, but we also recruited workers and receivers to incentivize, respectively, the elicitation of beliefs and equality–efficiency preferences. Three parts of the experiment focused on eliciting spectators’ (i) beliefs about behavioral responses to taxation, (ii) equality–efficiency preferences, and (iii) policy views on redistribution. To elicit their beliefs about behavioral responses to taxation, we first ask the spectators to estimate how much the workers produced under different incentive schemes. To elicit their equality–efficiency preferences, we let the spectators decide whether to redistribute earnings between a pair of receivers or not. Finally, to elicit their views on redistribution, we ask the spectators whether they think society should aim to equalize incomes.

3.2.1 The workers

We recruited 1616 workers from the online labor market Amazon Mechanical Turk (MTurk). Column 1 in Table 3.6 provides the summary statistics for the workers. We chose to recruit actual workers for three main reasons. First, the design allows us to introduce treatment variations to explore the mechanism. In our experiment, we had four worker groups who were given different incentives during the real-effort task. Second, the design allows us to give the spectators full information about the economic environment. By fixing the economic environment and measuring beliefs on a quantitative scale, beliefs are easily comparable across respondents and have the same interpretation for everyone. Third, recruiting actual workers is a transparent way of incentivizing the spectators’ beliefs. Incentivizing beliefs was particularly important for our purposes as monetary incentives have been shown to strongly reduce biases in reported beliefs about economic and political facts

(Bullock et al., 2015; Prior et al., 2015).

We paid the workers a \$2 participation fee and they could work on a real-effort task for up to one hour. The real-effort task consisted of checking off even numbers in large matrices of random numbers. There were 30 matrices in total, and the workers could spend up to two minutes on each matrix.² After each matrix, the workers were shown a summary screen that summarized how many points they had produced and their bonus so far. In a between-subject design, we offer the workers four different incentive schemes:

- *High incentives*: The workers earn a bonus of 20 cents for every 100 points produced.
- *High incentives with government tax*: The workers earn a bonus of 20 cents for every 100 points produced, but have to pay a tax of 50 percent on earnings. Taxes are transferred to the U.S. federal government for general use.
- *Low incentives*: The workers earn a bonus of 10 cents for every 100 points produced.
- *High incentives with redistributive tax*: The workers earn a bonus of 20 cents for every 100 points produced, but have to pay a tax of 50 percent on earnings. Taxes are redistributed back to the workers as a lump-sum payment.

²Figure 3.10 shows an example matrix.

3.2.2 The spectators

We recruited 4,217 spectators using Research Now, which is one of the leading digital data collection agencies in the US. In the main analysis, we focus on the 4128 respondents who spent at least 15 seconds on the belief elicitation page.³ We recruited the spectators from Research Now's *Political Panel*, which has two especially attractive features.⁴ First, data on people's political affiliations is provided directly by L2, which is one of the largest voting tracking companies in the US. The data on political affiliation is therefore partly based on the spectators' real voting behavior.⁵ Second, we did not have to ask people about their political affiliations in the experiment. We believe this mitigates concerns about experimenter demand.

Column 2 in Table 3.6 provides the summary statistics for the spectators. Since we wanted to focus on political differences, we only recruited Republicans and Democrats to participate in this study. The samples of both Republicans and Democrats were selected to match the general US population in terms of gender, age, income, race, and geography.

3.2.3 The receivers

To be able to incentivize the elicitation of equality–efficiency preferences from the spectators, we recruited an additional 900 people on MTurk to answer a 10-minute opinion survey. After finishing the survey, these receivers were

³Results with the full sample are shown in Section 3.B of the appendix. The restriction does not change any of the results.

⁴Extensive information about the panel is available at the following web page: <https://www.researchnow.com/products-services/global-audiences-and-panel/political-panel/>.

⁵More information about L2 and their voter file is available on their web page, <http://www.l2political.com/>.

informed that they had been matched in pairs and that their pay would be determined by a lottery in which the winner would earn \$7 and the loser would earn \$1.

3.2.4 Eliciting beliefs about behavioral responses to taxation

In all treatments, the spectators are first told that they will be asked how much they think others performed on a task and that they may earn a \$10 bonus if their answer is sufficiently close to how others actually performed on the task. We then give the spectators the opportunity to spend up to two minutes on the task to gain familiarity with it.⁶ After the spectators have tried out the task themselves, we inform them that two groups of workers from an online labor market have worked on the task for one hour and that these workers were offered different bonus schemes: i.e., *Bonus A* and *Bonus B*. In the main treatment, we inform the spectators that workers offered *Bonus A* earned a 20 cents piece rate, whereas workers offered *Bonus B* earned a 20 cents piece rate with a 50 percent tax to the US government. All spectators are then informed about how much workers offered *Bonus A* produced (3032 points on average). To fix beliefs about the distribution of effort among workers offered *Bonus A*, we also show the spectators a histogram of the distribution of the production by workers in this group. Finally, to elicit beliefs about how different incentives affect effort choices, we ask the spectators to estimate how many points individuals offered *Bonus B* produced on average. To incentivize their answers, we furthermore inform

⁶To participate in their surveys, Research Now pays respondents in points that can be converted into “e-Rewards.” While we paid Research Now \$10 for correct estimates, the respondents received points equal to \$10 in this panel currency. The points can be spent on retail vouchers that the respondents preselected, e.g., on Amazon, when they reach a certain number of points.

them that they will receive a \$10 bonus if their answer is within +/- 5 percent of the actual production for individuals offered Bonus B.

In the main treatment, we inform the spectators that workers offered Bonus B earned a 20 cents piece rate with a 50 percent tax to the US government. When estimating how workers respond to a government tax, spectators could differ in their beliefs about two factors: i.e., i) how costly it is for the workers to provide effort, and ii) how motivated by social incentives the workers are. To differentiate between these two factors, we add a second treatment where we describe Bonus B as a 10 cents piece rate (instead of a 20 cents piece rate with a 50 percent tax). Since the workers face the same personal incentives as in the base treatment, the standard model in economics—according to which workers only care about their personal incentives—predicts that beliefs about production in these two treatments should be identical. The second treatment tests whether beliefs are in line with the standard model in economics by isolating the importance of beliefs about social incentives. Finally, to assess robustness, we add a third treatment to test whether any motivation to pay taxes depends on the recipient of the tax revenue. In this treatment, we describe a bonus as a 20 cents piece rate with a 50 percent tax that is redistributed back to workers as a lump-sum payment. This treatment allows us to assess whether beliefs about the social motivation to pay taxes depend on the recipient of the tax revenue. We can summarize the three spectator treatments as follows:

- **Government Tax:** Spectators are informed about the production of workers offered *high incentives* and state their beliefs about the production of workers offered *high incentives with government tax*.
- **Low Pay:** Spectators are informed about the production of workers

offered *high incentives* and state their beliefs about the production of workers offered *low incentives*.

- **Redistributive Tax:** Spectators are informed about the production of workers offered *high incentives* and state their beliefs about the production of workers offered *high incentives with redistributive tax*.

3.2.5 Equality–efficiency preferences

In the second part of the experiment, we introduced a real redistributive setting to measure people’s equality efficiency preferences. Specifically, we told the spectators that they had been given the opportunity to redistribute earnings between two receivers that had completed an identical assignment and had their earnings determined by a lottery. The spectators were informed that the receiver winning the lottery had earned \$7 and the recipient losing the lottery had earned \$1. We also told the spectators that the receivers did not know the outcome of the lottery, but that they had been informed that a third person would be given the opportunity to redistribute their earnings. Finally, we introduced a redistribution cost: i.e., each dollar redistributed from the lucky recipient to the unlucky recipient would reduce the payments to the lucky worker by \$2. Thus, the spectators could choose between keeping the unequal income distribution (7:1) or implement any of the following income distributions: (5:2), (3:3), or (1:4). We informed the spectators that their decisions would be implemented with a one in ten chance.

This redistributive setting has two key features. First, by fixing the redistribution cost, we eliminated the role of differences in beliefs from the redistributive decisions. Second, by making redistribution costly, we created a

real trade-off between implementing equality and efficiency.

3.3 Theory

To guide the interpretation of the results presented in the next section, we first present two simple frameworks that motivated our design choices in the elicitation of beliefs and preferences.

3.3.1 Beliefs about behavioral responses to taxation

We assume that the spectators consider two factors when estimating behavioral responses to taxation: (i) how costly they think it is for workers to provide effort and (ii) how much they think the workers value a dollar paid in taxes. The first factor follows from the *standard model in economics*, according to which workers only care about their personal incentives after taxes. The second factor is more behavioral: i.e., the spectators may believe that the workers are motivated by social incentives and thus place some weight on the welfare of the tax recipient.⁷ In our model of how the spectators form their expectations, we assume the spectators envision that workers maximize utility given by:

$$U(e; \cdot) = we[(1 - \tau) + \gamma\tau] - c(e) \quad (3.1)$$

where w is the piece-rate wage, e is points produced (effort), τ is the tax rate, γ is the weight on taxed income, and $c(e)$ is a convex cost-of-effort

⁷There is mixed evidence of whether taxes discourage or motivate workers. A recent study by Rick et al. (2018) find that taxes motivate people who favor redistribution and government intervention to work harder. By contrast, Kessler and Norton (2016) find that workers provide less effort when they are taxed compared to when their wages are cut by the same amount as the tax.

function that satisfies the usual conditions. Utility is linear in money (i.e., we abstract from income effects). The first-order condition (assuming an interior solution) for this problem is given by:

$$e^* = c'^{-1}(w[(1 - \tau) + \gamma\tau]) \quad (3.2)$$

Thus, the spectators can have different beliefs about workers' cost of providing effort, $c(e)$, and the workers' social preferences towards the tax recipient, γ . The treatment difference between Government Tax and Low Pay allows us to identify whether spectators think $\gamma = 0$ as the standard model in economics predicts. Furthermore, the treatment difference between government and redistributive taxes allows us to identify whether beliefs about γ depend on whether the tax revenues benefit the government or other workers. We will later assume that $c(e)$ in Equation (3.2) is quadratic (i.e., on the form ae^2 , where a is a constant) to derive structural estimates of how much the spectators believe the workers would be willing to give up to increase tax revenues by \$1.

3.3.2 Equality–efficiency preferences

The spectators choose whether to redistribute costs between two receivers. We use a standard spectator framework to guide the analysis of how the spectators make a trade-off between implementing equality and efficiency in this setting (Almås et al., 2016; Cappelen et al., 2013). In our framework, the spectators care about *fairness* and *efficiency*. Formally, the spectators' utility function is given by:

$$V(y; \cdot) = -\frac{\beta}{2}(y - m)^2 - \psi y \quad (3.3)$$

where $\beta > 0$ is the weight attached to fairness relative to efficiency, y is the share of total income to the unlucky recipient, m is the spectators' perceived fair share of total income for the unlucky recipient, and ψ is the redistribution cost. The optimal solution (assuming an interior solution) is given by:

$$y^* = m - \frac{\psi}{\beta} \quad (3.4)$$

The model captures that the spectators may differ in two respects: i.e., what they think is fair, m , and how much weight should be attached to fairness relative to efficiency, β . It follows from (3.4) that spectators who mainly care about fairness should redistribute earnings such that the actual share to the unlucky receivers equals the perceived fair share (i.e., $\beta \rightarrow \infty$ implies that $y^* \rightarrow m$). By contrast, spectators who mainly care about efficiency should choose to not redistribute at all (i.e., $\beta \rightarrow 0$ implies that $y^* \rightarrow 0$). If $m = \frac{1}{2}$, we have the standard equality–efficiency trade-off.⁸

3.4 Empirical strategy: Experiment 1

We pre-specified our analysis in a document uploaded to the AEA RCT Registry prior to starting the data collection. The pre-analysis plan is available from the following link: <https://www.socialscienceregistry.org/trials/2186>.

⁸Almås et al. (2016) find that the majority of Americans consider an equal split as fair when incomes are determined by luck.

3.4.1 Analysis of beliefs

Main treatment effects

In the first specification of interest, we test the effects of our treatment manipulations. First, we investigate whether the spectators have beliefs about behavioral responses to taxation that are consistent with the standard model in economics, according to which individuals only care about their own personal incentives. Second, we study the robustness of the main treatment by manipulating the recipient of the tax revenue. We estimate treatments effects with the following regression:

$$\text{diff}_i = \alpha_0 + \alpha_1 \text{Low_Pay}_i + \alpha_2 \text{Redistributive_Tax}_i + \phi \mathbf{X}_i + \varepsilon_i \quad (3.5)$$

where

- diff_i — individual i 's belief about the percentage change in production between workers offered Bonus A and workers offered Bonus B.
- Low_Pay_i — an indicator for whether subject i was in the Low Pay treatment.
- $\text{Redistributive_Tax}_i$ — an indicator for whether subject i was in the redistributive tax treatment.
- \mathbf{X}_i — a vector of controls (we also report results without controls).⁹

⁹We include the following indicator variables as controls: gender (male/female), age (older/younger than 44 years old), ethnicity (white/nonwhite), three regional indicators, household income (above/below \$49,999), education (at least a 2-year college degree or not), employment (full-time employed or not), and political affiliation (Republican/Democrat). We also control for household size (coded continuously).

- ε_i — an individual-level error term. For all specifications, we use robust standard errors for inference.

According to the standard model in economics, workers should provide the same level of effort in all three treatments. We are interested in whether we can reject the null hypothesis that people’s beliefs are in line with the standard model in economics; i.e., whether $\alpha_1, \alpha_2 = 0$.

Do Republicans and Democrats have different beliefs?

In the second specification of interest, we investigate whether Republicans and Democrats have different beliefs about how personal and social incentives shape work effort:

$$\begin{aligned} \text{diff}_i = & \alpha_0 + \alpha_1 \text{Low_Pay}_i + \alpha_2 \text{Redistributive_Tax}_i + \alpha_3 \text{R}_i + \alpha_4 \text{Low_Pay}_i \times \text{R}_i \\ & + \alpha_5 \text{Redistributive_Tax}_i \times \text{R}_i + \phi \mathbf{X}_i + \varepsilon_i \end{aligned} \tag{3.6}$$

where R_i is an indicator for whether subject i is a Republican.

We are interested in whether we can reject the null hypothesis that Republicans and Democrats do not differ in their beliefs about how taxes affect work effort. We study this question in two different settings. We first look at whether Democrats and Republicans have different beliefs about how workers respond to *paying taxes to the government*; i.e., whether $\alpha_3 = 0$. We then use the Low Pay treatment to shed light on the underlying mechanisms as to why Republicans and Democrats may differ in their beliefs about how paying taxes to the government affects behavior. First, testing whether $\alpha_3 + \alpha_4 = 0$ allows us to answer whether Republicans and Democrats have different

beliefs about how the workers respond to a lower personal incentive. Second, testing whether $\alpha_3 + \alpha_5 = 0$ allows us to test whether Republicans and Democrats have different beliefs about the effect of a tax when the tax revenues are redistributed back to the workers as a lump-sum payment.

3.4.2 Analysis of equality–efficiency preferences

To analyze differences in equality–efficiency preferences, we run the following regression:

$$\text{amount}_i = \beta_0 + \beta_1 R_i + \phi \mathbf{X}_i + \varepsilon_i \quad (3.7)$$

where amount_i is the amount redistributed between the lucky and unlucky receivers and \mathbf{X}_i is a vector of controls which, in addition to demographics, include treatment indicators. We also estimate Equation (3.7) without demographic controls. We are primarily interested in whether Republicans and Democrats differ in the amount distributed; i.e., whether we can reject the null hypothesis that $\beta_1 = 0$.

3.5 Results: Experiment 1

This section presents our main results from the first experiment. While we do not discuss all the pre-specified specifications in the main text, Section 3.C of the appendix provides all the pre-specified tables in the order stated in the pre-analysis plan.

