Does Information Change Attitudes Towards Immigrants? Evidence from Survey Experiments*

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Abstract

Many people in the U.S. and in Europe have biased beliefs about immigrants. In this paper, we examine whether providing information about immigrants affects people's attitude towards them. We first use a large representative cross-country survey experiment with more than 19,000 participants to show that people who are told the actual share of immigrants in their country become less likely to state that there are too many of them. We also conduct an online experiment in the U.S., where we provide information about immigration to half of the participants, before measuring their attitude towards immigrants with self-reported and behavioral outcomes. We find that participants in the treatment group update their beliefs about immigrants, and they donate more money to a pro-immigrant charity. However, their self-reported policy preferences remain broadly unchanged, and they do not become more willing to sign a petition in favor of immigration reform. Interestingly, Republicans and people who are worried about immigration respond more strongly to the information treatment, both in terms of their views on immigrants and their policy preferences. Finally, we also measure people's self-reported policy preferences, attitudes, and beliefs in a four-week follow-up, and we show that the treatment effects persist.

Keywords: Biased Beliefs, Survey Experiment, Immigration, Policy Preferences, Persistence.

JEL classification: C90, J15, Z1, Z13

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1 Introduction

In recent years, the United States and many European countries have witnessed a surge in anti-immigrant sentiment (Sniderman et al., 2004), and a large proportion of the population views immigration as one of the most pressing issues facing their country. For instance, more than three quarters of British citizens wanted to reduce immigration in 2015 (Blinder, 2015b), while more than forty percent of Americans reported being very dissatisfied with the level of immigration in the U.S. in January 2016 (Gallup, 2016). Moreover, political parties and candidates with a strong anti-immigration stance have gained support in the last few years, such as the Front National in France, or Donald Trump in the United States.

People's attitude towards immigrants hinges on their beliefs about immigration, which have been shown to be often biased (Blinder, 2015a; Citrin and Sides, 2008; IpsosMori, 2014). Indeed, people consistently over-estimate the proportion of immigrants in their own country, as we show in Figure 1. In the United States, the average person thinks that 37 percent of the population are immigrants, whereas the true Figure is only 13 percent, and we observe similar patterns for other countries. It is therefore particularly important to understand whether people would change their attitude towards immigrants if they received accurate information about immigration.

[insert Figure 1]

To answer this question, we use two data sources. First, we analyze a large cross-country survey experiment, contained in the Transatlantic Trends Survey, which gathered representative data on attitudes towards immigrants from thirteen countries around the world, including the United States, Russia, and several European countries. In the survey, half of the 19,000 respondents were told the proportion of immigrants in their country, before being asked whether they thought that there were too many immigrants in their country. The other half did not receive any information about the proportion of immigrants in their country, but they were asked the same question.

We find that people who were told the exact percentage of immigrants in their country are significantly less likely to say that there are too many immigrants in their country. This effect is particularly pronounced for participants who come from countries with a large share of immigrants, and for people who are concerned about immigration. We also observe that respondents who self-identify as right-wing react more strongly to the information treatment than people on the left of the political spectrum.

This first experiment provides strong evidence that correcting one crucial misconception about immigration can make people less concerned about the number of immigrants in their country. However, it is also important to understand whether a more comprehensive information treatment could durably change people's opinions of immigrants, and affect their policy preferences regarding immigration.

This is why we conducted a second experiment using a large sample of U.S. citizens, where we explicitly test these hypotheses. First, we provided half of the participants with five general facts about immigration in the U.S.: the share of immigrants, the share of illegal immigrants, the unemployment rate and the incarceration rate of immigrants, and the share of immigrants who cannot speak English. Then, we asked all participants to complete a questionnaire on their beliefs about immigrants and their policy preferences. We also obtained two behavioral measures of their attitude towards immigrants, first by asking them how much money they wanted to donate to a pro-immigrant charity, and then by asking them whether they were willing to sign a real petition in favor of immigration reform. To see whether the effects of the information treatment persisted over time, we conducted a follow-up study approximately four weeks after the main experiment.

We pre-specified our empirical strategy and our hypotheses in a pre-analysis plan, which was registered on the Social Science Registry website prior to running the experiment.[1] We find that the information treatment improved people's impression of immigrants, and that it increased people's willingness to donate money to a pro-immigrant charity. These effects are substantial, and they represent a change of approximately 0.25 of a standard deviation in the outcomes.

However, the treatment did not change people's policy preferences regarding immigration, both legal and illegal. For instance, participants who received the information treatment were not more likely to sign a petition in favor of facilitating legal immigration in the U.S., and they were as likely to be in favor of deporting all illegal immigrants as the control group. These effects were precisely estimated, as we had the statistical power to detect very small effect sizes. This evidence indicates that, while providing information can change how people perceive immigrants, it might not be enough to change their policy preferences.

In our follow-up experiment, we asked people the same set of self-reported questions on immigration as the ones they answered in the main experiment. On top of that, we asked them to estimate the same five statistics about immigration as the ones in the main experiment. Overall, 88 percent of the original sample completed the follow-up survey, and we observe no differential

[1] `https://www.socialscienceregistry.org/trials/1092`

attrition between the treatment and the control arm. Importantly, we find that the treatment effects are very similar four weeks after the treatment. Indeed, participants who received the information treatment four weeks earlier still have a more positive opinion of immigrants, even though their policy preferences remain unchanged.

The evidence from the online experiment is consistent with what we found in the cross-country one. In both experiments, we observe that people who receive the information treatment are less likely to say that there are too many immigrants in their country. Moreover, we find similar heterogeneous effects, with the treatment having the strongest effects for people who are more concerned about immigration, and who are more right-wing. For instance, in the online experiment, we find that not only do participants who self-identify as Republicans develop a more positive opinion of immigrants, but they also change their policy preferences accordingly. Similarly, in the cross-country experiment, we observed the largest treatment effects for the subsample who self-identified as right-wing.

Our project contributes to the literature on political misinformation (Gaines et al., 2007; Gilens, 2001; Kuklinski et al., 2003; Nyhan and Reifler, 2010; Taber and Lodge, 2006). Overall, there is mixed evidence on whether correcting people's misperceptions can affect their policy preferences. Some studies, such as Cruces et al. (2013) and Karadja et al. (2014), find that informing people about some relevant facts changes their political opinions. Others, such as Kuziemko et al. (2015), do not see any large effects of information on political preferences.

To the best of our knowledge, only one study has been conducted on the effects of information on people's attitude towards immigrants. Sides and Citrin (2007) conducted a survey experiment with a representative sample of the American population, where they told a random subset of their participants the proportion of immigrants in the U.S., before asking them a series of questions on their attitude towards immigrants. Interestingly, they found that the information they provided had no significant effect on people's attitude towards immigrants.

It is important to note that their evidence is not in line with what we observed in our two survey experiments. Indeed, in the cross-country experiment, people who were told the true proportion of immigrants in their country were significantly less likely to say that there were too many immigrants. This effect becomes even stronger if we restrict our sample to participants from the United States. In our online experiment, we show that people's opinion of immigrants can be changed by an information treatment. Indeed, the information treatment even makes participants donate more money to a charity supporting immigrants in the U.S.

Our two survey experiments have several advantages compared to the existing literature. First, the Transatlantic Trends Survey allows us to get representative evidence from thirteen countries on the effects of information on people's attitude towards immigration, which reduces concerns about external validity. Unlike previous studies, our second experiment uses behavioral measures to assess the impact of information on people's political opinions, instead of relying solely on self-reported measures. Finally, our follow-up experiment allows us to show that the treatment effects persist over time, and that people retain the information that they received. This is particularly important as experimenter demand (Zizzo, 2010) is much lower in the four-week follow-up, where no additional treatment was administered (Cavallo et al., 2014).

Our paper also contributes to the literature on immigration (Bianchi et al., 2012; Brader et al., 2008; Carlsson et al., 2015; Hainmueller et al., 2015; Hainmueller and Hopkins, 2014a; Knoll et al., 2011; Mastrobuoni and Pinotti, 2015; Scheve and Slaughter, 2001). Indeed, some studies have tried to explain people's attitude towards immigrants by focusing on characteristics such as age, education or income (Card et al., 2012; Citrin et al., 1997; d'Hombres and Nunziata, 2016; Dustmann and Preston, 2001, 2006; Mayda, 2006; Mayda and Facchini, 2009), while others have focused on how information shapes people's policy preferences regarding immigration (Hainmueller and Hiscox, 2010; Hainmueller and Hopkins, 2014b; Sides and Citrin, 2007). Our results add to this body of evidence by showing how misperceptions about immigrants affect people's attitude towards immigrants as well as their political preferences.

This paper proceeds as follows: in section 2, we outline the evidence from the cross-country survey experiment. In section 3, we present the design of the online experiment. The results from the online experiment are described in section 4. Finally, section 5 concludes.

2 Cross-Country Experiment

2.1 Description of the Dataset

We use data from the Transatlantic Trends Survey, which is a large representative survey on political attitudes conducted every year in the U.S. and in many other countries around the world. In particular, we focus on two waves of the survey, the 2010 and 2014 waves, which are the only ones that included an experiment on the effect of information on people's attitude towards immigration.

The 2010 wave of the Transatlantic Trends Survey was conducted in the United States, Canada, Germany, France, Italy, the UK, the Netherlands and Spain. In each country, participants were randomly drawn from the adult population who had access to a landline.[2]

The 2014 wave was conducted in the United States, Germany, France, Italy, the UK, the Netherlands, Spain, Greece, Portugal, Sweden, Russia and Poland. In most countries, participants were randomly drawn from the adult population who had access to a landline or a mobile phone. In Germany and in the UK, only people with access to a landline could take part in the survey. In Poland and Russia, participants were randomly selected from the general population, and face-to-face interviews were conducted instead of phone interviews.

To increase the response rate for both waves, respondents were contacted up to five times for telephone interviews, and up to four times for face-to-face interviews. The response rate for phone interviews ranged from 4 percent in France, the UK and the Netherlands to 27 percent in the US.[3] For face-to-face interviews, the response rate was significantly higher (49 percent in Russia and 40 percent in Poland) (Stelzenmueller et al., 2014; Wunderlich et al., 2010). Importantly, more than 94 percent of people who started the survey answered the main questions of interest, which means that attrition is not an issue for this experiment.

In order to get as representative a sample as possible for each country, we use the probability weights constructed by the Transatlantic Trends Survey in the main analysis. It is important to note that our results are not affected in any way by the use of these weights, which shows that our results are robust to slight changes in the sample composition.

2.2 Information Treatment

At the start of the survey, participants were asked which issues they thought were the most important ones facing their country, and how closely they followed news on immigration. Then, they were randomly asked one of the following two questions:

- **Treatment:** As you may know, according to official estimates, around [X] percent of the [COUNTRY] population was born in another country. In your opinion, is this too many, a lot but not too many, or not many?

[2] The landline numbers were first randomly drawn. Then, the respondent was randomly chosen among the people who had access to that landline, using a randomization procedure based on birth dates.

[3] These response rates are comparable to response rates for similar phone-based surveys of the general population.