3.5.1 Beliefs about behavioral responses to taxation

In panel (a) of Figure 3.2, we study whether people have accurate beliefs about behavioral responses to taxation. We find that, on average across the treatments, the spectators believe that the workers will reduce production by 34.4 percent in response to a 50 percent reduction in the after-tax wage (an implied wage elasticity of 0.69). By contrast, the workers actually reduce production by 9.3 percent in response to a 50 percent reduction in the after-tax wage (an implied wage elasticity of 0.19). These *overestimations* of behavioral responses to changes in wages are of similar magnitude across treatments and are robust to different specifications. For instance, we find that 63.2 percent of our respondents overestimate behavioral responses across treatments.

The overestimation of labor supply elasticities may help to explain why Americans demand relatively low levels of redistribution despite high levels of income inequality (Piketty and Saez, 2003).¹⁰ One way to illustrate the importance of this overestimation is to apply the well-known formula for the Laffer optimum, $\tau = 1/(1 + e)$, where e is the labor supply elasticity with respect to the net-of-tax rate τ . While the worker elasticity of 0.19 implies a Laffer optimum of $\tau = 84$ percent, the spectators' preferred estimate of 0.69 imply a substantially lower Laffer optimum of $\tau = 59$ percent; i.e., the overestimation has quantitatively important implications for the policy debate on taxation if voters' preferences are guided by beliefs about incentive costs.

In panel (b) of Figure 3.2, we illustrate political differences in beliefs across

¹⁰Other explanations include beliefs about the mobility process (Bénabou and Ok, 2001), fairness concerns (Alesina and Angeletos, 2005), motivated beliefs (Tirole and Bénabou, 2006), and misconceptions about the income distribution (Kuziemko et al., 2015).

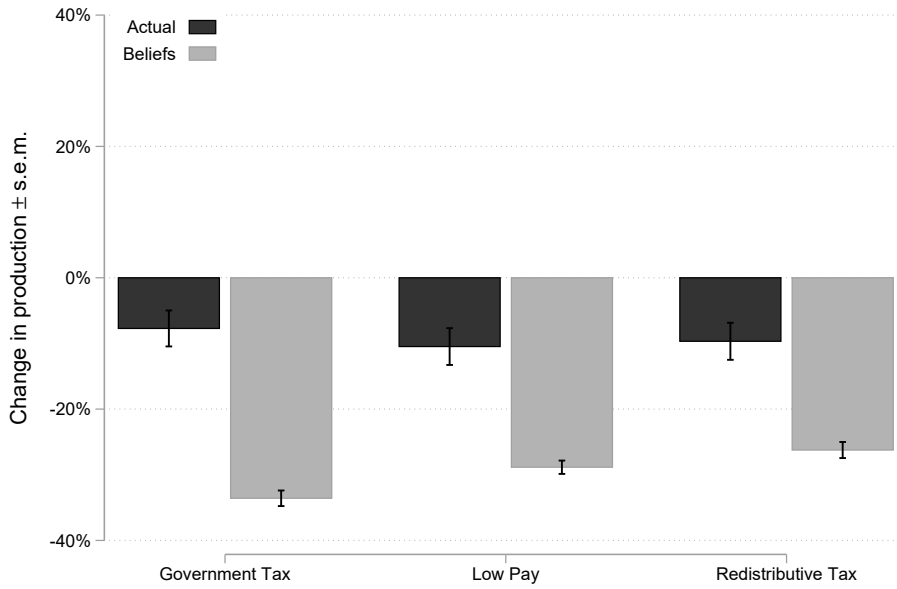
treatment. Strikingly, we observe no systematic differences in beliefs between Democrats and Republicans in either of the treatments (Figure 3.4 reports histograms of people's beliefs about production by treatment status and political affiliation). For instance, when estimating responses to a 50 percent government tax, Democrats and Republicans estimate on average that production will decrease by 34.7 and 34 percent, respectively.

In Table 3.1, we investigate in a regression framework whether beliefs differ between treatments and by political affiliation. Column 1 shows that the spectators believe that the workers will produce 0.11 of a standard deviation fewer points in the Government Tax treatment than in the Low Pay treatment. The difference in beliefs is highly significant ($p < 0.01$). This result demonstrates that the spectators believe that paying taxes to the government is more detrimental to production than paying workers a lower wage with the same personal incentives. Furthermore, the spectators believe that the workers produce 0.06 of a standard deviation more points in the Redistributive Tax treatment than in the Low Pay treatment ($p < 0.10$). Since there were 400 workers in each treatment, this finding is not mechanically driven by the fact that the workers get back a small portion of what they pay in taxes (i.e., they only get 25 cents back for every 100 dollars they pay in taxes). This result thus suggests that the spectators think the workers are socially motivated to pay taxes if the revenue benefits other workers. Column 2 shows that the estimated treatment effects are virtually unchanged when we include controls. Furthermore, column 2 confirms that there are no significant differences in beliefs between Democrats and Republicans ($p = 0.9$).

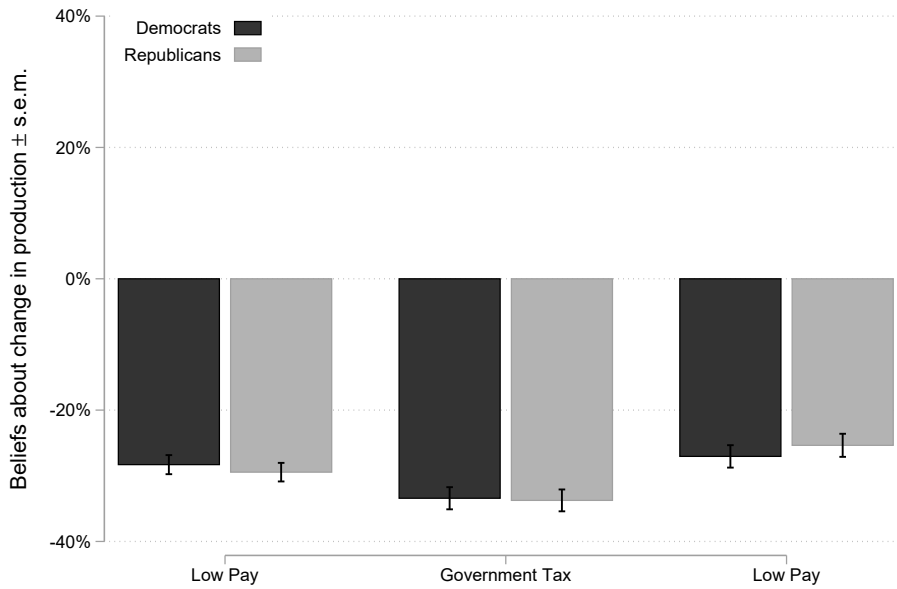
In columns 3–4 of Table 3.1, we include interaction terms between the treatments and people's political affiliation. Column 3 shows that there are no

Figure 3.2: Treatment effects: Experiment 1

(a) Beliefs versus actual



(b) Political differences in beliefs



Notes: Panel (a) shows the actual percentage change in production and beliefs about percentage change in production by each treatment. Panel (b) shows beliefs about the percentage change in production by treatment and political affiliation. Both panels use data from Experiment 1. The lines indicate the standard error of the mean.

significant differences in beliefs between Democrats and Republicans in either of the three treatments. In column 4, where we include controls, we find significant correlations between beliefs and race and between beliefs and education (both $p < 0.01$). We also find a marginal significant correlation between income and beliefs ($p < 0.1$). The correlation between education and beliefs is particularly pronounced: on average across treatments, college graduates estimate that workers produce one-third of a standard deviation more points than noncollege graduates.

In columns 5–8 of Table 3.1, we test for robustness by changing the outcome variable to an indicator for whether the respondents overestimate behavioral responses to taxation (i.e., estimating that workers produce fewer points than they actually do). While 63 percent overestimate behavioral responses in the Low Pay treatment, Column 5 shows that the spectators are five percentage points more likely to overestimate behavioral responses to taxation in the Government Tax treatment than in the Low Pay treatment ($p < 0.01$) and five percentage points less likely to overestimate behavioral responses to taxation in the Redistributive Tax treatment ($p < 0.01$). Column 6 shows that Republicans are three percentage points more likely than Democrats to overestimate behavioral responses. However, in columns 7–8, where we include interaction terms between the treatment and political affiliation, we find no significant differences in beliefs between Republicans and Democrats.

Table 3.1: Beliefs about behavioral responses to taxation

	Change in production (z-scored)			Overestimate behavioral responses				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Government Tax	-0.11*** (0.04)	-0.10*** (0.04)	-0.12** (0.05)	-0.11** (0.05)	0.05** (0.02)	0.04** (0.02)	0.05** (0.03)	0.05* (0.03)
Redistributive Tax	0.06* (0.04)	0.07* (0.04)	0.03 (0.05)	0.04 (0.05)	-0.05** (0.02)	-0.05*** (0.02)	-0.04 (0.03)	-0.04 (0.03)
Republican		-0.01 (0.03)	-0.03 (0.05)	-0.04 (0.05)		0.03** (0.02)	0.04 (0.03)	0.04 (0.03)
Government Tax × Republicans			0.02 (0.07)	0.02 (0.07)			-0.01 (0.04)	-0.01 (0.04)
Redistributive Tax × Republicans			0.07 (0.07)	0.06 (0.07)			-0.02 (0.04)	-0.02 (0.04)
Male		-0.01 (0.03)		-0.01 (0.03)		-0.01 (0.02)		-0.01 (0.02)
Age > 45 years old		-0.03 (0.03)		-0.03 (0.03)		-0.00 (0.02)		-0.00 (0.02)
White		0.11*** (0.04)		0.10*** (0.04)		-0.03 (0.02)		-0.03 (0.02)
Income > 45,000 USD		0.06* (0.03)		0.06* (0.03)		-0.02 (0.02)		-0.02 (0.02)
2-year college degree		0.33*** (0.06)		0.33*** (0.06)		-0.09*** (0.02)		-0.09*** (0.02)
Full-time employment		0.01 (0.03)		0.01 (0.03)		0.00 (0.02)		0.00 (0.02)
Government Tax + Government Tax × Republicans			-0.10** (0.05)	-0.09* (0.05)			0.04 (0.03)	0.04 (0.03)
Redistributive Tax + Redistributive Tax × Republicans			0.10* (0.05)	0.09* (0.05)			-0.06** (0.03)	-0.06** (0.03)
N	4128	4128	4128	4128	4128	4128	4128	4128

Note: In columns 1–4, the dependent variable is beliefs about change in production (standardized). In columns 5–8, the dependent variable is an indicator for overestimating the change in production. Controls include household size and regional indicators in addition to the coefficients displayed in the table.

* p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses.

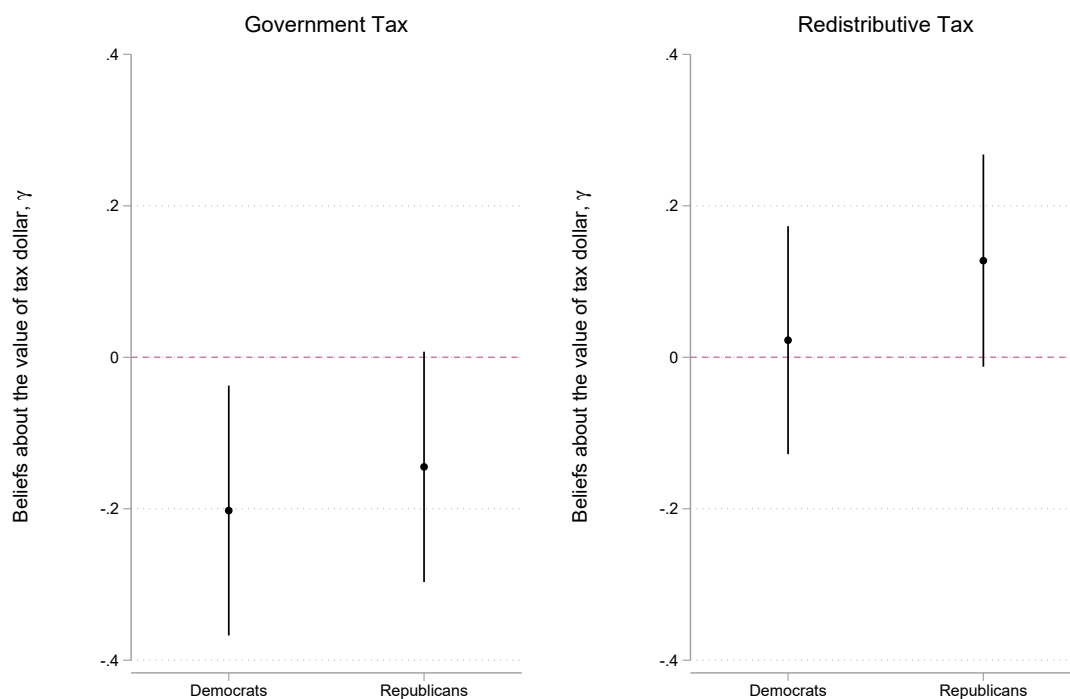
By analyzing the worker data, we can also look at whether Democrats and Republicans actually differ in their behavioral responses to taxation. As shown in Table 3.7, we find no significant differences between Democrats and Republicans in their actual behavioral responses to taxation. We also asked the workers how much they believed other workers within their treatment had produced. For the workers, we also fail to detect significant differences in beliefs between Republicans and Democrats. But in contrast to the spectators, the workers do not overestimate behavioral responses to taxation. However, these result might be partly mechanical given that the high correlation between beliefs about the production of others and own production (correlation coefficient of 0.76).

To interpret the quantitative importance of beliefs about the social motivation of workers, we can derive structural estimates of how much the spectators believe the workers would be willing to give up to increase tax revenue by \$1. We estimate these beliefs separately for Republicans and Democrats. By our assumptions from Section 3.3.1, Figure 3.3 shows that Democrats and Republicans believe that the workers would be willing to give up 18 cents ($p < 0.05$) and 15 cents ($p < 0.10$), respectively, to *reduce* government tax revenue by \$1. By contrast, they believe the workers would be willing to give up 4 cents ($p = 0.77$) and 14 cents ($p < 0.10$), respectively, to *increase* worker tax revenue by \$1.¹¹

Given the estimated uncovered in this section, we can summarize our first

¹¹DellaVigna and Pope (2018) use a similar model to study the motivations of workers in different settings. In their model, they include an additional parameter to capture non-monetary rewards from working (i.e., “intrinsic motivation”). This allows the model to predict a non-zero effort level in the absence of monetary incentives, whereas our model would predict zero effort in the absence of monetary incentives. If we allowed for non-monetary rewards from working in the model, we would need an additional treatment to pin down the cost of providing effort. However, we intentionally choose a boring and repetitive task that we did not expect workers to be intrinsically motivated to work on.

Figure 3.3: Structural estimates of beliefs about the value of a tax dollar



Notes: The figure shows structural estimates of beliefs about the value of a tax dollar by treatment and political affiliation. Lines indicate 95 percent confidence intervals. We assume that beliefs are formed by Equation (3.1) and that $c(e)$ in Equation (3.2) is quadratic (i.e., on the form ae^2 , where a is a constant that is identified by the group-level by mean differences in beliefs between the Low Pay and Government Tax treatments).

set of results as follows:

Result 1. *There is systematic overestimation of behavioral responses to taxation in our sample. While the estimated labor supply elasticity for workers is 0.19, the spectators estimate a labor supply elasticity of 0.69.*

Result 2. *Beliefs about behavioral responses to taxation are not in line with the standard model in economics, which predicts that workers only care about their personal incentives. Americans think that paying taxes to the government is more detrimental to production than paying workers a lower wage with the same personal incentives. By contrast, our evidence suggests that Americans think people are socially motivated to pay taxes to the government if the taxes will benefit other workers.*

Result 3. *We find no systematic differences between Republicans and Democrats in their beliefs about behavioral responses to taxation. We also find no systematic political differences in beliefs about the cost of providing effort or the social motivation of workers.*

3.5.2 Equality–efficiency preferences

Figure 3.5 reports the distribution of people’s redistributive choices by their political views. We document a striking political difference in equality–efficiency preferences: Republicans are much less willing than Democrats to redistribute income between receivers. For instance, we find that we find that 54.4 percent of Republicans choose not to redistribute any income—and thus keep the (7:1) income distribution between the lucky and unlucky receiver—compared to 43.3 percent of Democrats. Republicans are thus more likely than Democrats to assign maximum weight on efficiency relative to equality.

Table 3.2: Equality–efficiency preferences

	Amount redistributed			Amount redistributed > 0		
	(1)	(2)	(3)	(4)	(5)	(6)
Republican	-0.22*** (0.03)	-0.22*** (0.03)	-0.20*** (0.03)	-0.11*** (0.02)	-0.11*** (0.02)	-0.10*** (0.02)
Male			-0.08*** (0.03)			-0.03* (0.02)
Age > 45 years old			-0.01 (0.03)			0.01 (0.02)
White			-0.13*** (0.04)			-0.09*** (0.02)
Income > 45,000 USD			-0.11*** (0.03)			-0.06*** (0.02)
2-year college degree			-0.11** (0.05)			-0.07*** (0.03)
Full-time employment			-0.03 (0.03)			0.01 (0.02)
N	4128	4128	4128	4128	4128	4128
R-sq	0.012	0.012	0.023	0.012	0.013	0.026
Treatment indicators	No	Yes	Yes	No	Yes	Yes

Note: The dependent in columns 1–3 (amount redistributed between workers) has been z-scored. Controls include, in addition to the coefficients displayed in the table, household size and regional indicators. The treatment indicators are jointly insignificant in a regression on the amount redistributed ($p=0.31$).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

In Table 3.2, we investigate in a regression framework whether equality-efficiency preferences differ between Republicans and Democrats. In columns 1–3, we standardize the amount redistributed between the lucky and unlucky receivers. Column 1 shows that Republicans on average redistribute a 0.22 of a standard deviation lower amount than Democrats do. Columns 2 and 3 show that this estimate is virtually unaffected when we include controls for treatment indicators (column 2) as well as demographic controls (column 3). Furthermore, column 3 shows that being Republican is the strongest predictor of the amount redistributed. We also observe negative correlations between the amount redistributed and being male ($p < 0.01$), white ($p < 0.01$), having high income ($p < 0.01$), and having a college degree ($p < 0.05$).

In columns 4–6 of Table 3.1, we test for robustness by changing the outcome variable to an indicator for whether the respondents choose to redistribute any income at all between the lucky and unlucky receivers. Column 4 shows that Republicans are 11 percentage points less likely than Democrats to redistribute incomes ($p < 0.01$). This estimate is not sensitive to inclusion of controls (columns 5–6). The main result from this section can be summarized as follows:

Result 4. *We find systematic differences between Republicans and Democrats in their equality–efficiency preferences. In a real redistributive setting with a cost of redistribution, Republicans are 11 percentage points less likely than Democrats to redistribute incomes.*

3.6 Experiment 2: Introducing a political context

One reason that Republicans and Democrats tend to express very polarized beliefs in opinion surveys (e.g., Newport, 2016) could be that surveys are often framed in a political context. In the second experiment, we explore whether a political context polarizes beliefs by priming the respondents about the political debate on taxation and party views on behavioral responses to taxation. Importantly, we also assess the robustness of some of our main findings from the first experiment by replicating the Government Tax treatment and the elicitation of equality–efficiency preferences in the second experiment.

3.6.1 Sample and design

We submitted a pre-analysis plan to the AEA RCT Registry under the same trial as the first experiment prior to starting the data collection. We recruited 5,579 spectators to participate in the second experiment.¹² As in the first experiment, we exclusively recruited Democrats and Republicans from Research Now's *Political Panel*. The sample was similarly recruited to match the general US population in terms of gender, age, income, race, and geography. Column 3 of Table 3.6 provides the summary statistics.

The second round included two treatments in addition to the Government Tax treatment: i.e., the Motivated Beliefs and Group Identity treatments. In these treatments, we prime people about the political debate on taxation

¹²In the main analysis, we focus on the 5,631 who spent at least 15 seconds on the belief elicitation page. This restriction does not change any of our main results except for a correlation between beliefs and policy views for Republicans; all main tables are replicated in Section 3.B of the appendix on the full sample.

and party views on behavioral responses to taxation, respectively. In the Motivated Beliefs treatment, we emphasize that a key issue in the political debate on taxation is how taxes “affect people’s willingness to work hard.” In the Group Identity treatment, we emphasize that political parties disagree about how taxes affect people’s willingness to work hard and that “the Republican Party more often than the Democratic Party claims that taxes discourage people from working hard.” These treatments allow us to provide evidence of whether people’s beliefs about behavioral responses to taxation are motivated by a desire to justify their existing attitudes or motivated by a desire to enhance their political group identity, respectively (Alesina and Giuliano, 2011; Bénabou, 2015).

After the belief elicitation, we elicit equality–efficiency preferences in the same way as in the first experiment. To incentivize choices in this task, we recruit an additional 1,194 receivers from MTurk to participate in the lottery. At the end of the experiment, we elicit attitudes towards redistribution of income in society. We first ask the same question as in Experiment 1; i.e., whether they think “a society should aim to equalize incomes.” We then ask a new question that directly addresses support for redistribution through the tax system: “Where would you rate yourself on a scale from 1 to 10, where 1 means “I think the US should increase income taxes to reduce inequality” and 10 means: “I think the US should *not* increase income taxes to reduce inequality.”

3.6.2 Beliefs and preferences: Replication and robustness

In the second experiment, we restricted the design to different versions of the Government Tax treatment. Before analyzing treatment effects of introducing a political context, we first replicate our results on beliefs about government

taxes and equality-efficiency preferences from the first experiment.

Columns 1–6 of Table 3.3 show the results for beliefs about behavioral responses to taxation (the Government Tax treatment with a nonpolitical context). Column 1, where we regress beliefs on the Republican indicator, and Column 2, where we include additional controls, show that there was virtually no differences in beliefs between Republicans and Democrats in the first experiment. Columns 3 and 4 show that we replicate these results in the second experiment; i.e., we find no evidence of systematic political differences in the second experiment either. We also replicate our finding that college graduates are less likely than noncollege graduates to overestimate behavioral responses to taxation. In columns 5–6, we pool results from the two experiments and again find no evidence of political differences in beliefs. Overall, these results provide strong evidence that we can reject large differences in beliefs about behavioral responses to taxation between Republicans and Democrats.

Columns 7–12 of Table 3.3 show the corresponding estimates for equality-efficiency preferences (the amount redistributed). We see that there is a robust and strong correlation between Republicans and the amount distributed across specifications and experiments. Furthermore, the point estimates for the Republican indicator are also very similar across specifications. We also see that the two other background characteristics that significantly correlate with the amount redistributed in the first experiments—i.e., white and income—are also the only significant correlations besides political views in the second experiment.

Table 3.3: Beliefs and preferences: Replication

	Beliefs about behavioral responses to taxation				Equality-efficiency preferences (amount redistributed)							
	(1) Exp. 1	(2) Exp. 1	(3) Exp. 2	(4) Exp. 2	(5) Pooled	(6) Pooled	(7) Exp. 1	(8) Exp. 1	(9) Exp. 2	(10) Exp. 2	(11) Pooled	(12) Pooled
Republican	-0.01 (0.05)	-0.03 (0.05)	-0.06 (0.05)	-0.08 (0.05)	-0.04 (0.04)	-0.06 (0.04)	-0.29*** (0.05)	-0.27*** (0.05)	-0.28*** (0.05)	-0.24*** (0.05)	-0.28*** (0.04)	-0.25*** (0.04)
Male		-0.05 (0.05)		0.04 (0.05)		-0.01 (0.04)		-0.07 (0.05)		-0.03 (0.05)		-0.05 (0.04)
Age > 45 years old		0.03 (0.06)		-0.20*** (0.06)		-0.09** (0.04)		0.03 (0.06)		-0.03 (0.06)		0.00 (0.04)
White		0.14** (0.07)		0.12 (0.07)		0.12** (0.05)		-0.24*** (0.07)		-0.19*** (0.06)		-0.22*** (0.05)
Income > 45,000 USD		0.05 (0.06)		0.03 (0.06)		0.05 (0.04)		-0.14** (0.06)		-0.14*** (0.06)		-0.14*** (0.04)
2-year college degree		0.31*** (0.09)		0.21** (0.09)		0.25*** (0.06)		-0.07 (0.09)		-0.14 (0.09)		-0.11* (0.06)
Full-time employment		-0.01 (0.06)		-0.06 (0.06)		-0.04 (0.04)		0.04 (0.06)		0.03 (0.05)		0.03 (0.04)
N	1372	1372	1524	1524	2896	2896	1372	1372	1524	1524	2896	2896
R-sq	0.000	0.015	0.001	0.018	0.000	0.013	0.021	0.036	0.019	0.040	0.020	0.036

Note: Both beliefs and preferences have been z-scored. The controls were prespecified and include, in addition to the coefficients displayed in the table, an indicator for household size and regional indicators. We only include respondents from the Government Tax treatments. We include experiment fixed effects in regressions on the pooled sample.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

We also show that our main results are robust to using dummy variables for beliefs and preferences (Table 3.10) and also robust to reweighting the data based on six cells based on age, race, and education to match the underlying demographics of Republicans and Democrats (Table 3.11). Overall, the second experiment provides strong evidence that our results on equality–efficiency preferences are robust.

3.6.3 Treatment effects of a political context

We now investigate whether a political context causes polarization in beliefs and preferences between Republicans and Democrats. To test this question, we estimate the following regression:

$$y_i = \rho_0 + \rho_1 R_i + \rho_2 \text{Motivated_Beliefs}_i + \rho_3 \text{Group_Identity}_i + \rho_4 \text{Motivated_Beliefs}_i \times R_i + \rho_5 \text{Group_Identity}_i \times R_i + \phi \mathbf{X}_i + \varepsilon_i \quad (3.8)$$

where y_i is the outcome of interest; $\text{Motivated Beliefs}_i$ and Group Identity_i are treatment indicators; and \mathbf{X}_i is a vector of controls. The main coefficients of interest are ρ_4 and ρ_5 ; i.e., whether the treatments cause polarization in beliefs between Republicans and Democrats.

Table 3.4 presents the results from these regressions. Columns 1 and 2 show that the Motivated Beliefs treatment, in which we emphasized that a key issue in the political debate on taxes is how they affect people’s willingness to work hard, made Democrats and Republicans alike believe in a higher incentive cost of taxation. One interpretation of this result is that this treatment mainly succeeded in making the negative aspects of taxation more salient, which triggered the same response from both Democrats and Republicans. In the

Group Identity treatment, in which we highlighted that the Democratic Party and Republican Party had different views on whether taxes discourage people from working hard, we see no treatment effects on beliefs. That priming people about party views on taxation does not polarize beliefs may reflect that we had an incentivized belief elicitation which made it costly to engage in partisan motivated reasoning (Bullock et al., 2015).

Columns 3 and 4 of Table 3.4 show whether the treatments affected people's equality–efficiency preferences. Interestingly, we find evidence of political polarization in both treatments. In the Motivated Beliefs treatment, political polarization increases by 0.14 of a standard deviation ($p < 0.10$). This effect is about equally driven by Democrats redistributing more and Republicans redistributing less than in the nonpolitical context. We observe a similar effect size for the interaction effect in the Group Identity treatment, which increases political polarization by 0.15 of a standard deviation ($p < 0.05$). This effect is mostly driven by Democrats becoming more in favor of redistribution. Given these estimates, our fifth result can be summarized as follows:

Result 5. *Priming respondents about the political debate on taxation and party views on behavioral responses to taxation increases political polarization in equality–efficiency preferences, but does not cause political polarization in beliefs.*

We also find evidence of polarization in policy preferences on whether to increase income taxes to reduce income inequality (Table 3.8). In the Motivated Beliefs treatment, political polarization increases by 0.10 of a standard deviation, but the effect is not statistically significant ($p = 0.13$). For the Group Identity treatment, we see a larger and statistically significant increase in polarization equal to 0.13 of a standard deviation ($p < 0.05$). This effect is about equally driven by Democrats becoming more in favor of higher taxes

Table 3.4: Beliefs and preferences: Political context

	Beliefs		Preferences	
	(1)	(2)	(3)	(4)
Motivated Beliefs	-0.10*** (0.04)	-0.11** (0.05)	0.01 (0.04)	0.08 (0.05)
Group Identity	-0.00 (0.04)	-0.01 (0.05)	0.05 (0.04)	0.13** (0.05)
Republicans	-0.05 (0.03)	-0.07 (0.05)	-0.35*** (0.03)	-0.25*** (0.05)
Motivated Beliefs × Republicans		0.03 (0.07)		-0.14* (0.07)
Group Identity × Republicans		0.02 (0.07)		-0.15** (0.07)
Male	0.01 (0.03)	0.01 (0.03)	-0.05 (0.03)	-0.05 (0.03)
Age > 45 years old	-0.10*** (0.03)	-0.10*** (0.03)	-0.02 (0.03)	-0.02 (0.03)
White	0.09** (0.04)	0.09** (0.04)	-0.12*** (0.04)	-0.13*** (0.04)
Income > 45,000 USD	0.04 (0.03)	0.04 (0.03)	-0.09*** (0.03)	-0.10*** (0.03)
2-year college degree	0.26*** (0.05)	0.26*** (0.05)	-0.09* (0.05)	-0.09* (0.05)
Full-time employment	-0.06* (0.03)	-0.06* (0.03)	-0.04 (0.03)	-0.04 (0.03)
N	4353	4353	4353	4353
R-sq	0.014	0.014	0.042	0.043

Note: *Beliefs* refers to beliefs about behavioral responses to taxation (points produced in the Government Tax treatment) and *Preferences* refers to amount redistributed. Both variables have been standardized. Controls include race, gender, income, household size, region, employment, and education.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

and Republicans becoming less in favor of higher taxes. The effect size corresponds to almost one fifth of the Republican–Democrat difference in views on whether to increase taxes to reduce inequality. This result clearly demonstrates that people form their policy views on taxation to enhance their political group identity to some extent.

3.7 Demand for redistribution: The role of beliefs and preferences

We have uncovered significant heterogeneity in both beliefs about behavioral responses to taxation and equality–efficiency preferences. We now turn to the question of whether the heterogeneity we observe in beliefs and preferences is associated with people’s views on redistribution. We assess views on redistribution with two different measures: whether respondents think a “society should aim to equalize incomes” (asked in both experiments) and whether they think the US “should increase income taxes to reduce inequality” (only asked in the second experiment).

3.7.1 Empirical strategy

To analyze how our incentivized measures of beliefs about behavioral responses to taxation and equality–efficiency preferences relate to people’s policy views on redistribution, we estimate the following OLS equation:

$$\text{policy_view}_i = \delta_0 + \delta_1 \text{beliefs}_i + \delta_2 \text{amount}_i + \phi \mathbf{X}_i + \varepsilon_i$$

where policy_view is policy views on redistribution, beliefs_i is belief about the difference in production between workers offered Bonus A or Bonus B, amount_i is the amount distributed between the lucky and unlucky workers, and \mathbf{X}_i is a vector of controls, which include treatment indicators. In the regression, we standardize all variables for ease of interpretation. For robustness, we also report an alternative specification where we use indicator variables for beliefs and preferences: i.e., whether people respectively overestimate behavioral responses to wage changes and whether they redistribute any income between the two receivers.