- **Control:** Generally speaking, how do you feel about the number of people living in [COUNTRY] who were not born in that country? Are there too many, a lot but not too many, or not many?

The main difference between the two questions is that, in the first question, participants are informed about the true proportion of immigrants in their country, before being asked whether they think that there are too many immigrants. In other words, the first question includes an information treatment, whereas the second one does not. Since most people over-estimate the proportion of immigrants in their country, this piece of information will presumable change people's answer to the question.

Then, participants had to answer a large set of questions on immigration, which are not particularly relevant for our analysis. For instance, there were questions on people's perception of crime rates for immigrants, or on the terrorism risks caused by illegal immigration.[4] There is no reason to believe that the information treatment should affect the way people answer these questions, and we therefore do not include them in our main analysis.[5]

In the 2010 wave of the survey, participants in the control group were asked at a later point in the survey to estimate the proportion of immigrants in their country. Then, they were told the actual share of immigrants in their country, and they were asked again if they thought that there were too many, a lot but not too many, or not many immigrants. This allows us to see if participants in the control group changed their original answer after learning the actual proportion of immigrants in their country.

2.3 Results

2.3.1 Sample Characteristics

When we combine the two waves, we end up with a total sample of more than 19,000 observations. Table 13 in Appendix A summarizes the characteristics of the whole sample.[6] Overall, 46 percent of the respondents are male, and the average age of the participants is 49. Moreover, 30 percent of our sample only finished secondary school, while 27 percent graduated from college. Approximately 40 percent of respondents are employed full-time, and only 14 percent are em-

[4] The full list of questions is available in Wunderlich et al. (2010) and Stelzenmueller et al. (2014).

[5] We checked that these outcomes are not affected by the treatment in any meaningful way.

[6] Some of the demographic variables were only present in the 2010 wave, but not in the 2014 one, which explains why the sample size is not the same for all variables.

ployed part-time. Finally, 30 percent of participants self-identify as left-wing, while 42 percent self-identify as right-wing.

2.3.2 Randomization Check

To check that the randomization was correctly implemented, we perform a randomization check. Specifically, we examine whether respondents in the treatment group and in the control group have similar characteristics on average by regressing each pre-determined variable on the treatment indicator. As Table 14 in Appendix A indicates, the treatment group and the control group are very similar on average. There are two very small imbalances, as participants in the control group are slightly less likely to have a post-graduate degree, and slightly more likely to have gone to high school.[7] Moreover, when we regress the treatment indicator on the set of pre-determined characteristics, we find that the joint F-test is not statistically significant (p-value = 0.14), which provides additional evidence that the randomization worked as expected.

2.3.3 Main Results

We hypothesize that participants who are informed about the proportion of immigrants in their country will be less likely to say that there are too many immigrants, and more likely to say instead that there are not many immigrants. For the main analysis, we therefore estimate the following equation:

$$y_i = \alpha_0 + \alpha_1 T_i + \varepsilon_i \tag{1}$$

where y_i is the outcome variable, T_i is the treatment indicator, which is equal to one if the participant was told the proportion of immigrants in their country, and zero otherwise. The idiosyncratic error term is denoted as ε_i. Throughout the analysis, we use robust standard errors to account for potential heteroskedasticity in the error term. The main coefficient of interest is α_1, which corresponds to the treatment effect.

As Panel A of Table 1 clearly shows, people who receive some information about the share of immigrants in their country become much less likely to say that there are too many immigrants in their country (column 1), and they become more likely to say that there are not many immigrants (column 2). It is important to note that the effect sizes are very large. Indeed, the probability of

[7] Our results are not affected by the inclusion of education as a control variable.

saying that there are too many immigrants is 12 percentage points (33 percent) lower for those who receive the treatment, while the probability of saying that there are not many immigrants is 16 percentage points (approximately 50 percent) higher for those who get the information treatment.[8]

In column 3 of Panel A, we present the results from a regression where the dependent variable is equal to one if participants say that there are too many immigrants, two if they state that there are a lot of immigrants but not too many, and three if they think that there are not many immigrants in their country. Once again, we find a very large and highly significant effect of the information treatment on people's perception of immigration.[9]

In Panel B of Table 1, we show that our results hold even if we use a different empirical strategy. Indeed, in the 2010 wave of the survey, participants in the control group first had to answer the question without knowing the proportion of immigrants in their country, but later in the survey, they were told the actual proportion of immigrants, and they were asked the same question again. We can therefore compare their answers before receiving the information to their answers after they received the information. The equation we estimate is:

$$y_i = \alpha_0 + \alpha_1 Post_i + \varepsilon_i \tag{2}$$

where y_i is the outcome variable, and $Post_i$ is a dummy variable which is equal to one if y_i is the answer the participant gave after receiving the information treatment, and zero otherwise. We find that the results are very similar to the ones we observed in Panel A, and if anything, they are slightly larger.

[insert Table 1]

2.3.4 Heterogeneous Treatment Effects: By Countries

In Figure 2, we show for each country the proportion of people in the control group and in the treatment group who say that there are too many immigrants in their country. In most countries, the information treatment reduces the likelihood of people saying that there are too many immigrants in their country. Interestingly, we find a high degree of heterogeneity in terms of the magnitude of the treatment effect, and we observe the largest effect sizes for countries

[8] These results are robust to the inclusion of control variables, and of fixed effects for the different waves and the different countries. These results are omitted for brevity's sake, but are available upon request.

[9] results are robust to using ordered logit or probit regressions instead of OLS.

where many people think that there are too many immigrants, such as Greece, Italy, the UK, and the U.S. These results are also displayed in Table 2.

[insert Figure 2]

[insert Table 2]

2.3.5 Heterogeneous Treatment Effects: by Individual-Level Characteristics

In this section, we evaluate whether there are important heterogeneous treatment effects caused by the information treatment.[10] Specifically, we will estimate the following equation, where $interaction_i$ refers to the interaction variable:

$$y_i = \pi_0 + \pi_1 Treatment_i \times interaction_i + \pi_2 Treatment_i + \pi_3 interaction_i + \varepsilon_i$$

We first test whether people who think that immigration is the most important issue facing their country react more strongly to the information treatment. Specifically, we construct an indicator variable which is equal to one if people say that immigration is the most important issue facing their country. In Panel A of Table 3, we show that people who are more concerned about immigration react more strongly to the treatment, although this effect is not statistically significant.

However, we do find that people who think that the main reason why immigrants come to their country is to receive social benefits respond particularly strongly to the treatment.[11,12] Indeed, the treatment effect is twice as large for that group of people, as can be seen in Panel B of Table 3.

In Panel C of Table 3, we examine heterogeneous treatment effects by people's political orientation. We first create a dummy variable which is equal to one if people say that their political orientation is center right, right, or extreme right, and zero otherwise. Interestingly, we find that treated individuals who self-identify as right-wing react more strongly to the treatment.

[10] For all of the heterogeneity analysis, we use either questions which were asked before the treatment, or pre-determined characteristics, such as political orientation.

[11] We created an indicator variable, called "negative view on immigrants", which is equal to one if people state that the main reason why immigrants come to their country is to receive social benefits, and zero if they think that it is for other reasons, such as to be united with family members, to seek asylum, to work or to study.

[12] This question was only asked in the 2014 wave of the survey, which is why we restrict the analysis to the 2014 wave.

[insert Table 3]

3 Online Experiment

The cross-country experiment showed, using representative data from thirteen countries around the world, that information can change the way people felt about the number of immigrants in their country. However, we do not know whether this effect would persist over time, and whether the information treatment would also change people's policy preferences and their behavior towards immigrants. Moreover, the information given to participants was very limited, since it only concerned the proportion of immigrants in the country. To test whether information could change people's perception of immigrants and their policy preferences, we designed an experiment which provides information about different facets of immigration, and not just the proportion of immigrants in the country.

3.1 Experimental Design

3.1.1 Main Experiment

The experiment is structured as follows: First, all respondents are asked a few questions on how much they trust official statistics, how many petitions they have signed in the last 12 months, and how worried they are about immigration. Then, we ask them to estimate five important statistics about immigration. Specifically, they are asked to estimate the proportion of immigrants in the U.S., the proportion of illegal immigrants in the U.S., the unemployment rate of immigrants, their incarceration rate, and the proportion of immigrants who cannot speak English.[13],[14]

In order to help participants give plausible estimates for the unemployment rate and the incarceration rate of immigrants, we tell them what these rates are for U.S.-born citizens.[15] For each question, participants receive 10 cents if their estimate is within three percentage points of the official value, which we obtained from the American Community Survey. To avoid having participants look up the answers online, we only give them 25 seconds to answer each question. We also ask them how confident they are that their answer is correct.

[13]We chose these statistics for two main reasons. First, there is some evidence showing that people are particularly concerned about these issues. Second, there exists Census data on these issues, which increases the reliability of the information we provide.

[14]For a complete description of the experimental design, please refer to the pre-analysis-plan.

[15]It is important to note that both the treatment and the control group receive this information, and the internal validity of our study is therefore not compromised.

Then, the treatment group is told what the correct answers are to these five questions, while the control group is not told anything. We remind participants in the treatment group of the estimate they gave, before providing them with the correct answer. For instance, participants get the following feedback for the question on the unemployment rate of immigrants:[16] *"You estimated that X percent of immigrants are unemployed. According to the American Community Survey, around 6 percent of immigrants are unemployed."*

We then ask all the participants a series of questions on their perception of legal and illegal immigrants, as well as on their policy preferences. For instance, we ask them whether they think that there are too many immigrants in the U.S., whether legal immigration should be reduced, whether immigrants have a negative impact on American society as a whole, etc.

We also use two behavioral measures to assess whether the treatment changed our participants' attitude towards immigrants.[17] First, we give participants the option of signing an online petition in favor of facilitating legal immigration into the U.S., by increasing the number of green cards available for immigrants.[18] Second, we tell participants that ten percent of them will receive ten dollars, and that they must specify how much money they want to keep for themselves, and how much they want to give to the American Immigration Council, a non-profit organization which "promotes laws, policies, and attitudes that preserve [the United States'] proud history as a nation of immigrants" (Council, 2016), in case they receive the ten dollars.

Then, we ask participants in the treatment group to estimate the same five statistics as before (proportion of immigrants, proportion of illegal immigrants etc.), so that we can test whether they remember the information that we gave them. Finally, respondents complete a questionnaire on demographics, which includes variables on gender, age, education, income, etc.

3.1.2 Follow-Up Experiment

We also ran a follow-up experiment four weeks after the main experiment, where we asked people the same set of self-reported questions on immigration as the ones they answered in the main experiment. We also asked them to estimate the same five statistics about immigration as the ones in the main experiment.[19] This allows us to see whether people in the treatment group

[16] To make the treatment more salient, we also present the feedback using bar charts, where we show participants their estimate and the correct one.

[17] We randomize the order of the behavioral measures.

[18] The petition can be found online at the following URL: `https://petitions.whitehouse.gov//petition/facilitate-legal-immigration-us`.

[19] See Appendix B for a complete description of the follow-up experiment.

remember the information we gave them in the main experiment.

Half of the sample in the follow-up experiment had to estimate the five statistics first, and then answer the set of self-reported questions on immigration, while the other half of the sample had to answer the set of self-reported questions on immigration first, and then had to estimate the five statistics. This allows us to check whether the order of the questions affects people's answers.[20]

3.2 Description of the Sample

3.2.1 Amazon Mechanical Turk

We conducted this experiment on Amazon Mechanical Turk (MTurk), an online labor marketplace developed by Amazon.com, which is now commonly used by academics to recruit participants for online experiments (Paolacci and Chandler, 2014). Indeed, MTurk offers researchers a very efficient way of getting subjects for their studies, given that the pool of workers available on the platform is very large and much more representative of the US population than other convenience samples, such as student samples.

Moreover, MTurk participants have been shown to be more attentive to instructions than college students (Hauser and Schwarz, 2016), and to give high-quality answers. One reason for this is that participants on MTurk have an extra incentive to pay attention to the quality of their answers, since their approval rating depends on the quality of their submissions. Workers care about having a high approval rating as it allows them to complete more tasks and earn more money. To guarantee that the data we obtain is reliable, we only allowed workers with an overall rating of more than 95 percent to take part in our study (Peer et al., 2014).

To show that participants were very attentive when responding to our survey questions, we leverage the fact that participants had to answer the same questions in the main experiment and in the follow-up. If participants did not pay attention to the survey and gave random answers, we would expect to see very low correlations between our baseline and follow-up measures. We find that, for the control group, the correlation of the outcome variables between the main experiment

[20] We did not include any of the behavioral measures in the four-week follow-up as it would not make sense to ask people to sign the same petition a second time and to donate to the same charity twice. Using a different petition or a different charity would also have posed some problems, as we can expect people's behavior to be dynamic. For instance, those who signed the first petition might be less inclined to sign the second one, and those who already donated might be less inclined to donate to another charity.

and follow-up is very high (on average above 0.7) and above 0.8 for the indices.[21] These temporal correlations are comparable in magnitude to the ones found in previous studies on the stability of economic preferences over time, such as Meier and Sprenger (2015). This evidence strongly suggest that the data we collected is of a very high quality.

One criticism often levied against MTurk is that people on MTurk can easily misreport their nationality, and pretend that they come from the United States (Krupnikov and Levine, 2014). While this used to be a significant problem several years ago, it is no longer the case. Indeed, Amazon drastically improved its account verification procedure in 2013, which led to major shifts in the composition of the workforce (Ipeirotis, 2013). Not only did they disable most of non-US accounts, but they also required both new and existing workers to provide their social security number, their full legal name, and their real address. Workers who did not comply with these new rules saw their accounts deactivated.

People on MTurk are on average fairly experienced in taking surveys, which constitutes a potential threat to the external validity of our results (Chandler et al., 2014). To address this concern, we examine heterogeneous treatment effects by the number of tasks that our participants had previously completed on MTurk. We find no evidence for heterogeneous treatment effects by our participants' experience on MTurk.[22] This indicates that more experienced workers do not behave differently from less experienced ones in our study.

3.2.2 Main Sample

In order to participate in the experiment, people had to be based in the United States, have an overall rating of more than 95% and have completed more than 500 tasks on MTurk.[23]

The experiment was run on the 3rd March 2016 between 4pm and 6pm EST. In total, 802 participants completed our experiment. Less than 10 people dropped out after the treatment, which means that the attrition rate was less than two percent.[24] This is a particularly low drop-out rate for online experiments, which can be explained by the fact that the average wage per

[21] We only calculate these correlations for the control group, as we expect the treatment effects to vary over time, which would mechanically lower the temporal correlations.

[22] These results are not reported, but are available upon request.

[23] This means that at least 95% of the tasks completed by these workers were approved by the people who employed them. A task can be anything from classifying images to participating in an academic study. A threshold of 500 tasks is not very high, but it guarantees that participants are not newcomers.

[24] In the analysis, we include people who dropped out after the treatment, which explains why the sample size is sometimes slightly larger than 802.

participant was more than \$10 per hour. This is significantly higher than the average wage on MTurk, which is only \$4.80 according to Mason and Suri (2012).

Table 14 in Appendix A summarizes the characteristics of the whole sample. Overall, 55 percent of participants are male. The median age in our sample is 35, while the median age in the U.S. is 37.8 (CIA, 2015). Moreover, the median income in our sample is \$45,000, compared to \$52,000 for the general population. Similarly, 78 percent of our participants identify as white, while the proportion of white people in the U.S. is 80 percent (CIA, 2015). The proportion of unemployed people in our sample (8%) is slightly higher than in the general population (5.1%) (Bureau of Labor Statistics, 2015).

3.2.3 Follow-Up Sample

Four weeks after our main experiment, we re-invited everyone who had completed the main experiment for a follow-up survey. The follow-up survey was conducted between the 30$^{\text{th}}$ March 2016 and the 5$^{\text{th}}$ April 2016. In total, 92 percent of participants who had completed the first experiment took part in the follow-up experiment. 96 percent of those who started the follow-up survey finished it, which implies that the proportion of participants who completed both the main experiment and the follow-up is 88 percent. Our recontact rate is extremely high compared to other MTurk studies, where the recontact rates ranged from 20 percent (Kuziemko et al., 2015) to 70 percent (Berinsky et al., 2012). This difference can be partly explained by the fact that the hourly pay for our follow-up study was above \$15, which is much higher than what workers on MTurk usually earn. Moreover, our follow-up survey only took approximately four minutes to complete.

It is important to note that the recontact rate for the treatment group and the control group are very similar, and statistically indistinguishable (p-value = 0.708). Moreover, very few baseline characteristics predict the completion of the follow-up survey, which suggests that the overall sample composition remained more or less unchanged.

4 Results

4.1 Pre-Analysis Plan

Before running the experiment, we pre-registered the experimental design, our hypotheses and our empirical specifications on the Social Science Registry. All of the analyses presented in this paper were pre-specified.[25]

4.2 Baseline Balance

In Appendix A table 15, we examine in how far the control group and the treatment group differ in terms of observable characteristics, by regressing each variable on the treatment indicator.[26] Overall, we find that the two groups have very similar characteristics. We only observe two imbalances: people in the treatment group are less likely to be unemployed, and their estimate of the unemployment rate of immigrants is higher than for the control group. Given that we tested for 24 variables, it is not unexpected to find a few imbalances.[27]

To test whether this set of variables jointly predict the treatment assignment, we regress the treatment indicator on the pre-determined variables, and we observe that the F-test is significant at the ten percent level (p-value of 0.09).[28] We will show in the main analysis that our results are robust to the inclusion of these pre-determined characteristics as control variables. If anything, we find that including control variables improves the precision of the estimates, and increases the size of the treatment effects.

In Appendix A Table 16, we display the characteristics of the treatment group and the control group for the follow-up sample. We only find a couple of small imbalances when we regress the baseline characteristics on the treatment indicator. Furthermore, we show that the F-test for the regression of the treatment indicator on the baseline characteristics is statistically insignificant, which indicates that the treatment group and the control group are globally balanced.

[25] The full pre-analysis plan can be accessed at `https://www.socialscienceregistry.org/trials/1092/history/7106`.

[26] For the balance test, we use the 24 variables which were specified in the pre-analysis plan.

[27] We implemented the randomization using a JavaScript random number generator embedded in the survey. The observed imbalances are therefore due to chance alone.

[28] Some people did not provide an estimate for the five statistics within the time limit and some people did not respond to all questions, and there are therefore some missing values in the data. We include these observations in the regression by coding the missing values as zero and by including for each question with missing values a dummy variable which is equal to one if the participant failed to give an answer for that question.

4.3 Update in Beliefs

We first check that participants in the treatment group updated their beliefs about immigrants after having received the information treatment. In Panel A of Table 4, we show the average estimates that participants in the treatment group gave before receiving the correct information. The estimates that participants give after the treatment are presented in Panel B.

[insert Tables 4 and 5]

It is clear that, before the treatment, participants had very biased beliefs about immigration. Indeed, their estimates were on average consistently higher than the actual values. For instance, they over-estimated the percentage of immigrants in the U.S. by more than 8 percentage points, and the share of immigrants who are unemployed by more than 15 percentage points.

Moreover, participants strongly update their estimates after receiving the treatment. Indeed, the mean bias in the answers goes down by more than ten percentage points on average. The distribution of the answers also changes dramatically, and most people give the correct estimate after the treatment. For instance, almost three quarters of participants give correct estimates for the share of incarcerated immigrants after having received the information treatment. In Panel C of Table 4, we show that there is a large and statistically significant difference between the estimates given before the treatment and after the treatment.

We also test the extent to which people in the treatment group remember the information four weeks after the main experiment. In Table 5, we show that people's estimates four weeks after the treatment are still fairly accurate, and that they are different from people's estimates before the treatment (Panel D). For instance, the average estimate of the proportion of immigrants is fifteen percent in the follow-up, whereas the true value is thirteen percent. However, we do find that the variance of estimates is larger for the treatment group in the follow-up than in the main experiment, as can be seen in Figures 3-7. Finally, it is important to note that people's estimates in the follow-up are statistically different from their estimates in the main experiment (Panel E).

[insert Figures 3 - 7]

4.4 Main Results

4.4.1 Empirical Strategy

In this section, we explore how the information treatment affected people's beliefs and attitudes towards immigration, as well as their policy preferences regarding immigration. To do so, we simply compare the behavior of people in the treatment group with that of people in the control group, by estimating the following equation:[29]

$$y_i = \pi_0 + \pi_1 Treatment_i + \varepsilon_i$$

where y_i is the outcome variable, and $Treatment_i$ is the treatment indicator. For the sake of clarity, we recode all of our outcomes such that higher values denote a more positive attitude towards immigrants. We also present all results controlling for all variables used in the pre-specified balance test.[30]

We account for multiple hypothesis testing in two ways. First, we adjust the p-values using the "sharpened q-value approach" (Anderson, 2008; Benjamini et al., 2006). For each family of outcomes, we control for a false discovery rate of 5 percent, i.e. the expected proportion of rejections that are type I errors (Anderson, 2008).[31] For each table, we also create an index of the outcomes, which we regress on the treatment indicator, as specified in the pre-analysis plan.

4.4.2 Results

In Table 6, we show that, compared to the control group, the treatment group is less inclined to say that immigrants are more likely to commit crimes than U.S. citizens. Moreover, participants in the treatment group are more likely to state that immigrants generally learn English within a reasonable amount of time, and that the unemployment rate of immigrants is similar to that of U.S. citizens. All of these results are statistically significant and the effect sizes are large (0.3 of a standard deviation). We also show that the treatment effect on the index is highly significant, and very large. Indeed, the treatment effect corresponds to half of the gap between Democrats and Republicans for this index in the control group.[32] We also show in Table 6 that these effects persist four weeks after the treatment, that they are statistically significant, and that they remain

[29] Robust standard errors are used throughout the analysis.

[30] We use the same strategy as before to deal with missing values. Our results are nearly identical when we do not recode missing values in this way, and are available upon request.

[31] These adjusted p-values are displayed in the tables as FDR-adjusted p-values.