3.7.2 Main results on demand for redistribution

In columns 1–3 of Table 3.5, we regress views on whether a society should aim to equalize incomes on our measures of beliefs and preferences. Column 1 shows that people’s beliefs about behavioral responses fail to predict people’s views of equalization of incomes. By contrast, people’s equality–efficiency preferences are strongly associated with their support for equalization. For instance, those who choose to redistribute income between the receivers are 0.38 of a standard deviation more in favor of equalization of incomes in society than those who choose not to redistribute any income ($p < 0.01$). By comparison, the difference between Republicans and Democrats in views on equalization amounts to 0.77 of a standard deviation.

In columns 2 and 3 of Table 3.5, we show results separately for Republicans and Democrats. For Democrats and Republicans alike, we find that beliefs fail to explain differences in views on equalization of incomes. By contrast, equality–efficiency preferences are strongly associated with views on redistribution. Interestingly, the relationship between equality–efficiency

preferences and views of equalization of incomes is even stronger for Republicans than for Democrats ($p < 0.01$). Furthermore, comparing results in Panel A and Panel B, we find that the patterns we uncover are robust to whether we use continuous or indicator measures for beliefs and preferences.

Table 3.5: Views on redistribution – beliefs and preferences

	Should aim to equalize incomes in society			Should increase taxes to reduce inequality		
	(1) Pooled sample	(2) Democrats	(3) Republicans	(4) Pooled sample	(5) Democrats	(6) Republicans
Panel A						
Beliefs (points produced)	-0.00 (0.01)	-0.00 (0.01)	-0.01 (0.02)	0.04*** (0.01)	0.10*** (0.02)	-0.02 (0.02)
Preferences (amount)	0.18*** (0.01)	0.14*** (0.01)	0.22*** (0.02)	0.11*** (0.01)	0.09*** (0.02)	0.12*** (0.02)
Panel B						
Beliefs (overestimate)	-0.03 (0.02)	-0.02 (0.03)	-0.04 (0.03)	-0.12*** (0.03)	-0.22*** (0.04)	0.01 (0.04)
Preferences (amount > 0)	0.38*** (0.02)	0.27*** (0.03)	0.48*** (0.03)	0.22*** (0.03)	0.16*** (0.04)	0.27*** (0.04)
N	8481	4318	4163	4353	2190	2163

Note: The dependent variable in columns 1–3 is support for equalization of incomes in society. The dependent variable in columns 4–6 is support for higher income taxes to reduce income inequality. In **Panel A**, *Beliefs* refers to beliefs about behavioral responses to government taxation and *Preferences* refers to the amount redistributed in the spectator decision. In **Panel B**, *Beliefs* is an indicator for overestimating behavioral responses to government taxation and *Preferences* is an indicator for redistributing a positive amount in the spectator decision. We include respondents from the Government Tax treatment. All variables, except for the indicators in Panel B, have been z-scored. Controls include race, gender, income, household size, region, employment, and education.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

In columns 4–6 of Table 3.5, we regress views on whether the US should increase income taxes to reduce income inequality on our measures of beliefs and preferences. Column 4 shows that beliefs about behavioral responses do predict policy support for higher taxes: a one-standard deviation change in beliefs is associated with a 0.04 standard deviation change in support for higher taxes ($p < 0.01$). Columns 5 and 6, where we split the sample by

Democrats and Republicans, show that this correlation is entirely driven by Democrats; for Republicans, there is no clear association between beliefs and policy views on taxes. By contrast, equality–preferences robustly predict policy views across all specifications. Furthermore, we find that these patterns are robust across treatments (Table 3.12). Given these estimates, our last result can be described as follows:

Result 6. *Equality–efficiency preferences are strongly associated with policy views on redistribution for both Democrats and Republicans. By contrast, beliefs about behavioral responses to taxation are only associated with policy views for Democrats.*

3.8 Conclusion

In this chapter, we provide novel evidence of the role of beliefs about behavioral responses to taxation and equality–efficiency preferences in driving people’s demand for redistribution. Eliciting incentivized measures of beliefs and preferences, we find no evidence of large differences in beliefs about incentive costs between Republicans and Democrats, but we do find strong evidence of large differences in equality–efficiency preferences. Furthermore, while equality–efficiency preferences are strongly associated with people’s policy views on redistribution, we find that beliefs about efficiency costs are only correlated with policy views for Democrats.

Overall, our results strongly suggest that equality–efficiency preferences are more important than beliefs about incentive costs for understanding political disagreements about redistribution. Future research should explore whether our main result of no political differences in beliefs is robust across different settings. Furthermore, while we fail to detect differences between

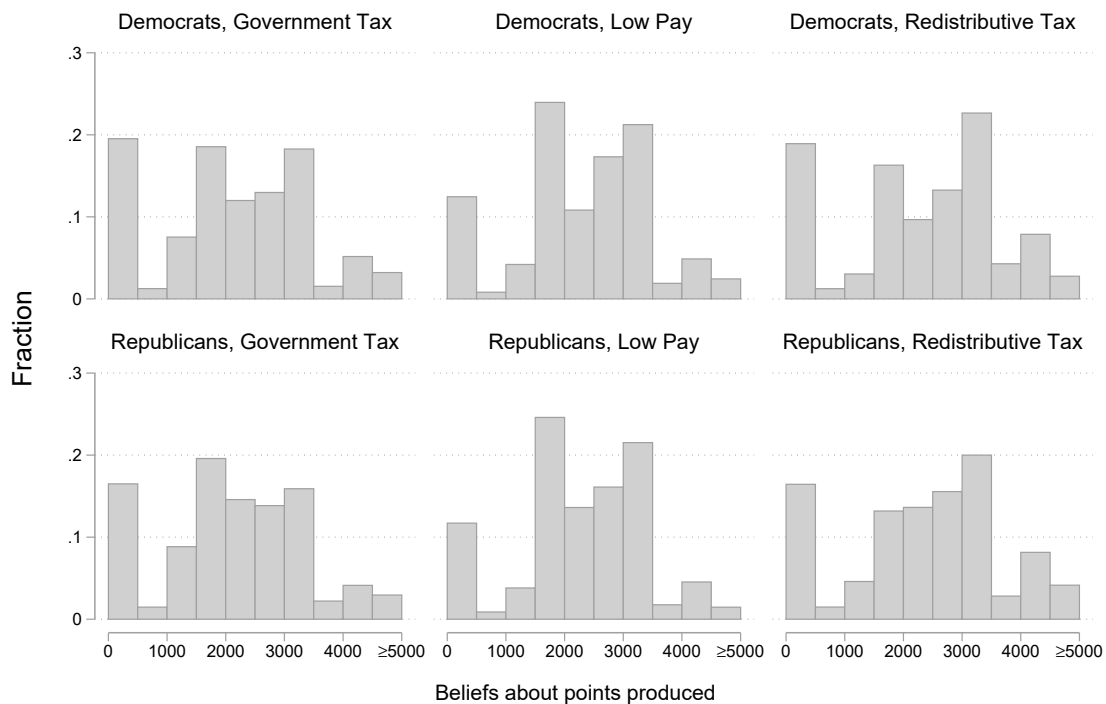
Republicans and Democrats in their beliefs about behavioral responses to taxation, this is not to say that Democrats and Republicans necessarily have identical beliefs about all aspects relevant for the debate on redistribution. In particular, beliefs that interact with people's perceptions of fairness—such as trust in business elites (Di Tella et al., 2017) or the perceived deservingness of the poor (Alesina et al., 2018a)—may also be instrumental to understand why voters have conflicting views on redistribution. An avenue for future research may be to further explore the importance of interactions between beliefs and perceptions of fairness in driving demand for redistribution.

Summary of the Appendices

Section 3.A provides additional figures and tables. Section 3.B replicates the main tables with the full sample (i.e., without excluding respondents who spent fewer than 15 seconds on the belief elicitation page). Section 3.C provides all pre-specified tables exactly as pre-specified. Instructions are provided in the pre-analysis plans.

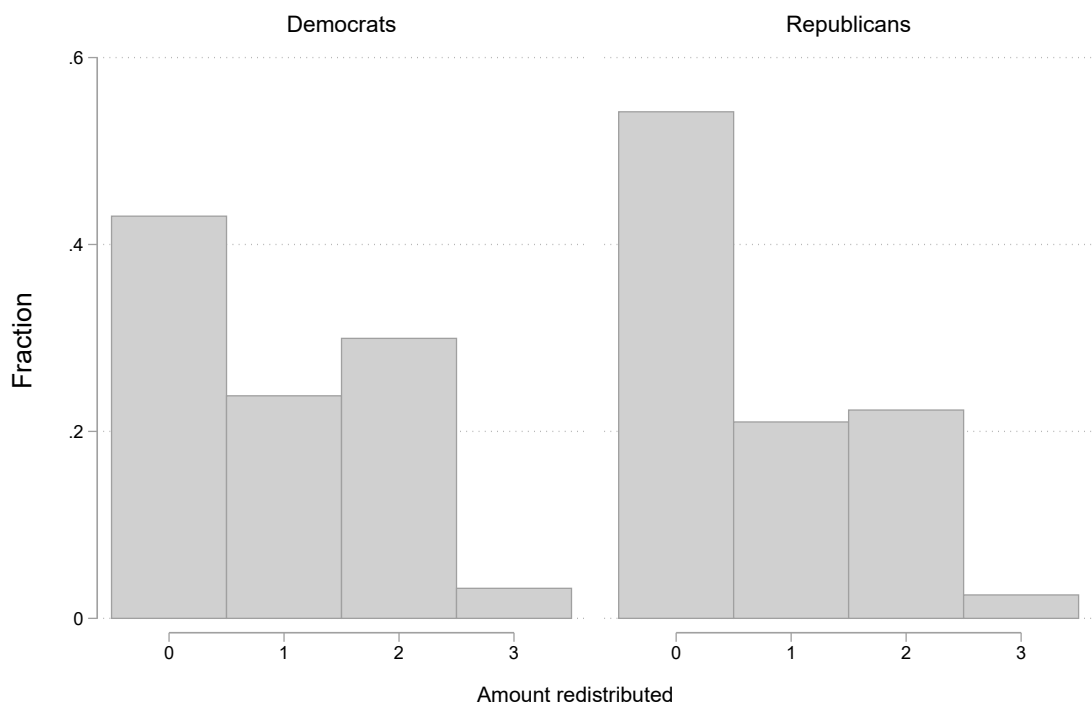
3.A Additional figures and tables

Figure 3.4: Distribution of beliefs: Experiment 1



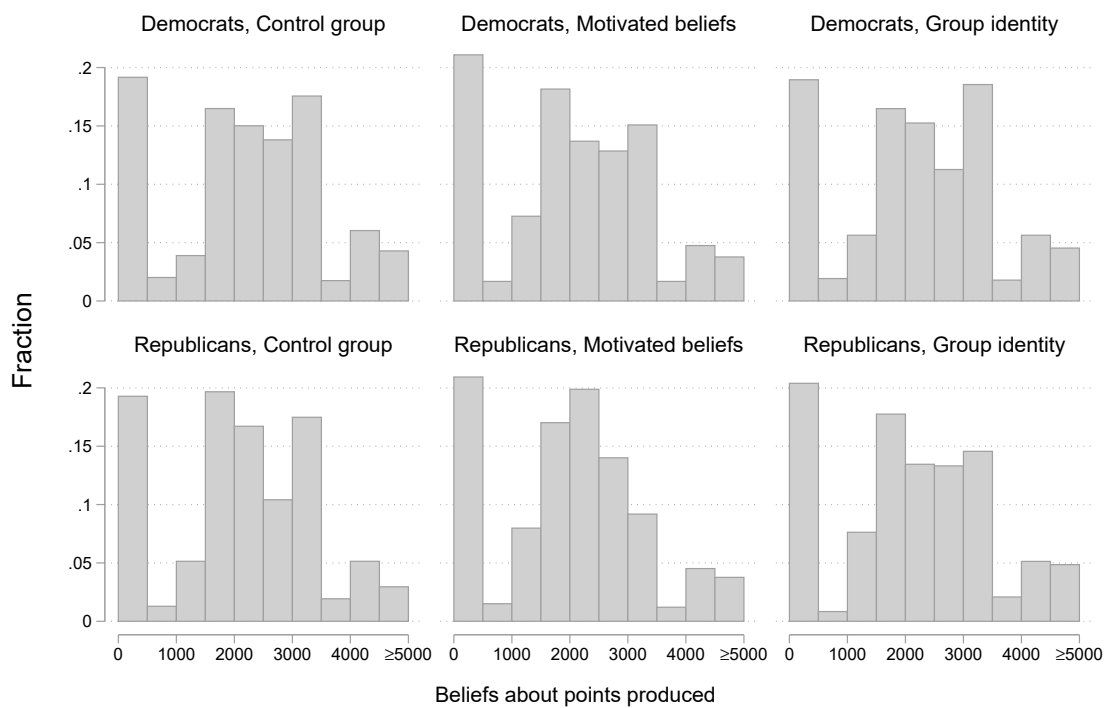
Notes: The figure shows the distribution of beliefs about the produced points by treatment and political affiliation.

Figure 3.5: Distribution of equality–efficiency preferences: Experiment 1



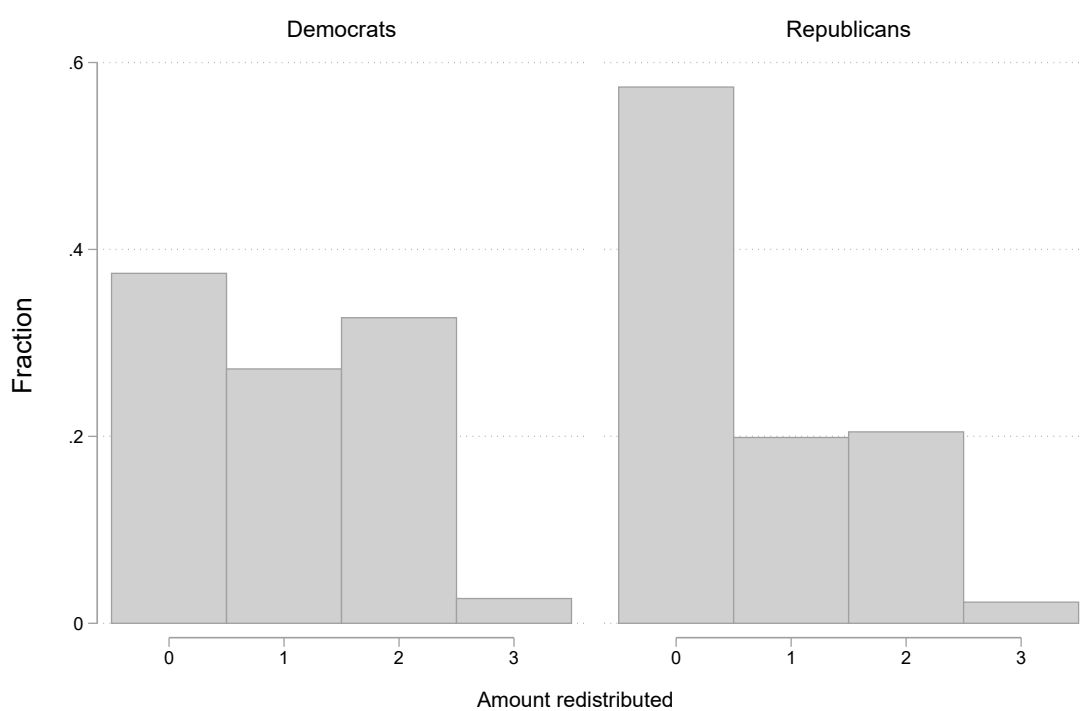
Notes: The figure shows the distribution of the amount redistributed between the lucky and unlucky workers by political affiliation. The treatment indicators are jointly insignificant in a regression on the amount redistributed ($p=0.25$).

Figure 3.6: Distribution of beliefs: Experiment 2



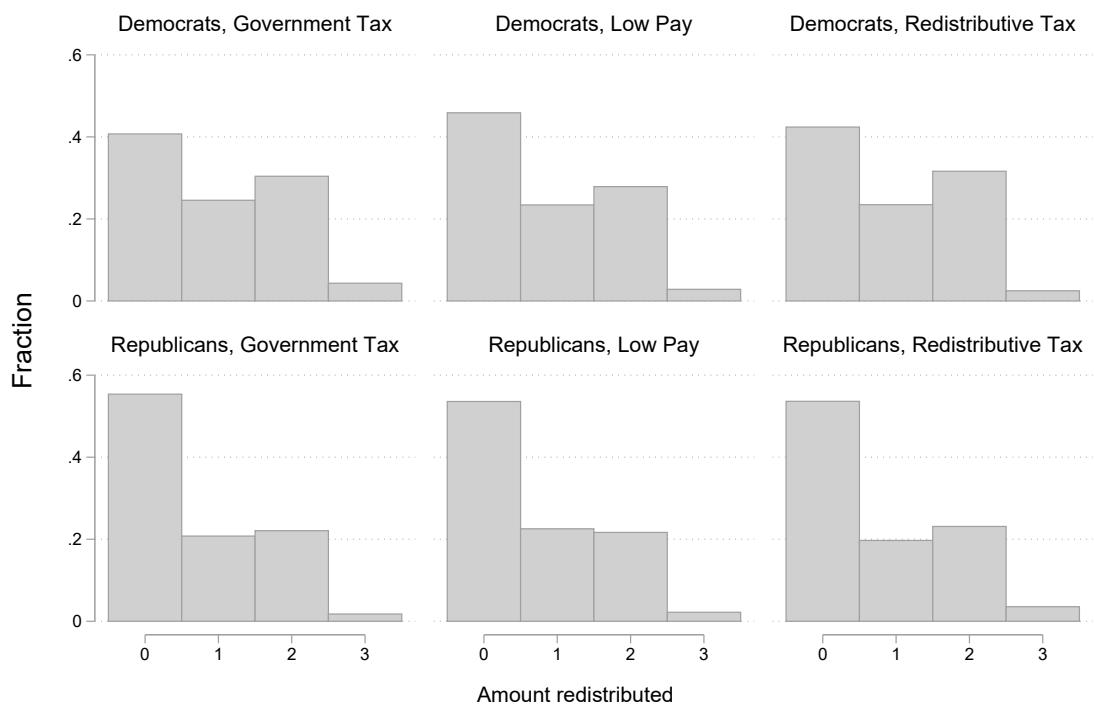
Notes: The distribution of beliefs about points produced according to treatment and political affiliation.

Figure 3.7: Distribution of equality–efficiency preferences: Experiment 2



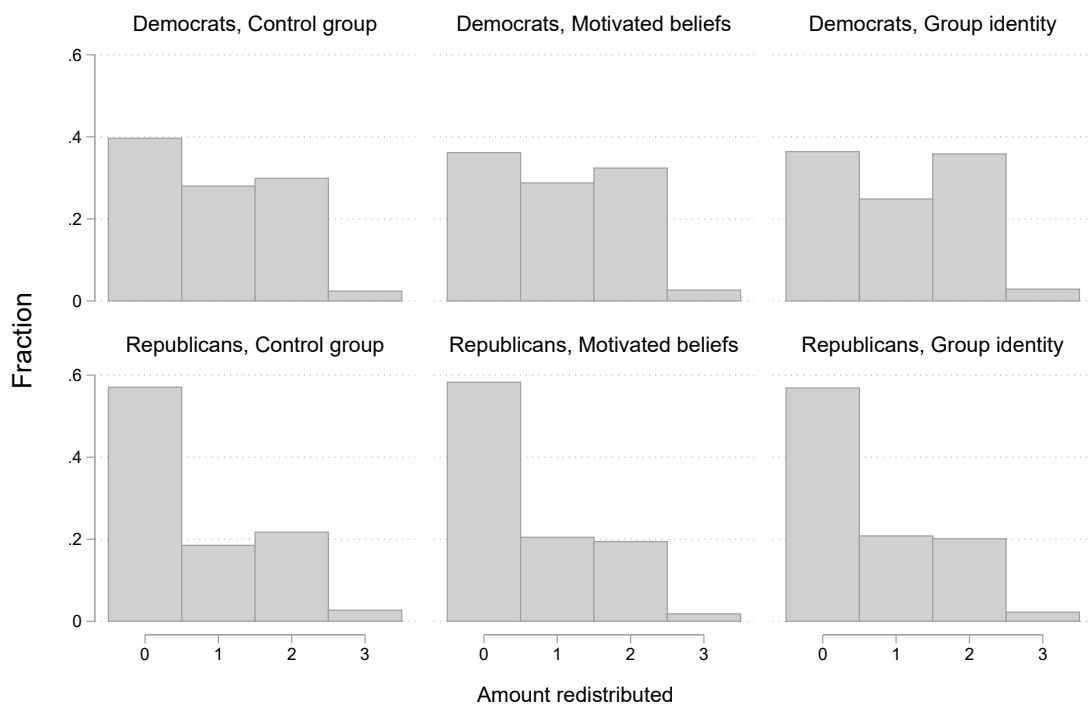
Notes: The figure shows the distribution of the amount redistributed between the lucky and unlucky workers by political affiliation.

Figure 3.8: Distribution of equality–efficiency preferences: Experiment 1



Notes: The figure shows the distribution of the amount redistributed between the lucky and unlucky workers by treatment and political affiliation.

Figure 3.9: Distribution of equality–efficiency preferences: Experiment 2



Notes: The figure shows the distribution of the amount redistributed between the lucky and unlucky workers by treatment and political affiliation.

0020

You gain 1 point for checking off an even number. You lose 1 point for checking off an odd number.

84	40	9	23	2	4	98	45	78	26	55	71	84	7	64	77	27	35	77
82	68	31	45	17	87	73	77	84	49	49	3	52	92	78	29	91	12	90
69	78	42	55	100	5	6	32	14	96	67	93	40	6	84	88	29	59	52
37	10	96	99	48	65	87	83	24	4	24	36	98	60	28	42	67	49	33
81	31	28	88	79	57	63	48	24	22	56	87	46	44	13	95	72	90	41
99	97	46	13	37	66	92	80	99	29	23	34	66	59	85	99	25	90	46
77	71	25	32	97	5	3	80	92	60	98	64	51	42	44	67	5	56	60
60	85	11	60	72	16	11	59	80	70	72	16	54	93	5	26	21	4	52
40	46	24	96	82	13	95	58	44	76	82	53	39	61	21	38	64	25	31
88	24	14	37	2	40	91	88	24	99	82	6	68	93	34	49	6	3	53
84	2	39	57	20	74	65	95	32	15	11	44	68	4	14	59	31	19	46
24	83	46	99	16	87	90	11	18	70	42	33	60	33	71	79	86	74	22
41	85	21	99	11	90	77	80	86	74	90	85	9	69	14	65	98	60	9
59	21	22	44	42	92	35	40	21	54	72	56	31	17	39	8	28	46	84
66	62	71	50	98	38	21	25	65	30	65	6	98	66	79	85	27	73	10
21	12	87	36	81	23	37	94	63	62	69	43	75	13	2	4	46	91	58
86	12	62	13	20	61	72	70	63	56	44	45	34	85	72	58	37	35	90
87	17	68	91	85	21	38	76	69	11	58	60	70	16	25	11	35	55	92
65	74	94	98	94	97	74	60	90	98	99	72	91	23	55	11	30	14	21

>>

Figure 3.10: Example of a matrix from the task

Table 3.6: Summary statistics

	(1)	(2)	(3)
	Workers	Spectators (Experiment 1)	Spectators (Experiment 2)
Male	0.457	0.457	0.491
Age > 45 years old	0.245	0.556	0.520
White	0.803	0.756	0.756
Household size	2.658	2.320	2.391
Income > 45,000 USD	0.496	0.535	0.631
2-year college degree	0.890	0.893	0.882
Full-time employee	0.551	0.497	0.544
Northeast	0.184	0.192	0.190
Midwest	0.215	0.218	0.225
West	0.228	0.249	0.223
Republicans	0.226	0.483	0.501
Observations	1616	4217	5979

Note: The first column shows summary statistics for the workers recruited through MTurk. The second and third columns show summary statistics separately for spectators recruited from Research Now's Political Panel for Experiment 1 and Experiment 2, respectively. We further recruited 2,094 receivers from MTurk to incentivize the equality-efficiency preference elicitation. We do not display summary statistics for the receivers as they did not make any choices in the experiment.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.7: Behavioral responses to taxation: Actual responses vs. own beliefs

	Actual production (z-scored)				Beliefs about production (z-scored)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low Pay	-0.25*** (0.07)	-0.24*** (0.07)	-0.30*** (0.08)	-0.29*** (0.08)	-0.19** (0.07)	-0.18** (0.07)	-0.23*** (0.09)	-0.22*** (0.08)
Government Tax	-0.27*** (0.07)	-0.28*** (0.07)	-0.34*** (0.08)	-0.34*** (0.08)	-0.18** (0.07)	-0.19** (0.07)	-0.25*** (0.09)	-0.25*** (0.09)
Redistributive tax	-0.20*** (0.07)	-0.20*** (0.07)	-0.25*** (0.08)	-0.24*** (0.08)	-0.19** (0.07)	-0.18** (0.07)	-0.25*** (0.08)	-0.24*** (0.08)
Republican		-0.02 (0.06)	-0.16 (0.12)	-0.19 (0.12)		-0.01 (0.06)	-0.14 (0.12)	-0.18 (0.11)
Low Pay × Republicans			0.21 (0.18)	0.21 (0.18)			0.17 (0.18)	0.17 (0.18)
Government Tax × Republicans			0.29* (0.17)	0.28* (0.17)			0.29* (0.17)	0.29* (0.16)
Redistributive tax × Republicans			0.21 (0.17)	0.19 (0.17)			0.26 (0.17)	0.24 (0.17)
Male		-0.20*** (0.06)		-0.20*** (0.06)		-0.32*** (0.05)		-0.32*** (0.05)
Age > 45 years old		0.00 (0.00)		0.00 (0.00)		0.01** (0.00)		0.01** (0.00)
White		0.02 (0.07)		0.02 (0.07)		-0.01 (0.07)		-0.01 (0.07)
Income > 45,000 USD		0.06 (0.06)		0.06 (0.06)		0.03 (0.06)		0.03 (0.06)
2-year college degree		0.13 (0.08)		0.13 (0.08)		0.04 (0.09)		0.03 (0.09)
Full-time employment		-0.16*** (0.06)		-0.16*** (0.06)		-0.14** (0.06)		-0.14** (0.06)
Low Pay + Low Pay × Republicans			-0.09 (0.16)	-0.08 (0.16)			-0.06 (0.16)	-0.05 (0.15)
Government Tax + Government Tax × Republicans			-0.05 (0.15)	-0.08 (0.16)			0.04 (0.14)	-0.05 (0.15)
Redistributive Tax + Redistributive Tax × Republicans			-0.04 (0.15)	-0.05 (0.15)			0.02 (0.14)	0.00 (0.14)
N	1616	1616	1616	1616	1616	1616	1616	1616

Note: This table shows data for the workers on MTurk. In columns 1–4, the dependent variable is actual production (standardized). In columns 5–8, the dependent variable is the workers' beliefs about average production in their treatment (standardized). Controls include household size and regional indicators in addition to the coefficients displayed in the table.

* p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses.

Table 3.8: Policy preferences: Political context

	Equalize incomes		Higher taxes	
	(1)	(2)	(3)	(4)
Motivated Beliefs	-0.04 (0.03)	-0.00 (0.04)	0.00 (0.03)	0.05 (0.05)
Group Identity	-0.03 (0.03)	0.00 (0.04)	0.01 (0.03)	0.08 (0.05)
Republicans	-0.83*** (0.03)	-0.78*** (0.05)	-0.89*** (0.03)	-0.81*** (0.05)
Motivated Beliefs × Republicans		-0.08 (0.07)		-0.10 (0.07)
Group Identity × Republicans		-0.07 (0.07)		-0.13** (0.07)
Male	-0.24*** (0.03)	-0.24*** (0.03)	0.05* (0.03)	0.05* (0.03)
Age > 45 years old	-0.08** (0.03)	-0.08** (0.03)	-0.10*** (0.03)	-0.10*** (0.03)
White	-0.08** (0.03)	-0.08** (0.03)	0.07** (0.04)	0.07** (0.04)
Income > 45,000 USD	-0.22*** (0.03)	-0.22*** (0.03)	-0.04 (0.03)	-0.04 (0.03)
2-year college degree	-0.20*** (0.05)	-0.20*** (0.05)	0.18*** (0.05)	0.18*** (0.05)
Full-time employment	0.05* (0.03)	0.05* (0.03)	-0.03 (0.03)	-0.03 (0.03)
N	4353	4353	4353	4353
R-sq	0.212	0.212	0.205	0.206

Note: *Equalize incomes* refers to support for equalization of incomes in society and *Higher taxes* refers to support for higher income taxes to reduce inequality. Both variables have been z-scored. Controls include race, gender, income, household size, region, employment, and education.

* p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses.