[32] On average, Republicans have a significantly more negative view of immigrants than Democrats.

fairly large (about 0.25 of a standard deviation effect size). This evidence demonstrates that the information treatment durably changed people's beliefs about immigrants, and that the results from the main experiment are not simply caused by experimenter demand.[33]

[insert Table 6]

In fact, the information treatment also had an effect on how people perceive immigration more generally, as shown in Table 7. Indeed, people in the treatment group were less likely to say that immigrants have produced more disadvantages than advantages for the U.S. as a whole over the last ten years. When we include controls, this result is significant at the five percent level, and the effect size is around 0.2 of a standard deviation. However, people in the treatment group did not change their opinion as to whether removing almost all illegal immigrants from the U.S. would have a positive or a negative impact on the economy. In the four-week follow-up, we observe very similar treatment effects, and some of them are actually slightly larger than in the main experiment. For instance, people in the treatment group are slightly more likely to state that deporting most illegal immigrants would not have a positive impact on the economy.

The strongest evidence that the information changed people's overall attitude towards immigrants is that participants in the treatment group donated more money to a pro-immigration charity than the control group. Indeed, people in the treatment group donated on average \$0.44 more to the American Immigration Council than the control group. As shown in Column 4 of Table 7, this effect is highly statistically significant, and the effect size is moderate once we include control variables (0.2 of a standard deviation). Put differently, the treatment effect is equal to one third of the difference in donation between Democrats and Republicans in the control group. In Figure 8, we also observe that the proportion of people donating nothing to the American Immigration Council is ten percentage point lower in the treatment group than in the control group, which is consistent with the idea that the information treatment improved the general impression people had of immigrants.

[insert Table 7]

[insert Figure 8]

It is also interesting to note that people in the treatment group are less likely to state that there are too many legal and illegal immigrants in the U.S., as shown in Columns 1 and 2 of Table

[33] See Cavallo et al. (2014) for a similar argument.

8. These effects are highly statistically significant, their effect size is large (approximately 0.3 of a standard deviation), and they persist even four weeks after the main experiment. To a large extent, these results replicate the findings of the cross-country experiment. When people learn the actual proportion of immigrants in their country, they become less inclined to say that there are too many immigrants.

However, this does not imply that they change their policy preferences regarding immigration. Indeed, if we look at Table 8, we clearly see that participants in the treatment group do not change their views on the number of green cards to issue every year, or on the legalization of illegal immigrants. Similarly, their views on the budget that should be devoted to deporting illegal immigrants are not affected by the treatment. All these effects are very small in magnitude (mostly below .05 of a standard deviation) and precisely estimated, and we can therefore be confident that the treatment did not significantly affect these variables. In the four-week follow-up, we see slightly larger treatment effects, but their size remains fairly small. This result is compatible with the evidence by Kuziemko et al. (2015) who found that redistributive preferences are inelastic to information about income inequality in the US.

[insert Table 8]

Moreover, in Table 9, we observe that the treatment group is not more likely to sign an online petition on the White House's website in favor of increasing the number of green cards available for immigrants.[34] If anything, we observe the opposite, since the number of signatures is higher for the control group than the treatment group. However, this difference is only marginally statistically significant, and this result is not robust to correcting for multiple hypothesis testing. Moreover, we do not see any difference in the number of people who intended to sign the petition, and in the number of people who reported signing the petition.[35]

[insert Table 9]

Overall, we find that the information treatment improved people's impression of immigrants, but that it did not affect people's policy preferences regarding immigration. Indeed, although

[34] It is worth noting that about 10 percent of our sample actually ended up signing the petition. This in turn means that we had sufficient variation in order to detect treatment effects.

[35] The number of people who reported having signed the petition is higher than the number of signatures, which can partly be explained by the fact that signing the petition was a multi-stage process. Indeed, people who signed the petition received a confirmation email which contained a link that they had to click on to confirm their signature. If they did not complete this second step, their signature was not counted.

participants in the treatment group donated more money to a pro-immigrant charity, they were not more likely to sign a petition in favor of increasing the number of green cards available for immigrants. In other words, correcting people's misconceptions about immigration has a positive impact on how people view immigrants, which is however not reflected by a change in policy preferences.

Our evidence that the treatment group increased donations to a pro-immigrant charity is in line with a literature documenting that providing people with information about recipients can affect social and psychological proximity and thereby affect giving (Bohnet and Frey, 1999; Charness and Gneezy, 2008; Small and Loewenstein, 2003). It is also in line with previous evidence emphasizing that social interactions with outgroup members can lower prejudice (Burns et al., 2015; Rao, 2013) and our results provide clean evidence that information per se can be sufficient in changing attitudes towards outgroup members.

4.5 Heterogeneous Treatment Effects

In the main analysis, we focused on five families of outcomes: people's beliefs about immigrants, their general opinion on immigration, their generosity towards a pro-immigrant charity, their policy preferences, and their willingness to sign a petition in favor of more green cards.[36] For all of the heterogeneity analysis, we only look at the indices for these families of outcomes, as this makes the interpretation easier. Specifically, we estimate the following equation, where $interaction_i$ refers to the interaction variable, and X_i is a vector of pre-determined characteristics:[37]

$$y_i = \pi_0 + \pi_1 Treatment_i \times interaction_i + \pi_2 Treatment_i + \pi_3 interaction_i + X_i \varepsilon_i$$

In Panel A of Table 10, we examine whether participants who have a high level of trust in official statistics respond more strongly to the information treatment. Indeed, one might expect that people who do not trust official statistics will not change their beliefs regarding immigration after receiving the information treatment, whereas people who trust official statistics will. However, we do not find any convincing evidence that this is the case, given that the interaction term is not statistically significant for almost all families of outcomes.

In Panel B, we show that participants who are particularly worried about immigration tend

[36] A precise definition of the different families can be found in the pre-analysis plan: `https://www.socialscienceregistry.org/trials/1092`

[37] We include control variables in the analysis due to the slight imbalances we observed between the treatment group and the control group.

to respond more strongly to the treatment. Indeed, not only do they change their views on immigrants, but they also become more supportive of immigration reform. However, they are not differentially more generous towards the American Immigration Council, and they are not more willing to sign a petition in favor of immigration than people who are not particularly worried about immigration. This result is particularly striking as it is in line with what we had observed in the cross-country experiment.

Finally, in Panel C, we find that people who self-identify as Republican respond more strongly to the information treatment than people who identify as Democrat or as neither Republican nor Democrat. In particular, we observe that Republicans update their beliefs about immigrants, and generally become more supportive of immigration reform after the treatment compared to other political groups.[38] Indeed, they even become more willing to sign a petition in favor of increasing the number of green cards available to immigrants.[39] It is important to note that this effect is in line with what we had found in the cross-country experiment. The high level of consistency across datasets is a very strong indication that these heterogeneous treatment effects are not just an artifact of the composition of our online sample.

We also examined heterogeneous treatment effects by people's biases in beliefs with three different pre-specified measures of biases. We find that the patterns are as expected: people with higher biases seem to respond more strongly to information. However, this effect is not statistically significant for most families of outcomes. We believe that this can be due to measurement error as we do not know how people weight the five different biases, which causes some issues for the aggregation of the biases. This measurement error naturally results in attenuation bias which renders the detection of significant effects much harder. These results can be found in Table 1 of the online Appendix.

[insert Table 10]

In Table 11, we show that the heterogeneous treatment effects are qualitatively similar in the follow-up. Indeed, we find that, even four weeks after the treatment, the effects are stronger for Republicans, especially regarding policy preferences and their general opinion of immigrants. There is also some indication that people who are very worried about immigration still respond

[38] We have verified that these effects are robust to other measures of political conservatism. For example, we find that people planning to vote for Trump and Cruz in the primaries respond more strongly to the information and that people who self-identify as conservatives show a stronger reaction to our treatment. These additional results (which were not-pre-specified) are available upon request.

[39] The p-value for the effect of treatment on the petition index is 0.12 for Republicans.

more strongly to the treatment, although the interaction effect is not as large in the follow-up. Finally, we do not observe any heterogeneous treatment effects for people who trust official statistics.

[insert Table 11]

4.6 Mechanisms

In this section, we will consider different explanations for why the information treatment affects people's attitude towards immigrants, but not their policy preferences.

4.6.1 Main Mechanisms

First, it could be the case that policy preferences are more stable than attitudes, which would explain why we only observe treatment effects on people's attitude towards immigrants. For instance, policy preferences are often influenced by party affiliation, which tends to stay the same over time. Similarly, other stable characteristics could have a very strong influence on people's policy preferences, which would make it more difficult to find treatment effects. To corroborate this hypothesis, we find that temporal correlations are higher for policy preferences than for attitudes.[40]

Another explanation is that the questions on policy preferences are less directly related to the information contained in the treatment than some of the attitudinal questions. The topics mentioned in the attitudinal questions were addressed in the information treatment, which is not necessarily the case for the policy preference measures. For instance, when people learn that the incarceration rate of immigrants is much lower than they thought, they should change their views on the criminality of immigrants, but not necessarily their policy preferences.

Still, we observe that the information treatment changes people's general attitude towards immigrants. For instance, participants in the treatment group are more likely to state that immigrants have produced more advantages than disadvantages in the past ten years, and this effect is highly statistically significant when we include controls. This particular question is not directly related to the information provided in the treatment, but we nevertheless observe a fairly large treatment effect. This shows that the lack of treatment effects for the policy preference

[40] We only calculate these correlations for the control group, as we expect the treatment effects to vary over time, which would mechanically lower the temporal correlations.

measures cannot be entirely explained by the fact that these measures are less directly related to the information contained in the treatment.

4.7 Potential Confounds

4.7.1 Experimenter Demand Effects

We are confident that our results are not caused by experimenter demand effects (Zizzo, 2010). First of all, online studies have been shown to be less affected by experimenter demand effects (Kreuter et al., 2008; Van Gelder et al., 2010), since participants do not interact at all with the experimenter. Moreover, if experimenter demand effects were driving our results, we would expect stronger treatment effects in the non-incentivized measures compared to the incentivized one, where participants need to forgo some of their own money. We find that the effect of the treatment is larger for donation to a pro-immigrant charity than for some of the unincentivized measures, which is reassuring.

In addition, the fact that our treatment effects persist four weeks after the main experiment suggests that experimenter demand effects cannot explain the patterns in our data. Indeed, it seems highly unlikely that respondents from the treatment and the control group will hold different beliefs about the experimenters' hypotheses and intentions four weeks after receiving the information treatment (Cavallo et al., 2014).[41]

4.7.2 Other Potential Confounds

One might also worry that participants become less focused as the survey progresses, and become more prone to giving random answers as time passes. This would imply that treatment effects would be lower for questions coming later, such as our measures of policy preferences. We can rule out this mechanism by looking at the stability of the control group's answers between the main experiment and the follow-up. Indeed, if people are more likely to give random answers for the policy preferences questions in the main experiment, we should observe that the correlation over time of policy preferences is lower than for other questions. In Appendix A Table 19, we show that the opposite is true: in the control group, policy preferences are more stable over time than the other questions which come earlier in the survey.[42] This indicates that the lack of

[41] This is particularly true for respondents who answered the explicit questions before the factual questions on immigrants. We find no heterogeneous treatment effects by the order in which questions were presented in the follow-up.