Table 3.9: Policy preferences: Neutral context

	(1)	(2)
Government Tax	-0.00 (0.03)	0.01 (0.05)
Redistributive Tax	-0.01 (0.03)	0.00 (0.05)
Republicans	-0.68*** (0.03)	-0.66*** (0.05)
Government Tax × Republicans		-0.03 (0.07)
Redistributive Tax × Republicans		-0.04 (0.07)
Male	-0.20*** (0.03)	-0.19*** (0.03)
Age > 45 years old	-0.13*** (0.03)	-0.13*** (0.03)
White	-0.17*** (0.04)	-0.17*** (0.04)
Income > 45,000 USD	-0.28*** (0.03)	-0.28*** (0.03)
2-year college degree	-0.21*** (0.05)	-0.21*** (0.05)
Full-time employment	0.03 (0.03)	0.03 (0.03)
N	4128	4128
R-sq	0.173	0.173

Note: Dependent variable: Support for equalization of incomes (z-scored). Controls include race, gender, income, household size, region, employment, and education.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.10: Beliefs and preferences: Robustness

	Overestimate behavioral responses to taxation					Equality-efficiency preferences (amount > 0)						
	(1) Exp. 1	(2) Exp. 1	(3) Exp. 2	(4) Exp. 2	(5) Pooled	(6) Pooled	(7) Exp. 1	(8) Exp. 1	(9) Exp. 2	(10) Exp. 2	(11) Pooled	(12) Pooled
Republican	0.03 (0.03)	0.03 (0.03)	0.03 (0.02)	0.03 (0.03)	0.03* (0.02)	0.03* (0.02)	-0.14*** (0.03)	-0.13*** (0.03)	-0.17*** (0.03)	-0.16*** (0.03)	-0.16*** (0.02)	-0.14*** (0.02)
Male	0.00 (0.03)	0.00 (0.03)	-0.03 (0.03)	-0.03 (0.03)	-0.01 (0.02)	-0.01 (0.02)	-0.01 (0.03)	-0.01 (0.03)	-0.02 (0.03)	-0.02 (0.03)	-0.02 (0.03)	-0.02 (0.02)
Age > 45 years old	-0.02 (0.03)	-0.02 (0.03)	0.03 (0.03)	0.03 (0.03)	0.01 (0.02)	0.01 (0.02)	0.03 (0.03)	0.03 (0.03)	-0.03 (0.03)	-0.03 (0.03)	-0.00 (0.02)	-0.00 (0.02)
White	-0.04 (0.03)	-0.04 (0.03)	0.00 (0.03)	0.00 (0.03)	-0.02 (0.02)	-0.02 (0.02)	-0.12*** (0.03)	-0.12*** (0.03)	-0.09*** (0.03)	-0.09*** (0.03)	-0.11*** (0.02)	-0.11*** (0.02)
Income > 45,000 USD	-0.01 (0.03)	-0.01 (0.03)	-0.01 (0.03)	-0.01 (0.03)	-0.01 (0.02)	-0.01 (0.02)	-0.08*** (0.03)	-0.08*** (0.03)	-0.06** (0.03)	-0.06** (0.03)	-0.06*** (0.02)	-0.06*** (0.02)
2-year college degree	-0.06 (0.04)	-0.06 (0.04)	-0.05 (0.04)	-0.05 (0.04)	-0.05* (0.03)	-0.05* (0.03)	-0.06 (0.04)	-0.06 (0.04)	-0.09** (0.04)	-0.09** (0.04)	-0.07** (0.03)	-0.07** (0.03)
Full-time employment	-0.01 (0.03)	-0.01 (0.03)	0.03 (0.03)	0.03 (0.03)	0.02 (0.02)	0.02 (0.02)	0.04 (0.03)	0.04 (0.03)	0.03 (0.03)	0.03 (0.03)	0.03* (0.02)	0.03* (0.02)
N	1372	1372	1524	1524	2896	2896	1372	1372	1524	1524	2896	2896
R-sq	0.001	0.007	0.001	0.006	0.001	0.004	0.020	0.038	0.030	0.050	0.025	0.041

Note: Both beliefs and preferences are indicator variables for, respectively, overestimating behavioral responses to taxation and redistributing a positive amount in the equality-efficiency task. The controls were pre-specified and include, in addition to the coefficients displayed in the table, an indicator for household size and regional indicators. We only include respondents from the Government Tax treatments. We include experiment fixed effects in regressions on the pooled sample.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.11: Beliefs and preferences: Reweighted data

	Beliefs about behavioral responses to taxation						Equality-efficiency preferences (amount redistributed)					
	(1) Exp. 1	(2) Exp. 1	(3) Exp. 2	(4) Exp. 2	(5) Pooled	(6) Pooled	(7) Exp. 1	(8) Exp. 1	(9) Exp. 2	(10) Exp. 2	(11) Pooled	(12) Pooled
Republican	0.06 (0.08)	0.03 (0.07)	-0.07 (0.08)	-0.06 (0.07)	-0.01 (0.05)	-0.02 (0.05)	-0.39*** (0.07)	-0.35*** (0.07)	-0.24*** (0.07)	-0.22*** (0.06)	-0.31*** (0.05)	-0.28*** (0.05)
Male		-0.03 (0.07)		0.10 (0.08)		0.03 (0.05)		-0.12* (0.06)		-0.05 (0.07)		-0.08 (0.05)
Age > 45 years old		0.05 (0.08)		-0.22*** (0.08)		-0.10* (0.06)		0.04 (0.07)		-0.02 (0.08)		0.02 (0.05)
White		0.23*** (0.08)		0.06 (0.09)		0.13** (0.06)		-0.28*** (0.08)		-0.16* (0.09)		-0.22*** (0.06)
Income > 45,000 USD		0.02 (0.08)		0.02 (0.08)		0.03 (0.06)		-0.07 (0.07)		-0.13* (0.08)		-0.10* (0.05)
2-year college degree		0.29*** (0.10)		0.19** (0.09)		0.23*** (0.07)		-0.10 (0.09)		-0.17* (0.09)		-0.14** (0.06)
Full-time employment		0.07 (0.08)		-0.09 (0.07)		-0.02 (0.05)		0.00 (0.07)		0.09 (0.08)		0.05 (0.05)
N	1372	1372	1524	1524	2896	2896	1372	1372	1524	1524	2896	2896
R-sq	0.001	0.028	0.001	0.027	0.000	0.020	0.037	0.064	0.015	0.038	0.024	0.043

Note: Both beliefs and preferences have been z-scored. The controls were pre-specified and include, in addition to the coefficients displayed in the table, an indicator for household size and regional indicators. We only include respondents from the Government Tax treatments. We include experiment fixed effects in regressions on the pooled sample. The data has been reweighted to match the demographic profile of Republicans and Democrats separately with respect to six cells based on age (above/below 65 years old), race (white/nonwhite), and education (some college/not some college). We used data from Pew Research to create the weights, <http://www.people-press.org/2016/09/13/1-the-changing-composition-of-the-political-parties/> (accessed July 6, 2018).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.12: Views on redistribution: Robustness

	Should aim to equalize incomes in society				Should increase taxes to reduce inequality				
	(1) Low Pay	(2) Government Tax (Exp. 1)	(3) Redistributive Tax	(4) Government Tax (Exp. 2)	(5) Motivated Beliefs	(6) Group Identity	(7) Government Tax (Exp. 2)	(8) Motivated Beliefs	(9) Group Identity
Panel A: Full sample									
Beliefs	0.02 (0.03)	0.01 (0.02)	-0.04* (0.02)	-0.00 (0.03)	-0.03 (0.03)	0.02 (0.02)	0.06*** (0.02)	0.03 (0.02)	0.03 (0.02)
Preferences	0.19*** (0.03)	0.17*** (0.02)	0.17*** (0.03)	0.18*** (0.02)	0.20*** (0.02)	0.17*** (0.02)	0.10*** (0.02)	0.12*** (0.03)	0.09*** (0.02)
N	1396	1372	1360	1524	1380	1449	1524	1380	1449
Panel B: Democrats									
Beliefs	0.05 (0.04)	0.04 (0.03)	-0.05 (0.03)	0.01 (0.03)	-0.07** (0.03)	0.03 (0.03)	0.13*** (0.04)	0.06* (0.04)	0.10*** (0.04)
Preferences	0.14*** (0.03)	0.15*** (0.03)	0.13*** (0.03)	0.12*** (0.03)	0.15*** (0.03)	0.14*** (0.03)	0.07* (0.04)	0.10*** (0.04)	0.11*** (0.03)
N	724	701	703	746	716	728	746	716	728
Panel C: Republicans									
Beliefs	-0.02 (0.04)	-0.03 (0.04)	-0.03 (0.03)	-0.02 (0.04)	0.03 (0.04)	0.02 (0.04)	-0.02 (0.03)	-0.02 (0.03)	-0.03 (0.03)
Preferences	0.25*** (0.04)	0.20*** (0.04)	0.20*** (0.04)	0.22*** (0.03)	0.27*** (0.04)	0.20*** (0.04)	0.14*** (0.03)	0.15*** (0.04)	0.07** (0.03)
N	672	671	657	778	664	721	778	664	721

Note: All variables have been z-scored. Controls include race, gender, income, household size, region, employment, and education.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

3.B Main tables with full sample

In the main analysis, we focused on the spectators who spent at least 15 seconds on the belief elicitation page. This section replicates the main tables with the full sample.

Table 3.13: Beliefs about behavioral responses to taxation

	Change in production (z-scored)				Overestimate behavioral responses			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Government Tax	-0.12** (0.04)	-0.11*** (0.04)	-0.14*** (0.05)	-0.13** (0.05)	0.05*** (0.02)	0.05** (0.02)	0.06** (0.03)	0.05** (0.02)
Redistributive Tax	0.05 (0.04)	0.06 (0.04)	0.02 (0.05)	0.02 (0.05)	-0.04** (0.02)	-0.05** (0.02)	-0.03 (0.03)	-0.03 (0.03)
Republican		-0.00 (0.03)	-0.02 (0.05)	-0.04 (0.05)	0.03** (0.01)	0.04 (0.03)	0.04 (0.03)	0.04* (0.03)
Government Tax × Republicans			0.04 (0.07)	0.04 (0.07)			-0.02 (0.04)	-0.02 (0.04)
Redistributive Tax × Republicans			0.07 (0.07)	0.06 (0.07)			-0.03 (0.04)	-0.02 (0.04)
Male		-0.01 (0.03)		-0.01 (0.03)		-0.01 (0.01)		-0.01 (0.01)
Age > 45 years old		-0.01 (0.03)		-0.01 (0.03)		-0.01 (0.02)		-0.01 (0.02)
White		0.12*** (0.04)		0.12*** (0.04)		-0.03* (0.02)		-0.03* (0.02)
Income > 45,000 USD		0.07** (0.03)		0.07** (0.03)		-0.02 (0.02)		-0.02 (0.02)
2-year college degree		0.34*** (0.06)		0.34*** (0.06)		-0.09*** (0.02)		-0.09*** (0.02)
Full-time employment		0.02 (0.03)		0.02 (0.03)		0.00 (0.02)		0.00 (0.02)
Government Tax + Government Tax × Republicans			-0.10** (0.05)	-0.09* (0.05)		0.04 (0.03)		0.04 (0.03)
Redistributive Tax + Redistributive Tax × Republicans			0.09* (0.05)	0.09* (0.05)		-0.06** (0.03)		-0.06** (0.03)
N	4217	4217	4217	4217	4217	4217	4217	4217

Note: In columns 1–4, the dependent variable is beliefs about change in production (standardized). In columns 5–8, the dependent variable is an indicator for overestimating the change in production. Controls include household size and regional indicators in addition to the coefficients displayed in the table.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.14: Equality–efficiency preferences

	Amount redistributed			Amount redistributed > 0		
	(1)	(2)	(3)	(4)	(5)	(6)
Republican	-0.22*** (0.03)	-0.22*** (0.03)	-0.21*** (0.03)	-0.11*** (0.02)	-0.11*** (0.02)	-0.10*** (0.02)
Male			-0.07** (0.03)			-0.02 (0.02)
Age > 45 years old			-0.02 (0.03)			0.00 (0.02)
White			-0.14*** (0.04)			-0.10*** (0.02)
Income > 45,000 USD			-0.11*** (0.03)			-0.07*** (0.02)
2-year college degree			-0.11** (0.05)			-0.07*** (0.02)
Full-time employment			-0.03 (0.03)			0.01 (0.02)
N	4217	4217	4217	4217	4217	4217
R-sq	0.012	0.013	0.024	0.012	0.013	0.027
Treatment indicators	No	Yes	Yes	No	Yes	Yes

Note: The dependent in columns 1–3 (amount redistributed between workers) has been z-scored. Controls include, in addition to the coefficients displayed in the table, household size and regional indicators. The treatment indicators are jointly insignificant in a regression on the amount redistributed ($p=0.31$).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.15: Beliefs and preferences: Replication

	Beliefs about behavioral responses to taxation					Equality-efficiency preferences (amount redistributed)						
	(1) Exp. 1	(2) Exp. 1	(3) Exp. 2	(4) Exp. 2	(5) Pooled	(6) Pooled	(7) Exp. 1	(8) Exp. 1	(9) Exp. 2	(10) Exp. 2	(11) Pooled	(12) Pooled
Republican	0.01 (0.05)	-0.01 (0.05)	-0.09* (0.05)	-0.12** (0.05)	-0.04 (0.04)	-0.07* (0.04)	-0.31*** (0.05)	-0.28*** (0.05)	-0.25*** (0.05)	-0.22*** (0.05)	-0.28*** (0.04)	-0.25*** (0.04)
Male		-0.06 (0.05)		-0.07 (0.05)		-0.07* (0.04)		-0.06 (0.05)		0.00 (0.05)		-0.02 (0.04)
Age > 45 years old		0.05 (0.06)		-0.07 (0.06)		-0.01 (0.04)		0.03 (0.06)		-0.07 (0.05)		-0.03 (0.04)
White		0.15** (0.07)		0.24*** (0.07)		0.19*** (0.05)		-0.23*** (0.06)		-0.24*** (0.06)		-0.24*** (0.05)
Income > 45,000 USD		0.05 (0.06)		0.01 (0.06)		0.03 (0.04)		-0.15*** (0.06)		-0.10* (0.05)		-0.12*** (0.04)
2-year college degree		0.31*** (0.09)		0.25*** (0.09)		0.28*** (0.06)		-0.07 (0.08)		-0.05 (0.08)		-0.07 (0.06)
Full-time employment		-0.01 (0.06)		-0.09* (0.05)		-0.05 (0.04)		0.04 (0.06)		0.04 (0.05)		0.04 (0.04)
N	1396	1396	1606	1606	3002	3002	1396	1396	1606	1606	3002	3002
R-sq	0.000	0.018	0.002	0.024	0.001	0.019	0.023	0.039	0.016	0.040	0.019	0.036

Note: Both beliefs and preferences have been z-scored. The controls were pre-specified and include, in addition to the coefficients displayed in the table, an indicator for household size and regional indicators. We only include respondents from the Government Tax treatments. We include experiment fixed effects in regressions on the pooled sample.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.16: Beliefs and preferences: Political context

	Beliefs		Preferences	
	(1)	(2)	(3)	(4)
Motivated Beliefs	-0.12*** (0.03)	-0.13*** (0.05)	0.02 (0.04)	0.08 (0.05)
Group Identity	-0.01 (0.04)	-0.03 (0.05)	0.04 (0.04)	0.11** (0.05)
Republicans	-0.10*** (0.03)	-0.11** (0.05)	-0.31*** (0.03)	-0.23*** (0.05)
Motivated Beliefs × Republicans		0.03 (0.07)		-0.13* (0.07)
Group Identity × Republicans		0.02 (0.07)		-0.14** (0.07)
Male	-0.08*** (0.03)	-0.08*** (0.03)	-0.01 (0.03)	-0.01 (0.03)
Age > 45 years old	0.03 (0.03)	0.03 (0.03)	-0.07** (0.03)	-0.07** (0.03)
White	0.25*** (0.04)	0.25*** (0.04)	-0.15*** (0.04)	-0.16*** (0.04)
Income > 45,000 USD	0.02 (0.03)	0.02 (0.03)	-0.06* (0.03)	-0.06* (0.03)
2-year college degree	0.32*** (0.05)	0.32*** (0.05)	-0.00 (0.05)	-0.01 (0.05)
Full-time employment	-0.08** (0.03)	-0.08** (0.03)	-0.05 (0.03)	-0.05 (0.03)
N	4642	4642	4642	4642
R-sq	0.032	0.032	0.036	0.037

Note: *Beliefs* refers to beliefs about behavioral responses to taxation (points produced in the Government Tax treatment) and *Preferences* refers to amount redistributed. Both variables have been standardized. Controls include race, gender, income, household size, region, employment, and education.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.17: Views on redistribution – beliefs and preferences

	Should aim to equalize incomes in society			Should increase taxes to reduce inequality		
	(1) Pooled sample	(2) Democrats	(3) Republicans	(4) Pooled sample	(5) Democrats	(6) Republicans
Panel A						
Beliefs (points produced)	-0.02** (0.01)	0.01 (0.01)	-0.05*** (0.02)	-0.01 (0.01)	0.09*** (0.02)	-0.09*** (0.02)
Preferences (amount)	0.18*** (0.01)	0.13*** (0.01)	0.23*** (0.01)	0.12*** (0.01)	0.09*** (0.02)	0.14*** (0.02)
Panel B						
Beliefs (overestimate)	-0.02 (0.02)	-0.03 (0.03)	0.00 (0.03)	-0.07** (0.03)	-0.20*** (0.04)	0.10*** (0.04)
Preferences (amount > 0)	0.37*** (0.02)	0.26*** (0.03)	0.47*** (0.03)	0.21*** (0.03)	0.15*** (0.04)	0.26*** (0.04)
N	8859	4480	4379	4642	2300	2342

Note: The dependent variable in columns 1–3 is support for equalization of incomes in society. The dependent variable in columns 4–6 is support for higher income taxes to reduce income inequality. In **Panel A**, *Beliefs* refers to beliefs about behavioral responses to government taxation and *Preferences* refers to the amount redistributed in the spectator decision. In **Panel B**, *Beliefs* is an indicator for overestimating behavioral responses to government taxation and *Preferences* is an indicator for redistributing a positive amount in the spectator decision. We include respondents from the Government Tax treatment. All variables, except for the indicators in Panel B, have been z-scored. Controls include race, gender, income, household size, region, employment, and education.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

3.C Pre-specified tables

We uploaded the first pre-analysis plan to the AEA RCT Registry on May 2, 2017. We uploaded an updated version of this pre-analysis plan on May 29, 2017 (on the same day, but before we started collecting data for the project). The only substantial difference between the two pre-analysis plans is that we added a specification in Section 4.1.4 on whether our elicited beliefs about behavioral responses to taxation could explain differences in people's support for redistribution of income in society. The reader should consult the updated pre-analysis plan when evaluating the pre-specified tables in Section 3.C. We uploaded a pre-analysis plan for the second experiment on February 6, 2018 and started to collect data for this project on February 7, 2018.