[42] We only look at the control group in order not to confound our estimates of stability of preferences over time with time-varying treatment effects.

treatment effects for policy preferences is not due to measurement error, but simply reflects the fact that the treatment did not affect the participants' policy preferences.

4.8 Determinants of the Biases

In this section, we try to understand why some people have much more biased beliefs about immigration than others. For each type of belief, we simply regress the size of the bias on a large set of demographics, such as income, education, age, etc. In table 17 in Appendix A, we show that people who have higher incomes or higher educational levels tend to have less biased beliefs about immigrants. We also observe that participants who are more worried about immigration have less biased beliefs about immigrants, which could be due to the fact that they may be better informed about immigration. Interestingly, we find that Republicans are not differentially biased about the characteristics of immigrants once we include controls.[43]

Participants who self-identify as Christian tend to be more upward biased than people who are from a different religion. Similarly, respondents from states with more unemployed immigrants tend to over-estimate the share of immigrants who are unemployed, who are incarcerated, and who cannot speak English.[44] One reason why they might over-estimate the national average is that their beliefs are shaped by immigration at the local level. Interestingly, neither the absolute share of immigrants in the state nor most of the other demographic variables appear to systematically affect the biases in beliefs about immigration.

5 Conclusion

In this paper, we show that providing information about immigration affects people's attitude towards immigrants. First, we analyze a representative cross-country survey experiment with more than 19,000 participants, and we show that people who learn the actual share of immigrants in their country are less likely to state that there are too many immigrants. This effect is very large, given that the probability of saying that there are too many immigrants is 12 percentage points (33 percent) lower for those who receive the treatment.

[43] Without controls, there is a large difference in bias between Republican and Democrats.

[44] The data on the share of immigrants and the characteristics of immigrants comes from the American Community Survey in 2014. In current work in progress, we are looking at more spatially disaggregated data on immigrants and their characteristics.

Second, we conducted an online experiment in the U.S. with 800 participants, where half of the sample received some general information about immigration, while the other half did not. We then measured people's attitude towards immigrants with some behavioral measures and some general questions about immigration. We observe that participants in the treatment group update their beliefs about immigrants, and they donate more money to a pro-immigrant charity. These effects are fairly large, and they correspond to a change of approximately 0.25 of a standard deviation. Moreover, these effects persist even four weeks after the treatment.

However, participants who receive the information treatment do not become more supportive of immigration reform. Indeed, they do not become more willing to sign a petition in favor of immigration reform, and their self-reported policy preferences remain broadly unchanged. Still, they are less likely to state that there are too many immigrants in the U.S.

Furthermore, we find that Republicans respond more strongly to the information treatment, both in terms of their views on immigrants and in terms of their policy preferences. Indeed, Republicans who receive the treatment become more likely to report having signed the petition in favor of immigration, and they become generally more supportive of immigration. Similarly, we observe that people who are initially more worried about immigration react more strongly to the information we provide them, and they update their views on immigrants and immigration more drastically than people who are less worried about immigration.

It is important to note that we find similar heterogeneous treatment effects in the cross-country survey experiment. Indeed, we observe much larger treatment effects for participants who self-identify as right-wing or as being very worried about immigration. The fact that we obtain similar results with representative samples for several countries indicates that our results are not an artifact of the composition of our online sample.

We also ran a follow-up four weeks after the main experiment, where we asked people the same set of self-reported questions on immigration as the ones they answered in the main experiment. Overall, 88 percent of the original sample completed the follow-up survey, and we observe no differential attrition between the treatment and the control arm. Importantly, we find very similar treatment effects in the follow-up. Indeed, participants who received the information treatment four weeks earlier still have a more positive opinion of immigrants, even though their policy preferences remain unchanged.

Our survey experiment has several advantages compared to the existing literature. Unlike previous studies, our experiment uses behavioral measures to assess the impact of information on people's political opinions. We suggest that we can measure people's political preferences on immigration by giving them the opportunity to donate to a pro-immigrant charity and by allowing them to sign a real online petition. We believe that the use of real online petitions and donations to NGOs with clear ideological inclinations are ways to advance the measurement of political preferences through behavioral measures.

We think that our research has important policy implications. In particular, our research suggests that the government should think about effective ways of disseminating information about immigrants in order to reduce people's biases. This in turn may be effective in increasing people's liking of current immigrant populations and in enhancing their integration.

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6 Figures and Tables

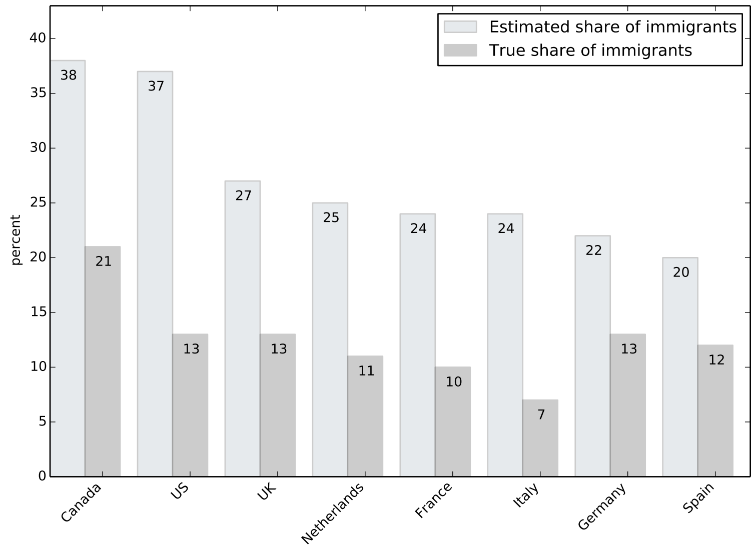

Figure 1: Biases in beliefs about the share of immigrants in different OECD countries. Source: 2010 wave of the Transatlantic Trends Survey.

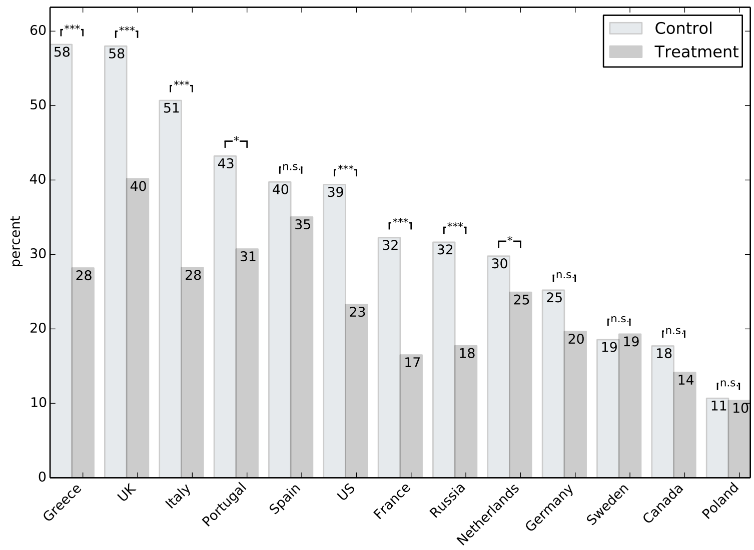

Figure 2: Cross-country evidence: the effect of information on the probability of saying that there are too many immigrants.

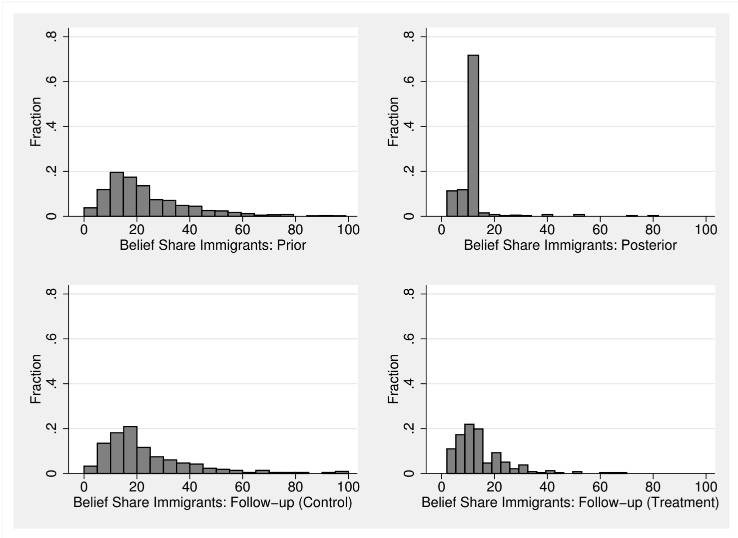

Figure 3: The panels on the top half show the distribution of beliefs about the share of immigrants in the U.S. for the treatment group, before the treatment (left panel), and after the treatment (right panel). The panels on the bottom half show the distribution of beliefs in the follow-up, in the control group (left panel), and in the treatment group (right panel).

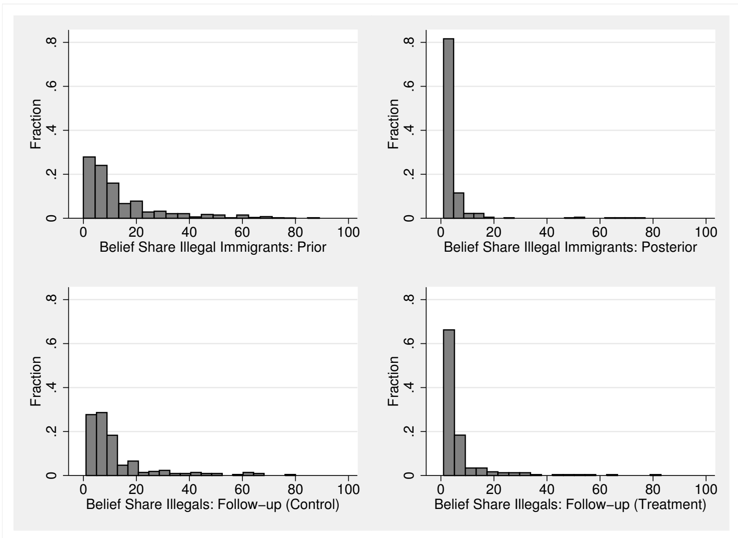

Figure 4: The panels on the top half show the distribution of beliefs about the share of illegal immigrants in the U.S. for the treatment group, before the treatment (left panel), and after the treatment (right panel). The panels on the bottom half show the distribution of beliefs in the follow-up, in the control group (left panel), and in the treatment group (right panel).

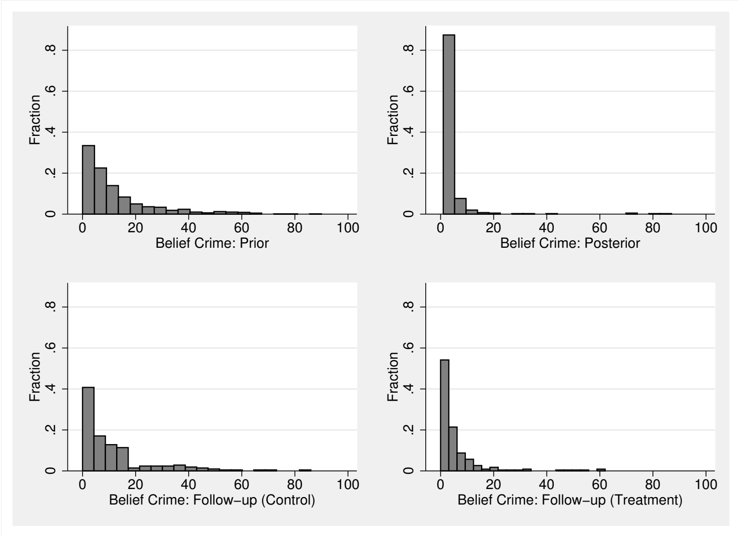

Figure 5: The panels on the top half show the distribution of beliefs about the share of incarcerated immigrants in the U.S. for the treatment group, before the treatment (left panel), and after the treatment (right panel). The panels on the bottom half show the distribution of beliefs in the follow-up, in the control group (left panel), and in the treatment group (right panel).

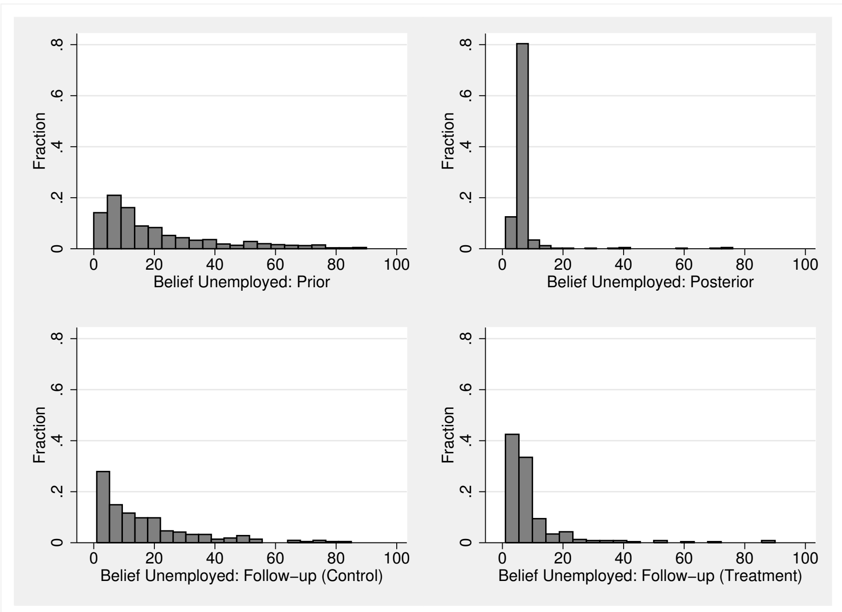

Figure 6: The panels on the top half show the distribution of beliefs about the share of unemployed immigrants in the U.S. for the treatment group, before the treatment (left panel), and after the treatment (right panel). The panels on the bottom half show the distribution of beliefs in the follow-up, in the control group (left panel), and in the treatment group (right panel).

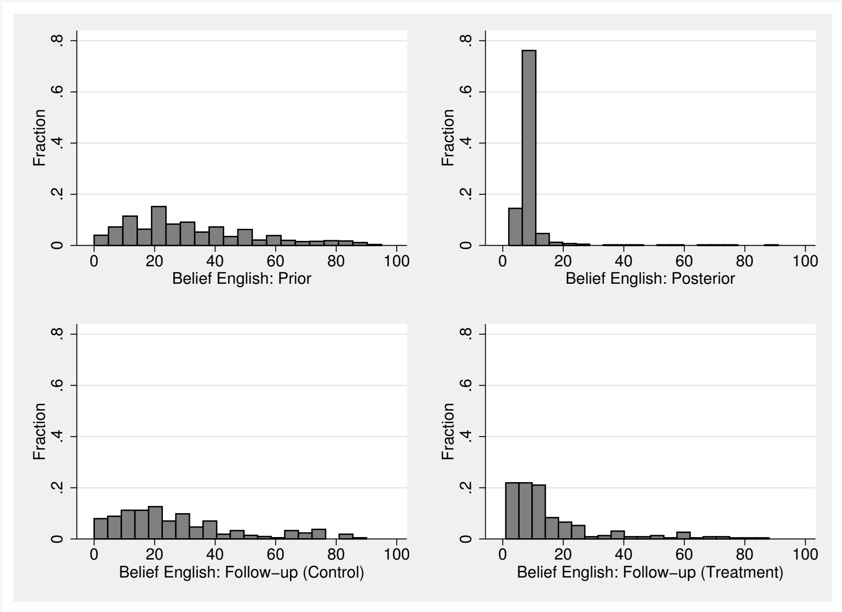

Figure 7: The panels on the top half show the distribution of beliefs about the share of immigrants who cannot speak English for the treatment group, before the treatment (left panel), and after the treatment (right panel). The panels on the bottom half show the distribution of beliefs in the follow-up, in the control group (left panel), and in the treatment group (right panel).

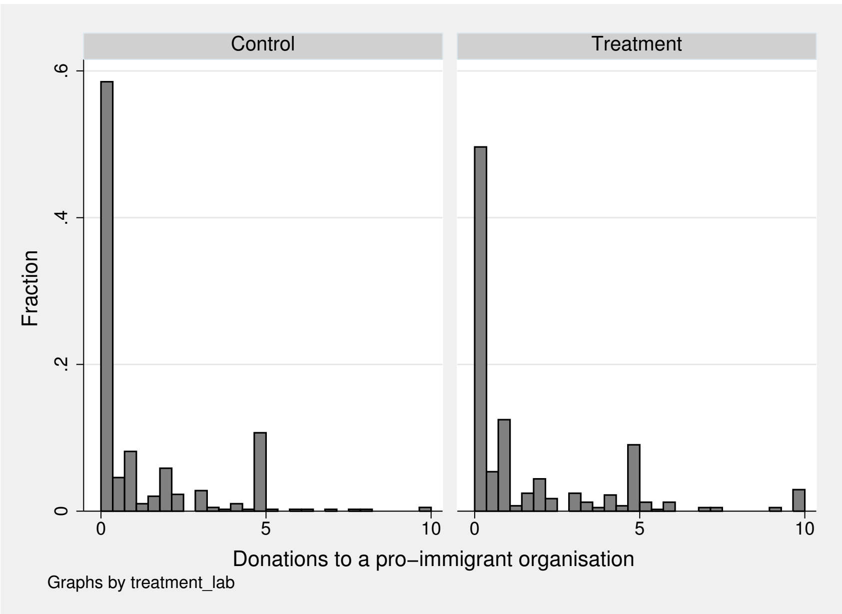

Figure 8: Distribution of donations to the "American Immigration Council" in the control group (on the left hand-side) and in the treatment group (on the right).

Table 1: Main Specification: Transatlantic Trends Survey

	Percentage of people saying yes to: "Too Many"	"Not Many"	Ordered Variable
	(1)	(2)	(3)
Panel A: Between			
Treatment	-0.1190***	0.1639***	0.2829***
	(0.0095)	(0.0093)	(0.0155)
FDR-adjusted p-values	[0.001]***	[0.001]***	[0.001]***
Control Mean	0.3651	0.1876	1.8225
N	19407	19407	19407
Scaled Effect	.55	1.5	.88

	Percentage of people saying yes to: "Too Many"	"Not Many"	Ordered Variable
	(1)	(2)	(3)
Panel B: Within			
Post-treatment	.1777	.3915	2.207
Pre-treatment	.351	.1962	1.782
Difference	-.1735***	.1952***	.4243***
	(.0076)	(.0088)	(.0128)
FDR-adjusted p-values	[0.001]***	[0.001]***	[0.001]***
N	3587	3587	3587

In Panel A, we present the results of the experiment embedded in the Transatlantic Trends survey. In Panel B, we show how participants in the control group felt about the number of immigrants before and after learning the proportion of immigrants in their country. The scaled effect is the treatment effect divided by the average difference in the answers given by participants in the control group who are right-wing and those who are left-wing. The ordered outcome variable takes value one if individuals agree that there are too many immigrants in their country, value two if individuals agree that there are a lot but not too many immigrants in their country, and value three if they agree that there are not many immigrants. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Heterogeneity by Countries

	Percentage of people saying yes to the following statement: "There are too many immigrants in this country"						
	Germany	France	Spain	Italy	UK	NL	US
Treatment	-0.0556*	-0.1575***	-0.0471	-0.2246***	-0.1782***	-0.0486**	-0.1610***
	(0.0309)	(0.0245)	(0.0323)	(0.0323)	(0.0307)	(0.0234)	(0.0352)
FDR-adjusted p-values	[0.106]	[0.001]***	[0.189]	[0.001]***	[0.001]***	[0.062]*	[0.001]***
Control Mean	0.2521	0.3225	0.3974	0.5069	0.5799	0.2978	0.3938
N	1938	1892	1919	1888	1917	1922	1858
	Portugal	Poland	Greece	Sweden	Russia	Canada	
Treatment	-0.1249**	-0.0031	-0.3003***	0.0075	-0.1392***	-0.0356	
	(0.0590)	(0.0268)	(0.0418)	(0.0338)	(0.0286)	(0.0352)	
FDR-adjusted p-values	[.062]*	[0.909]	[0.001]***	[0.893]	[0.001]***	[0.368]	
Control Mean	0.4322	0.1068	0.5821	0.1855	0.3165	0.1771	
N	908	889	965	965	1406	940	

We present the results from the 2010 and 2014 waves of the Transatlantic Trends survey for the different countries in our sample. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Heterogeneity by Beliefs about Immigrants

	Percentage of people saying yes to the following statement: "Too many immigrants"
Panel A: Concern Immigration	
Treatment	-0.1152***
	(0.0096)
Treatment × Concerned: Immigration	-0.0415
	(0.0392)
Concerned: Immigration	0.3303***
	(0.0276)
Constant	0.3401***
	(0.0073)
N	19407
Panel B: Negative View on Immigrants	
Treatment	-0.1062***
	(0.0126)
Treatment × Negative View on Immigrants	-0.0905***
	(0.0296)
Negative View on Immigrants	0.2906***
	(0.0218)
Constant	0.2836***
	(0.0098)
N	11845
Panel C: Right-wing	
Treatment	-0.0980***
	(0.0118)
Treatment × Right-wing	-0.0494**
	(0.0193)
Right-wing	0.1420***
	(0.0145)
Constant	0.3055***
	(0.0088)
N	19407

In Panel A, we look at heterogeneous treatment effects by people's concern with immigration. In Panel B, we look at heterogeneity by people's pre-treatment views on immigration. In particular, we create a dummy variable which is equal to one for individuals who say that most immigrants come to receive welfare benefits. In Panel C, we create a variable which goes from one to seven, where one denotes that the person is extremely liberal, and seven that he or she is extremely conservative. Robust standard errors are displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: MTurk: Prior and Posterior Beliefs