Below we list some minor deviations from the pre-analysis plans.

- We did not pre-specify the investigation of treatment effects of the political primes on equality–efficiency preferences and policy views; i.e., columns 2–4 of Table 3.4 were not pre-specified.
- We pre-specified collecting 4500 and 6000 spectators for the first and second experiments, respectively. We actually recruited 4218 and 5979 spectators, respectively. The reason for the small discrepancy was that the market research company had difficulties recruiting enough respondents. We also pre-specified to collect data for 1600 workers, but ended up recruiting 16 workers more (i.e., 1616 in total) because of a small glitch.

In the remainder of this section, we list all pre-specified tables.

Table 3.18: Dependent variable: Beliefs about absolute change in production

	(1)	(2)	(3)
Government Tax	155.81*** (47.13)	144.61*** (46.81)	169.20** (66.42)
Redistributive Tax	-65.71 (47.89)	-71.35 (47.58)	-31.84 (67.04)
Republicans		5.05 (40.13)	49.25 (60.92)
Government Tax × Republicans			-51.06 (93.66)
Redistributive Tax × Republicans			-82.13 (95.10)
Constant	886.76*** (30.61)	1463.41*** (107.41)	1441.64*** (110.60)
N	4217	4217	4217
R-sq	0.005	0.021	0.021
Controls	No	Yes	Yes

Note: Columns 1 and 2 show the specification from Section 4.1.1. of PAP 1 (pages 6–7).

Column 3 shows the specification from Section 4.1.2. the PAP (page 7).

* p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses.

Table 3.19: Structural estimates of beliefs about the tax dollar value

	(1)
$\gamma_{T2}^{Democrat}$	-0.21** (0.08)
$\gamma_{T2}^{Republican}$	-0.14* (0.08)
$\gamma_{T3}^{Democrat}$	0.02 (0.08)
$\gamma_{T3}^{Republican}$	0.13* (0.07)
N	4217
P-value of test $\gamma_{T2}^{Democrat} - \gamma_{T2}^{Republican} = 0$	0.59
P-value of test $\gamma_{T3}^{Democrat} - \gamma_{T3}^{Republican} = 0$	0.32

Note: The table shows the specification from Section 4.1.3. of PAP 1 (pages 8–9).

* p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses.

Table 3.20: Demand for redistribution and beliefs about behavioral responses

	(1)	(2)
diff	0.0000 (0.0001)	0.0000 (0.0001)
diff × Government Tax	-0.0000 (0.0001)	-0.0000 (0.0001)
diff × Redistributive Tax	0.0001 (0.0001)	0.0001 (0.0001)
N	4217	4217
R-sq	0.001	0.172
Controls	No	Yes
P-value joint significance	0.221	0.616

Note: The table shows the specification from Section 4.1.4 of PAP 1 (pages 9–10).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.21: Political differences

	(1)
equal_inc1_mean Republicans	-1.91*** (0.08)
equal_inc2_mean Republicans	-1.91*** (0.08)
N	4217
P-value equality of Republican indicator	0.421

Note: The table shows the second specification (“Political differences”) from Section 4.1.4 of PAP 1 (pages 9–10).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.22: Exploratory analysis of heterogeneity in beliefs

	(1)	(2)	(3)	(4)	(5)	(6)
	Gender	Age	College	White	Income	Work
Interactant	2.6 (61.0)	85.5 (64.2)	-451.1*** (121.4)	-185.4** (77.2)	-111.3* (62.9)	-15.2 (63.5)
Government Tax × Interactant	77.5 (93.2)	-144.3 (93.6)	56.9 (169.2)	-22.8 (113.2)	38.7 (94.1)	47.6 (93.4)
Redistributive Tax × Interactant	-36.2 (95.3)	-78.4 (95.0)	-29.6 (183.7)	101.6 (116.1)	36.7 (95.7)	-68.1 (95.3)
N	4217	4217	4217	4217	4217	4217

Note: The table shows the specification from Section 4.1.5 of PAP 1 (pages 10–11).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.23: Do taxes affect worker effort?

	(1)	(2)	(3)
Low Pay	-293.30*** (85.66)	-291.24*** (85.25)	-261.92** (125.59)
Government Tax	-317.59*** (85.01)	-330.90*** (83.78)	-352.63*** (131.57)
Redistributive tax	-234.00*** (83.09)	-243.13*** (82.78)	-253.35** (126.46)
Republicans		94.63 (80.45)	-57.51 (148.35)
Low Pay × Republicans			156.05 (222.60)
Government Tax × Republicans			275.98 (213.52)
Redistributive tax × Republicans			188.73 (212.11)
Low Pay × Independents			-205.28 (192.77)
Government Tax × Independents			-131.58 (192.96)
Redistributive tax × Independents			-107.93 (188.22)
Constant	3031.91*** (57.04)	2728.16*** (171.91)	2737.68*** (183.27)
N	1616	1616	1616
R-sq	0.010	0.042	0.044
Controls	No	Yes	Yes
P-value joint signifiacne	0.0004	0.0002	0.04
P-value coefficients equal	0.000414	0.000231	0.0354
P-value Republican interactions			0.622
P-value standard model (Democrats)			0.730
P-value standard model (Republicans)			0.974

Note: The table shows the specification from Section 4.2.1. and 4.2.2. of PAP 1 (pages 12–13).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.24: Worker beliefs

	(1)	(2)
Low Pay	-203.38** (79.33)	-198.59** (78.48)
Government Tax	-195.99** (79.29)	-205.47*** (78.17)
Redistributive tax	-197.73** (77.50)	-203.70*** (76.57)
Constant	2825.84*** (52.40)	2619.23*** (165.29)
N	1616	1616
R-sq	0.006	0.051
Controls	No	Yes
P-value joint signifiacnce	0.017	0.013
P-value coefficients equal	1.00	1.00

Note: The table shows the specification from Section 4.2.3. of PAP 1 (page 13).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.25: Do the treatments polarize beliefs?

	(1)	(2)	(3)
Motivated Beliefs	164.10*** (49.99)	168.89*** (49.39)	188.86*** (71.16)
Group Identity	12.14 (50.67)	19.83 (49.93)	35.97 (71.18)
Republicans		138.26*** (41.27)	161.40** (69.37)
Motivated Beliefs \times Republicans			-39.35 (99.00)
Group Identity \times Republicans			-31.36 (100.22)
Constant	1099.30*** (34.68)	1712.94*** (106.45)	1701.87*** (110.28)
N	4642	4642	4642
R-sq	0.003	0.032	0.032
Controls	No	Yes	Yes

Note: The table shows the specification from Section 4.1.1. of PAP 1 (pages 3–4).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.26: Views on redistribution

	(1) Equalize income	(2) Higher taxes
Beliefs	0.00 (0.00)	-0.00 (0.00)
Amount redistributed	0.61*** (0.08)	0.39*** (0.08)
Motivated Beliefs	-0.27* (0.15)	-0.14 (0.16)
Group Identity	-0.05 (0.15)	-0.03 (0.16)
Motivated Beliefs × Amount redistributed	0.08 (0.11)	0.07 (0.11)
Group Identity × Amount redistributed	0.00 (0.11)	0.02 (0.11)
Motivated Beliefs × Beliefs	0.00 (0.00)	0.00 (0.00)
Group Identity × Beliefs	-0.00 (0.00)	0.00 (0.00)
Republicans	-1.97*** (0.08)	-2.26*** (0.09)
Constant	7.71*** (0.22)	6.16*** (0.24)
N	4642	4642
R-sq	0.222	0.189
Controls	Yes	Yes

Note: The table shows the specification from Section 4.1.2. of PAP 2 (pages 4–5).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3.27: Second-order beliefs

	(1) Difference	(2) Second-order beliefs about Republicans
Second-order beliefs about Democrats		0.73*** (0.03)
Constant	-93.80*** (28.44)	315.46*** (42.87)
N	1337	1337
R-sq	0.000	0.589

Note: The table shows the specification from Section 4.2.1. of PAP 2 (page 5).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

References

- Abrams, David S, Marianne Bertrand, and Sendhil Mullainathan**, “Do Judges Vary in Their Treatment of Race?,” *Journal of Legal Studies*, 2012, 41 (2), 347–383.
- Alan, Sule and Seda Ertac**, “Belief in Hard Work and Prosocial Behavior: Evidence from a Randomized Field Experiment,” 2017.
- Alesina, Alberto and Eliana La Ferrara**, “Preferences for Redistribution in the Land of Opportunities,” *Journal of Public Economics*, 2005, 89 (5), 897–931.
- **and —**, “A Test of Racial Bias in Capital Sentencing,” *American Economic Review*, 2014, 104 (11), 3397–3433.
- **and George-Marios Angeletos**, “Fairness and Redistribution,” *American Economic Review*, 2005, 95 (4), 960–980.
- **and Paola Giuliano**, “Preferences for Redistribution,” in Jess Benhabib, Alberto Bisin, and Matthew O Jackson, eds., *Handbook of Social Economics*, Vol. 1, Amsterdam, NL: Elsevier, 2011, chapter 4, pp. 99–131.
- **, Armando Miano, and Stefanie Stantcheva**, “Immigration and Redistribution,” NBER Working Paper 24733 2018.
- **, Stefanie Stantcheva, and Edoardo Teso**, “Intergenerational Mobility and Preferences for Redistribution,” *American Economic Review*, 2018, 108 (2), 521–554.

- Almås, Ingvild, Alexander W. Cappelen, and Bertil Tungodden**, “Cut-throat capitalism versus cuddly socialism: Are Americans more meritocratic and efficiency-seeking than Scandinavians?,” mimeo 2016.
- Arrow, Kenneth J**, “What Has Economics to Say about Racial Discrimination?,” *Journal of Economic Perspectives*, 1998, 12 (2), 91–100.
- Athey, Susan and Guido Imbens**, “Recursive Partitioning for Heterogeneous Causal Effects,” *Proceedings of the National Academy of Sciences*, 2016, 113 (27), 7353–7360.
- Bartoš, Vojtěch, Michal Bauer, Julie Chytilová, and Filip Matějka**, “Attention Discrimination: Theory and Field Experiments with Monitoring Information Acquisition,” *American Economic Review*, 2016, 106 (6), 1437–1475.
- Bénabou, Roland**, “The Economics of Motivated Beliefs,” *Revue d’économie politique*, 2015, 125 (5), 665–685.
- **and Efe A Ok**, “Social mobility and the demand for redistribution: The POUM hypothesis.,” *Quarterly Journal of Economics*, 2001, 116 (2).
- **and Jean Tirole**, “Belief in a Just World and Redistributive Politics,” *Quarterly Journal of Economics*, 2006, 12 (2), 699–746.
- Bertrand, Marianne and Esther Duflo**, “Field Experiments on Discrimination,” in Esther Duflo and Abhijit Banerjee, eds., *Handbook of Field Experiments*, Vol. 1, Elsevier, 2017, chapter 8.
- **and Sendhil Mullainathan**, “Are Emily and Greg More Employable than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination,” *American Economic Review*, 2004, 94 (4), 991–1013.

- Bobo, Lawrence and James R Kluegel**, "Opposition to Race-Targeting: Self-Interest, Stratification Ideology, or Racial Attitudes?," *American Sociological Review*, 1993, pp. 443–464.
- Bolton, Gary E. and Axel Ockenfels**, "ERC: A Theory of Equity, Reciprocity, and Competition," *American Economic Review*, 2000, 90 (1), 166–193.
- Bond, Timothy N and Kevin Lang**, "The Sad Truth About Happiness Scales," *Journal of Political Economy* (forthcoming), 2018.
- Bordalo, Pedro, Katherine Coffman, Nicola Gennaioli, and Andrei Shleifer**, "Stereotypes," *Quarterly Journal of Economics*, 2016, 131 (4), 1753–1794.
- Borjas, George J**, "The Wage Impact of the Marielitos: A Reappraisal," *ILR Review*, 2017, 70, 1077–1110.
- Bray, Margaret and David M Kreps**, "Rational Learning and Rational Expectations," in "Arrow and the ascent of modern economic theory," Springer, 1987, pp. 597–625.
- Bullock, John G, Alan S Gerber, Seth J Hill, and Gregory A Huber**, "Partisan bias in factual beliefs about politics," *Quarterly Journal of Political Science*, 2015, 10, 519–578.
- Bursztyn, Leonardo, Michael Callen, Bruno Ferman, Saad Gulzar, Ali Hasanain, and Noam Yuchtman**, "Political Identity: Experimental Evidence on Anti-americanism in Pakistan," 2016.
- Cappelen, Alexander W., Ingar K. Haaland, and Bertil Tungodden**, "Beliefs about Behavioral Responses to Taxation," 2019.

- , **James Konow, Erik Ø. Sørensen, and Bertil Tungodden**, “Just Luck: An Experimental Study of Risk Taking and Fairness,” *American Economic Review*, 2013, 103 (3), 1398–1413.
- Card, David**, “The Impact of the Mariel Boatlift on the Miami Labor Market,” *Industrial and Labor Relations Review*, 1990, 43 (2), 245–257.
- , **Christian Dustmann, and Ian Preston**, “Immigration, Wages, and Compositional Amenities,” *Journal of the European Economic Association*, 2012, 10 (1), 78–119.
- Cavallo, Alberto, Guillermo Cruces, and Ricardo Perez-Truglia**, “Inflation Expectations, Learning and Supermarket Prices: Evidence from Field Experiments,” *American Economic Journal: Macroeconomics*, 2016, 9 (3), 1–35.
- Charness, Gary and Matthew Rabin**, “Understanding Social Preferences with Simple Tests,” *Quarterly Journal of Economics*, 2002, 117 (3), 817–869.
- Christensen, Garret and Edward Miguel**, “Transparency, Reproducibility, and the Credibility of Economics Research,” *Journal of Economic Literature*, 2018, 56 (3), 920–80.
- Citrin, Jack, Donald P Green, Christopher Muste, and Cara Wong**, “Public Opinion Toward Immigration Reform: The Role of Economic Motivations,” *Journal of Politics*, 1997, 59 (3), 858–881.
- Clemens, Michael A.**, “What the Mariel Boatlift of Cuban Refugees Can Teach Us about the Economics of Immigration: An Explainer and a Revelation,” 2017.
- **and Jennifer Hunt**, “The Labor Market Effects of Refugee Waves: Reconciling Conflicting Results,” Working Paper 23433, National Bureau of Economic Research May 2017.