	(1)	(2)	(3)	(4)	(5)
			Beliefs about Immigrants		
	Share Immigrants	**Share Illegal Immigrants**	**Share Unemployed**	**Share Incarcerated**	**Can't Speak English**
Panel A					
Prior	21.94	13.88	22.25	12.66	32.62
Panel B					
Posterior	12.11	4.93	6.80	3.94	9.25
Panel C					
Difference (A - B)	9.83***	8.95***	15.45***	8.72***	23.37***
	(.76)	(.69)	(1.03)	(.692)	(1.079)
N	403	407	404	404	405
True values	13	3	6	2	8

The five outcome variables are: people's beliefs about the share of immigrants in the US, people's beliefs about the share of illegal immigrants, people's beliefs about the share of unemployed immigrants, people's beliefs about the share of incarcerated immigrants, and people's beliefs about the share of immigrants who cannot speak English. For the treatment group, participants' beliefs prior to the treatment are displayed in Panel A, while their beliefs after the treatment are displayed in Panel B. In Panel C, we show the difference between people's beliefs before and after the treatment. Robust standard errors are displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: MTurk: Persistence of Belief Changes

	(1)	(2)	(3)	(4)	(5)
			Beliefs about Immigrants		
	Share Immigrants	**Share Illegal Immigrants**	**Share Unemployed**	**Share Incarcerated**	**Can't Speak English**
Panel A					
Prior (Baseline)	21.02	13.24	21.37	12.04	31.78
Panel B					
Posterior (Baseline)	11.85	5.01	6.91	3.90	8.94
Panel C					
Follow-up	15.16	7.45	9.54	6.68	16.64
Panel D					
Difference (A - C)	5.86***	5.78***	11.83***	5.35***	15.13***
	(.795)	(.810)	(1.135)	(.771)	(1.308)
Panel E					
Difference (B - C)	3.32***	2.46***	2.51***	2.69***	7.40***
	(.638)	(.569)	(.615)	(.529)	(.904)
N	359	354	354	347	347

In these regressions, we only look at individuals from the treatment group. The five outcome variables are: people's beliefs about the share of immigrants in the US, people's beliefs about the share of illegal immigrants, people's beliefs about the share of unemployed immigrants, people's beliefs about the share of incarcerated immigrants, and people's beliefs about the share of immigrants who cannot speak English. Specifically, for the main experiment, participants' beliefs prior to the treatment are displayed in Panel A, while their beliefs after the treatment are displayed in Panel B. For the follow-up, participants' beliefs are displayed in Panel C. In Panel D, we show the difference between people's beliefs before the treatment in the main experiment and people's beliefs in the follow-up. In Panel E, we show the difference between people's beliefs after the treatment in the main experiment and people's beliefs in the follow-up. Robust standard errors are displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Main Effects: Opinion about Immigrants 1

	(1) Opinion: Crime	(2) Opinion: Unemployment	(3) Opinion: English	(4) Index
Main Experiment				
Treatment	0.113*	0.456***	0.331***	0.324***
	(0.067)	(0.067)	(0.068)	(0.053)
FDR-adjusted p-value	[.091]*	[.001]***	[.001]***	
N	805	805	804	804
With Controls				
Treatment	0.176***	0.519***	0.398***	0.387***
	(0.055)	(0.062)	(0.064)	(0.044)
FDR-adjusted p-value	[.002]***	[.001]***	[.001]***	
N	800	800	800	800
Scaled Effect	.19	1.23	.47	.54
Follow-up				
Treatment	0.082	0.289***	0.182**	0.198***
	(0.073)	(0.071)	(0.072)	(0.057)
FDR-adjusted p-value	[.227]	[.001]***	[.018]***	
N	702	702	702	702
With Controls				
Treatment	0.107*	0.289***	0.211***	0.215***
	(0.063)	(0.066)	(0.067)	(0.050)
FDR-adjusted p-value	[.084]*	[.001]***	[.005]***	
N	696	696	696	696

All outcome variables are normalized by the mean and the standard deviation of the variable for the control group (Kling et al., 2007). In other words, the coefficients represent the effect size in terms of standard deviations away from the mean. The scaled effect is the treatment effect divided by the average difference in the answers given by participants in the control group who are right-wing and those who are left-wing. In "Main Experiment", we display the results from the main experiment. In "Follow-up", we display the results from the four-week follow-up. We include the following control variables: log income, age, gender, household size, indicators for race, religion, indicators for employment status and education, the number of HITs completed, whether the respondent was born in the U.S., a question capturing people's pre-treatment worries about immigration, a dummy variable for Democrats as well as a set of prior beliefs about immigrants. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Main Effects: Opinion on Immigrants 2

	(1)	(2)	(3)	(4)
	No positive effect of Removing all illegals	**Immigrants produce more advantages**	**Index Opinions**	**Donation**
Main Experiment				
Treatment	-0.001	0.115*	0.057	0.224***
	(0.068)	(0.068)	(0.062)	(0.081)
FDR-adjusted p-value	[.982]	[.18]		
N	803	803	803	803
With Controls				
Treatment	0.046	0.176***	0.111**	0.219***
	(0.056)	(0.052)	(0.047)	(0.083)
FDR-adjusted p-value	[.496]	[.002]***		
N	800	800	800	800
Scaled Effect	.04	.17	.10	.34
Follow-up				
Treatment	0.082	0.119*	0.100	
	(0.072)	(0.070)	(0.065)	
FDR-adjusted p-value	[.244]	[.134]		
N	701	701	701	
With Controls				
Treatment	0.118*	0.139**	0.129**	
	(0.061)	(0.055)	(0.050)	
FDR-adjusted p-value	[.062]*	[.01]***		
N	695	695	695	

All outcome variables are normalized by the mean and the standard deviation of the variable for the control group (Kling et al., 2007). In other words, the coefficients represent the effect size in terms of standard deviations away from the mean. The scaled effect is the treatment effect divided by the average difference in the answers given by participants in the control group who are right-wing and those who are left-wing. In "Main Experiment", we display the results from the main experiment. In "Follow-up", we display the results from the four-week follow-up. We include the following control variables: log income, age, gender, household size, indicators for race, religion, indicators for employment status and education, the number of HITs completed, whether the respondent was born in the U.S., a question capturing people's pre-treatment worries about immigration, a dummy variable for Democrats as well as a set of prior beliefs about immigrants. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Main Effects: Policy Preferences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	There are not too many		Increase the number of		Decrease	Facilitate	Not Deport	Index
	Legal Imm	Illegal Imm	Incoming Legal Imm	Greencards	Budget to deport	Legalization	Illegals	Policy Preference
Main experiment								
Treatment	0.192***	0.190***	0.089	0.035	0.023	-0.032	-0.044	0.065
	(0.069)	(0.070)	(0.069)	(0.069)	(0.070)	(0.070)	(0.071)	(0.054)
FDR-adjusted p-value	[.025]**	[.025]***	[.46]	[.742]	[.742]	[.742]	[.742]	
N	805	805	803	803	803	803	803	803
With Controls								
Treatment	0.249***	0.270***	0.153***	0.105*	0.054	0.022	0.027	0.123***
	(0.056)	(0.052)	(0.059)	(0.059)	(0.060)	(0.060)	(0.058)	(0.038)
FDR-adjusted p-value	[.001]***	[.001]***	[.045]**	[.114]	[.692]	[.822]	[.822]	
N	800	800	800	800	800	800	800	800
Scaled Effect	.29	.26	.17	.12	.05	.01	.02	.12
Follow-up								
Treatment	0.110	0.113	0.141**	0.071	0.092	0.069	-0.020	0.082
	(0.074)	(0.075)	(0.072)	(0.073)	(0.074)	(0.075)	(0.075)	(0.059)
FDR-adjusted p-value	[.283]	[.283]	[.283]	[.44]	[.429]	[.44]	[.8]	
N	703	703	701	701	701	701	701	701
With Controls								
Treatment	0.136**	0.174***	0.183***	0.124**	0.117**	0.119*	0.019	0.121***
	(0.061)	(0.058)	(0.062)	(0.062)	(0.059)	(0.062)	(0.065)	(0.042)
FDR-adjusted p-value	[.042]**	[.018]**	[.018]*	[.102]	[.102]	[.102]	[.857]	
N	697	697	695	695	695	695	695	695

All outcome variables are normalized by the mean and the standard deviation of the variable for the control group (Kling et al., 2007). In other words, the coefficients represent the effect size in terms of standard deviations away from the mean. The scaled effect is the treatment effect divided by the average difference in the answers given by participants in the control group who are right-wing and those who are left-wing. In "Main Experiment", we display the results from the main experiment. In "Follow-up", we display the results from the four-week follow-up. We include the following control variables: log income, age, gender, household size, indicators for race, religion, indicators for employment status and education, the number of HITs completed, whether the respondent was born in the U.S., a question capturing people's pre-treatment worries about immigration, a dummy variable for Democrats as well as a set of prior beliefs about immigrants. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Main Effects: Online Petition

	(1) Intention to sign	(2) Self-report: Sign	(3) Actual Sign-up	(4) Index: Petition
Panel A				
Treatment	0.011	-0.040	-.036*	-0.029
	(0.032)	(0.027)	(.0188)	(0.055)
FDR-adjusted p-value	[.842]	[.183]	[.165]	
N	802	802	802	802
Panel B: Controls				
Treatment	0.034	-0.034	-	-0.000
	(0.032)	(0.027)	-	(0.055)
FDR-adjusted p-value	[.266]	[.266]		
N	800	800	-	800
Scaled Effect	.11	.16	-	0
Control mean	.290	.201	.101	0

All outcome variables are normalized by the mean and the standard deviation of the variable for the control group (Kling et al., 2007). In other words, the coefficients represent the effect size in terms of standard deviations away from the mean. The scaled effect is the treatment effect divided by the average difference in the answers given by participants in the control group who are right-wing and those who are left-wing. In "Main Experiment", we display the results from the main experiment. In Panel B, we include the following control variables: log income, age, gender, household size, indicators for race, religion, indicators for employment status and education, the number of HITs completed, whether the respondent was born in the U.S., a question capturing people's pre-treatment worries about immigration, a dummy variable for Democrats as well as a set of prior beliefs about immigrants. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Heterogeneous Effects

	(1)	(2)	(3)	(4)	(5)
	Donation	**Petition**	**Beliefs**	**Policy Preferences**	**Opinions**
Panel A					
Treatment	-0.951	0.186	0.165	0.063	0.077
	(0.581)	(0.207)	(0.165)	(0.144)	(0.175)
Treatment × Trust in Statistics	0.443**	-0.065	0.070	0.017	0.008
	(0.185)	(0.066)	(0.053)	(0.046)	(0.056)
Trust in Statistics	0.112	0.160***	0.067*	0.084**	0.129***
	(0.131)	(0.047)	(0.037)	(0.032)	(0.039)
N	800	800	800	800	800
Panel B					
Treatment	0.402***	-0.006	0.375***	0.108***	0.093**
	(0.154)	(0.055)	(0.043)	(0.036)	(0.045)
Treatment × Concerned about immigration	0.330	0.051	0.157**	0.149**	0.043
	(0.284)	(0.101)	(0.079)	(0.067)	(0.083)
Concerned about immigration	-0.690**	-0.229**	-0.526***	-0.654***	-0.706***
	(0.308)	(0.110)	(0.086)	(0.073)	(0.089)
N	800	800	800	800	800
Panel C					
Treatment	0.259	-0.061	0.312***	0.067	0.074
	(0.173)	(0.062)	(0.049)	(0.043)	(0.052)
Treatment × Republican	0.578	0.226*	0.306***	0.212**	0.121
	(0.356)	(0.126)	(0.101)	(0.087)	(0.107)
Republican	-1.711***	-0.686***	-0.356***	-0.539***	-0.660***
	(0.381)	(0.135)	(0.109)	(0.094)	(0.114)
N	800	800	800	800	800
P-value (Tr + Tr×Rep)	0.005	0.122	0.000	0.000	.021
Controls	Y	Y	Y	Y	Y