- Cruces, Guillermo, Ricardo Perez-Truglia, and Martin Tetaz**, “Biased perceptions of income distribution and preferences for redistribution: Evidence from a survey experiment,” *Journal of Public Economics*, 2013, 98, 100–112.
- de Quidt, Jonathan, Johannes Haushofer, and Christopher Roth**, “Measuring and Bounding Experimenters Demand,” *American Economic Review*, 2018, 108 (11), 3266–3302.
- Dehdari, Sirus**, “Economic Distress and Support for Far-right Parties - Evidence from Sweden,” available at SSRN: <https://ssrn.com/abstract=3160480> 2018.
- DellaVigna, Stefano**, “Psychology and Economics: Evidence from the Field,” *Journal of Economic Literature*, 2009, 47 (2), 315–372.
- , “The Obama Effect on Economic Outcomes: Evidence from Event Studies,” Technical Report, Working Paper 2010.
- **and Devin Pope**, “What Motivates Effort? Evidence and Expert Forecasts,” *Review of Economic Studies*, 2018, 85 (2), 1029–1069.
- , **John A List, Ulrike Malmendier, and Gautam Rao**, “Voting to tell others,” *Review of Economic Studies*, 2016, 84 (1), 143–181.
- Di Tella, Rafael, Juan Dubra, and Alejandro Luis Lagomarsino**, “Meet the Oligarchs: Business Legitimacy, State Capacity and Taxation,” NBER Working Paper 22934 2017.
- Diamond, Peter and Emmanuel Saez**, “The Case for a Progressive Tax: From Basic Research to Policy Recommendations,” *Journal of Economic Perspectives*, 2011, 25 (4), 165–190.

- Doherty, Carroll**, "7 things to know about polarization in America," 2014. <http://www.pewresearch.org/fact-tank/2014/06/12/7-things-to-know-about-polarization-in-america/> (accessed 03-01-2019).
- Downs, Anthony**, "An Economic Theory of Political Action in a Democracy," *Journal of Political Economy*, 1957, 65 (2), 135–150.
- Edelman, Benjamin G, Michael Luca, and Dan Svirsky**, "Racial Discrimination in the Sharing Economy: Evidence from a Field Experiment," *American Economic Journal: Applied Economics*, 2017, 2, 1–22.
- Facchini, Giovanni, Anna Maria Mayda, and Riccardo Puglisi**, "Illegal immigration and media exposure: Evidence on individual attitudes," *CEPR Discussion Papers 7593*, 2009.
- , **Yotam Margalit, and Hiroyuki Nakata**, "Countering Public Opposition to Immigration: The Impact of Information Campaigns," 2017.
- Falk, Armin and Florian Zimmermann**, "A taste for consistency and survey response behavior," *CESifo Economic Studies*, 2013, 59 (1), 181–193.
- Fehr, Ernst and Klaus M. Schmidt**, "A Theory of Fairness, Competition and Cooperation," *Quarterly Journal of Economics*, 1999, 114 (3), 817–868.
- Feldstein, Martin**, "The Effect of Taxes on Efficiency and Growth," NBER Working Paper 12201 2006.
- Fisman, Raymond, Pamela Jakiela, and Shachar Kariv**, "Distributional Preferences and Political Behavior," Working Paper 2015.

Flynn, D.J., Brendan Nyhan, and Jason Reifler, “The Nature and Origins of Misperceptions: Understanding False and Unsupported Beliefs About Politics,” *Political Psychology*, 2017, 38, 127–150.

Fryer, Roland G, Devah Pager, and Jörg L Spenkuch, “Racial Disparities in Job Finding and Offered Wages,” *Journal of Law and Economics*, 2013, 56 (3), 633–689.

– , **Philipp Harms, and Matthew O Jackson**, “Updating Beliefs when Evidence is Open to Interpretation: Implications for Bias and Polarization,” Available at SSRN: https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2263504 2018.

Gerber, Alan S, Dean Karlan, and Daniel Bergan, “Does the Media Matter? A Field Experiment Measuring the Effect of Newspapers on Voting Behavior and Political Opinions,” *American Economic Journal: Applied Economics*, 2009, pp. 35–52.

– , **Gregory A Huber, Daniel R Biggers, and David J Hendry**, “Self-Interest, Beliefs, and Policy Opinions: Understanding How Economic Beliefs Affect Immigration Policy Preferences,” *Political Research Quarterly*, 2017.

Gilens, Martin, “Political Ignorance and Collective Policy Preferences,” *American Political Science Review*, 2001, 95 (2), 379–396.

– , *Why Americans Hate Welfare: Race, Media, and the Politics of Antipoverty Policy*, University of Chicago Press, 2009.

Grigorieff, Alexis, Christopher Roth, and Diego Ubfal, “Does Information Change Attitudes Towards Immigrants? Representative Evidence From Survey Experiments,” available at SSRN: <https://ssrn.com/abstract=2768187> 2018.

- Haaland, Ingar K. and Christopher Roth**, "Labor Market Concerns and Support for Immigration," 2019.
- Hainmueller, Jens and Daniel J Hopkins**, "The Hidden American Immigration Consensus: A Conjoint Analysis of Attitudes Toward Immigrants," *American Journal of Political Science*, 2014, 59 (3), 529–548.
- and —, "Public Attitudes Toward Immigration," *Annual Review of Political Science*, 2014, 17, 225–249.
- , **Michael J Hiscox, and Yotam Margalit**, "Do Concerns about Labor Market Competition Shape Attitudes Toward Immigration? New Evidence," *Journal of International Economics*, 2015, 97 (1), 193–207.
- Halla, Martin, Alexander F Wagner, and Josef Zweimüller**, "Immigration and Voting for the Far Right," *Journal of the European Economic Association*, 2017, 15 (6), 1341–1385.
- Harrison, David A, David A Kravitz, David M Mayer, Lisa M Leslie, and Dalit Lev-Arey**, "Understanding attitudes toward affirmative action programs in employment: Summary and meta-analysis of 35 years of research," *Journal of Applied Psychology*, 2006, 91 (5), 1013–36.
- Hopkins, Daniel J, John Sides, and Jack Citrin**, "The Muted Consequences of Correct Information About Immigration," *Journal of Politics* (forthcoming), 2018.
- Horton, John J, David G Rand, and Richard J Zeckhauser**, "The Online Laboratory: Conducting Experiments in a Real Labor Market," *Experimental Economics*, 2011, 14 (3), 399–425.
- Iyengar, Shanto, Simon Jackman, Solomon Messing, Nicholas Valentino, Toril Aalberg, Raymond Duch, Kyu S Hahn, Stuart Soroka, Allison**

- Harell, and Tetsuro Kobayashi**, "Do attitudes about immigration predict willingness to admit individual immigrants? A cross-national test of the person-positivity bias," *Public Opinion Quarterly*, 2013, 77 (3), 641–665.
- Jacobson, Cardell K**, "Resistance to affirmative action: Self-interest or racism?," *Journal of Conflict Resolution*, 1985, 29 (2), 306–329.
- Karadja, Mounir, Johanna Mollerstrom, and David Seim**, "Richer (and Holier) Than Thou? The Effect of Relative Income Improvements on Demand for Redistribution," *Review of Economics and Statistics*, 2017, 99 (2), 201–212.
- Kessler, Judd B. and Michael I. Norton**, "Tax aversion in labor supply," *Journal of Economic Behavior & Organization*, 2016, 124, 15–28.
- Kluegel, James R and Eliot R Smith**, "Affirmative action attitudes: Effects of self-interest, racial affect, and stratification beliefs on Whites' views," *Social Forces*, 1983, 61 (3), 797–824.
- Kraus, Michael W, Julian M Rucker, and Jennifer A Richeson**, "Americans misperceive racial economic equality," *Proceedings of the National Academy of Sciences*, 2017, 114 (9), 10324–10331.
- Kuklinski, James H, Paul J Quirk, Jennifer Jerit, David Schwieder, and Robert F Rich**, "Misinformation and the currency of democratic citizenship," *Journal of Politics*, 2000, 62 (3), 790–816.
- , **Paul M Sniderman, Kathleen Knight, Thomas Piazza, Philip E Tetlock, Gordon R Lawrence, and Barbara Mellers**, "Racial Prejudice and Attitudes Toward Affirmative Action," *American Journal of Political Science*, 1997, 4 (2), 402–419.

- Kuziemko, Ilyana and Ebonya Washington**, “Why Did the Democrats Lose the South? Bringing New Data to an Old Debate,” *American Economic Review*, 2018, 108 (10), 2830–67.
- , **Michael I. Norton, Emmanuel Saez, and Stefanie Stantcheva**, “How Elastic are Preferences for Redistribution? Evidence from Randomized Survey Experiments,” *American Economic Review*, 2015, 105 (4), 1478–1508.
- Lergetporer, Philipp, Marc Piopiunik, Lisa Simon et al.**, “Do Natives’ Beliefs About Refugees’ Education Level Affect Attitudes Toward Refugees? Evidence from Randomized Survey Experiments,” Technical Report, CE-Sifo Group Munich 2017.
- List, John A.**, “The Nature and Extent of Discrimination in the Marketplace: Evidence from the Field,” *Quarterly Journal of Economics*, 2004, 119 (1), 49–89.
- Lord, Charles G, Lee Ross, and Mark R Lepper**, “Biased Assimilation and Attitude Polarization: The Effects of Prior Theories on Subsequently Considered Evidence,” *Journal of Personality and Social Psychology*, 1979, 37 (11), 2098.
- Luttmer, Erzo FP and Monica Singhal**, “Culture, Context, and the Taste for Redistribution,” *American Economic Journal: Economic Policy*, 2011, 3 (1), 157–79.
- Mankiw, N Gregory, Matthew Weinzierl, and Danny Yagan**, “Optimal Taxation in Theory and Practice,” *Journal of Economic Perspectives*, 2009, 23 (4), 147–74.

- Mayda, Anna Maria**, “Who is Against Immigration? A Cross-country Investigation of Individual Attitudes Toward Immigrants,” *Review of Economics and Statistics*, 2006, 88 (3), 510–530.
- , **Giovanni Peri**, and **Walter Steingress**, “The Political Impact of Immigration: Evidence from the United States,” 2018.
- Meltzer, Allan H** and **Scott F Richard**, “A Rational Theory of the Size of Government,” *Journal of Political Economy*, 1981, 89 (5), 914–927.
- Miguel, Edward**, **Colin Camerer**, **Katherine Casey**, **Joshua Cohen**, **Kevin M Esterling**, **Alan Gerber**, **Rachel Glennerster**, **Don P Green**, **Macartan Humphreys**, **Guido Imbens et al.**, “Promoting Transparency in Social Science Research,” *Science*, 2014, 343 (6166), 30–31.
- Mummolo, Jonathan** and **Erik Peterson**, “Demand Effects in Survey Experiments: An Empirical Assessment,” *American Political Science Review* (forthcoming), 2018.
- Neumark, David**, **Roy J Bank**, and **Kyle D Van Nort**, “Sex Discrimination in Restaurant Hiring: An Audit Study,” *Quarterly Journal of Economics*, 1996, 111 (3), 915–941.
- Newkirk, Vann R.**, “*The Myth of Reverse Racism*,” 2017. <https://www.theatlantic.com/education/archive/2017/08/myth-of-reverse-racism/535689/> [Accessed: 16/11/2018].
- Newport, Frank**, “Americans Still Say Upper-Income Pay Too Little in Taxes,” April 2016. <http://www.gallup.com/poll/190775/americans-say-upper-income-pay-little-taxes.aspx>.
- Nunley, John M**, **Adam Pugh**, **Nicholas Romero**, and **R Alan Seals**, “Racial discrimination in the labor market for recent college graduates: Evidence

from a field experiment," *The BE Journal of Economic Analysis & Policy*, 2015, 15 (3), 1093–1125.

Nyhan, Brendan and Jason Reifler, "When corrections fail: The persistence of political misperceptions," *Political Behavior*, 2010, 32 (2), 303–330.

Oreopoulos, Philip, "Why Do Skilled Immigrants Struggle in the Labor Market? A Field Experiment with Thirteen Thousand Resumes," *American Economic Journal: Economic Policy*, 2011, 3 (4), 148–171.

Persson, Torsten and Guido Tabellini, *Political Economics: Explaining Economic Policy*, MIT Press, 2000.

Piketty, Thomas, "Social Mobility and Redistributive Politics," *Quarterly Journal of Economics*, 1995, 110 (3), 551–584.

— **and Emmanuel Saez**, "Income Inequality in the United States, 1913-1998," *Quarterly Journal of Economics*, 2003, 118 (1), 1–39.

— , — , **and Stefanie Stantcheva**, "Optimal Taxation of Top Labor Incomes: A Tale of Three Elasticities," *American Economic Journal: Economic Policy*, 2014, 6 (1), 230–271.

Pope, Devin G, Joseph Price, and Justin Wolfers, "Awareness reduces racial bias," *Management Science*, 2018, 64 (11), 4967–5460.

Price, Joseph and Justin Wolfers, "Racial discrimination among NBA referees," *Quarterly Journal of Economics*, 2010, 125 (4), 1859–1887.

Prior, Markus, Gaurav Sood, and Kabir Khanna, "You Cannot be Serious: The Impact of Accuracy Incentives on Partisan Bias in Reports of Economic Perceptions," *Quarterly Journal of Political Science*, 2015, 10, 489–518.

Quillian, Lincoln, Devah Pager, Ole Hexel, and Arnfinn H Midtbøen, “Meta-analysis of field experiments shows no change in racial discrimination in hiring over time,” *Proceedings of the National Academy of Sciences*, 2017, p. 201706255.

Rick, Scott, Gabriele Paolacci, and Katherine Alicia Burson, “Income tax and the motivation to work,” *Journal of Behavioral Decision Making*, 2018.

Saez, Emmanuel and Stefanie Stantcheva, “Generalized Social Marginal Welfare Weights for Optimal Tax Theory,” *American Economic Review*, 2016, 106 (1), 24–25.

–, **Joel Slemrod, and Seth H. Giertz,** “The Elasticity of Taxable Income with Respect to Marginal Tax Rates: A Critical Review,” *Journal of Economic Literature*, 2012, 50 (1), 3–50.

Scheve, Kenneth F and Matthew J Slaughter, “Labor Market Competition and Individual Preferences over Immigration Policy,” *Review of Economics and Statistics*, 2001, 83 (1), 133–145.

Sides, John and Jack Citrin, “How Large the Huddled Masses? The Causes and Consequences of Public Misperceptions About Immigrant Populations,” in “Annual Meeting of the Midwest Political Science Association, Chicago” 2007.

Stephens-Davidowitz, Seth, “The cost of racial animus on a black candidate: Evidence using Google search data,” *Journal of Public Economics*, 2014, 118, 26–40.

Sunstein, Cass R, # *Republic: Divided Democracy in the Age of Social Media*, Princeton University Press, 2018.

Tabellini, Marco, "Gifts of the Immigrants, Woes of the Natives: Lessons from the Age of Mass Migration," Harvard Business School Working Paper 19-005 2018.

Taber, Charles S and Milton Lodge, "Motivated Skepticism in the Evaluation of Political Beliefs," *American Journal of Political Science*, 2006, 50 (3), 755–769.

Tirole, Jean and Roland Bénabou, "Belief in Just World and Redistributive Politics," *Quarterly Journal of Economics*, 2006, 121 (2), 699–746.

Tuch, Steven A and Michael Hughes, "Whites' Racial Policy Attitudes in the Twenty-First Century: The Continuing Significance of Racial Resentment," *The ANNALS of the American Academy of Political and Social Science*, 2011, 634 (1), 134–152.

Zizzo, Daniel John, "Experimenter Demand Effects in Economic Experiments," *Experimental Economics*, 2010, 13 (1), 75–98.