The definition of the indices is in Appendix B. All outcome variables are normalized by the mean and the standard deviation of the variable for the control group (Kling et al., 2007). In other words, the coefficients represent the effect size in terms of standard deviations away from the mean. All the specifications include the following control variables: log income, age, gender, household size, indicators for race, religion, indicators for employment status and education, the number of HITs completed, whether the respondent was born in the U.S., a question capturing people's pre-treatment worries about immigration, a dummy variable for Democrats as well as a set of prior beliefs about immigrants. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Heterogeneous Effects: Follow-up

	(1) **Opinions 1**	(2) **Policy Preferences**	(3) **Opinions 2**
Panel A			
Treatment	-0.200	0.170	0.329*
	(0.191)	(0.182)	(0.189)
Treatment × Trust in statistics	0.139** (0.061)	-0.018 (0.058)	-0.073 (0.060)
Trust in statistics	0.035	0.093**	0.152***
	(0.043)	(0.041)	(0.043)
N	697	696	696
Panel B			
Treatment	0.221***	0.113**	0.109**
	(0.050)	(0.046)	(0.047)
Treatment × Concerned with immigration	0.052 (0.091)	0.111 (0.084)	0.279*** (0.086)
Concerned with immigration	-0.497***	-0.700***	-0.841***
	(0.099)	(0.091)	(0.094)
N	697	696	696
Panel C			
Treatment	0.221***	0.067	0.066
	(0.058)	(0.054)	(0.057)
Treatment × Republican	0.044 (0.118)	0.257** (0.111)	0.250** (0.116)
Republican	-0.327***	-0.461***	-0.544***
	(0.089)	(0.084)	(0.088)
N	697	696	696
P-value (Tr + Tr×Rep)	0.010	0.001	0.002
Controls	Y	Y	Y

The definition of the indices is in Appendix B. All outcome variables are normalized by the mean and the standard deviation of the variable for the control group (Kling et al., 2007). In other words, the coefficients represent the effect size in terms of standard deviations away from the mean. All the specifications include the following control variables: log income, age, gender, household size, indicators for race, religion, indicators for employment status and education, the number of HITs completed, whether the respondent was born in the U.S., a question capturing people's pre-treatment worries about immigration, a dummy variable for Democrats as well as a set of prior beliefs about immigrants. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A Additional tables

Table 12: Summary statistics of the Transatlantic Trend Sample

Variable	**Mean**	**Std. Dev.**	**N**
	2010 and 2014 Wave		
Too many immigrants	0.283	0.45	20513
Concerned about immigration	0.067	0.25	20513
Male	0.457	0.498	20513
Elementary school	0.109	0.312	19382
Some secondary	0.169	0.375	19382
Secondary	0.292	0.455	19382
College	0.276	0.447	19382
Postgraduate	0.123	0.328	19382
Left-wing	0.301	0.459	20513
Right-wing	0.414	0.493	20513
Age	49.234	15.572	20314
	2010 Wave		
Financials worse	0.451	0.498	8010
Jobs available	0.922	0.269	8010
Full-time employed	0.397	0.489	8010
Part-time employed	0.141	0.348	8010

Table 13: Randomization Check: Transatlantic Trend Survey

	Treatment	**Control**	**P-value**
Concerned about immigration	0.07	0.07	0.268
Male	0.45	0.46	0.151
Elementary school	0.11	0.11	0.566
Some secondary	0.16	0.17	0.035**
Secondary	0.29	0.29	0.598
College	0.28	0.28	0.705
Postgraduate	0.13	0.12	0.026**
Financials worse	0.45	0.46	0.548
Jobs available	0.92	0.93	0.115
Full-time employed	0.39	0.41	0.188
Part-time employed	0.15	0.14	0.474
Rural	0.74	0.76	0.144
Left-wing	0.30	0.31	0.298
Right-wing	0.41	0.41	0.363
Age	49.18	48.94	0.281
P-value (joint F-test)			.1391
Observations	9733	9675	19234

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Sample Characteristics MTurk

Variable	Mean	Std. Dev.	N
Donation	1.389	2.185	802
Log income	10.56	0.791	800
Age	34.815	11.094	801
Male	0.559	0.497	801
Household size	3.584	1.393	800
Hispanic	0.036	0.187	800
Black	0.079	0.27	800
White	0.784	0.412	800
Christian	0.411	0.492	800
Full-time employed	0.574	0.495	800
Part-time employed	0.181	0.385	800
Unemployed	0.104	0.305	800
Number of hits	21034	36464	800
At least bachelor degree	0.465	0.499	800
Born in the US	0.946	0.226	800
Worried about Immigration	2.792	0.89	812
Democrat	0.567	0.496	800
Belief: Cannot Speak English	31.617	21.237	798
Belief: Unemployed	20.26	19.202	801
Belief: Share immigrants	22.59	15.882	798
Belief: Share illegal immigrants	13.789	15.086	802
Belief: Crime	12.343	13.906	799

Table 15: Balance Table: MTurk

	Treatment	**Control**	**P-value**
Participant completed the experiment	0.98	0.99	0.194
Log Income	10.56	10.56	0.879
Age	35.23	34.38	0.275
Male	0.59	0.53	0.110
Household Size	3.63	3.54	0.382
Hispanic	0.04	0.03	0.657
Black	0.06	0.09	0.103
White	0.79	0.77	0.554
Christian	0.43	0.40	0.405
Full-time Employed	0.58	0.57	0.634
Part-time Employed	0.18	0.18	0.981
Unemployed	0.08	0.13	0.015**
Number of HITs	22512	19488	0.241
At least bachelor degree	0.45	0.48	0.381
Born in the US	0.95	0.94	0.534
Worried about immigration	2.75	2.83	0.201
Belief Cannot Speak English: Prior	32.59	30.59	0.185
Belief Unemployed: Prior	22.20	18.23	0.003***
Belief Share immigrants: Prior	21.90	23.32	0.204
Belief Share illegal immigrants: Prior	13.80	13.78	0.986
Belief Crime: Prior	12.78	11.88	0.365
Democrat	0.57	0.58	0.639
P-value (Joint F-Test)			0.09*
Observations	417	395	812

In the joint F-test, we regress the treatment indicator on the complete set of covariates and then test whether the covariates are jointly different from zero. Some people did not provide an estimate for the five statistics within the time limit and some people did not respond to all questions, and there are therefore some missing values in the data. We include these observations in the regression by coding the missing values as zero and by including for each question with missing values a dummy variable which is equal to one if the participant failed to give an answer for that question. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 16: Balance Table: Follow-up Experiment

	Treatment	**Control**	**P-value**
	Treatment	Control	
Log Income	10.49	10.51	0.878
Age	35.48	34.80	0.436
Male	0.59	0.51	0.034**
Household size	3.51	3.51	0.948
Hispanic	0.04	0.03	0.606
Black	0.05	0.08	0.103
White	0.79	0.78	0.941
Christian	0.41	0.41	0.981
Full-time employed	0.58	0.57	0.848
Part-time employed	0.18	0.19	0.816
Unemployed	0.08	0.11	0.112
Number of Hits completed	21757	19451	0.388
At least bachelor	0.46	0.51	0.217
Born in the US	0.94	0.94	0.898
Worried about immigration	2.78	2.82	0.573
Belief English: Prior	31.92	29.75	0.181
Belief Unemployed: Prior	21.11	18.48	0.070*
Belief Share Immigrants: Prior	20.83	22.80	0.095*
Belief Share Illegal Immigrants: Prior	13.25	13.31	0.962
Belief Crime: Prior	12.06	11.54	0.606
Democrat	0.58	0.59	0.902
Observations	355	342	697
P-value (Joint F-Test)			0.23

In the joint F-test, we regress the treatment indicator on the complete set of covariates and then test whether the covariates are jointly different from zero. Some people did not provide an estimate for the five statistics within the time limit and some people did not respond to all questions, and there are therefore some missing values in the data. We include these observations in the regression by coding the missing values as zero and by including for each question with missing values a dummy variable which is equal to one if the participant failed to give an answer for that question. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 17: Determinants of size of the biases in beliefs

	(1)	(2)	(3)	(4)	(5)
			Biased Belief about immigrants		
	Crime	No English	Unemployed	Share Immigrants	Share Illegal Immigrants
Log Income	-2.540***	-0.530	-2.028**	-2.793***	-3.926***
	(0.711)	(0.792)	(0.823)	(0.764)	(0.541)
Age	-0.239***	-0.107	-0.101	-0.123***	-0.242***
	(0.038)	(0.080)	(0.070)	(0.039)	(0.045)
Male	-0.409	-0.965	0.930	-2.652*	-3.081***
	(0.915)	(1.634)	(1.327)	(1.387)	(0.997)
Household Size	0.736	-0.004	1.177*	1.131*	0.641**
	(0.476)	(0.575)	(0.678)	(0.647)	(0.243)
Hispanic	-4.927*	-5.794	0.221	-3.304	4.993**
	(2.742)	(5.187)	(3.393)	(3.344)	(2.480)
Black	0.137	-2.296	1.030	-3.447	2.944
	(3.586)	(3.266)	(3.028)	(2.893)	(2.719)
White	-3.466	-4.176	-3.385	-5.678***	-1.010
	(2.841)	(2.515)	(2.460)	(1.830)	(1.732)
Christian	2.717**	3.815**	2.240	4.263***	3.746***
	(1.150)	(1.715)	(1.652)	(1.363)	(1.199)
Full-time employed	2.414*	2.335	1.791	3.214*	4.924***
	(1.247)	(2.646)	(1.694)	(1.663)	(1.349)
Part-time employed	0.448	4.014	1.374	0.515	3.002*
	(1.574)	(3.250)	(2.585)	(2.405)	(1.518)
Unemployed	0.209	0.324	1.583	2.241	4.081
	(1.865)	(3.132)	(3.004)	(2.068)	(2.661)
Number of Hits	0.000	0.000	0.000	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
At least a bachelor degree	-1.743**	-1.547	-1.252	-4.026***	-3.850***
	(0.736)	(1.872)	(1.499)	(1.158)	(0.710)
Born in the US	-4.291*	2.074	0.688	-6.551	-9.381***
	(2.362)	(3.364)	(3.420)	(4.060)	(2.501)
Worried about immigration	-2.568***	-4.813***	-4.871***	-2.318**	-2.945***
	(0.624)	(0.881)	(0.754)	(0.873)	(0.616)
Republican	0.296	0.289	1.808	1.220	1.476
	(1.162)	(2.117)	(1.454)	(1.687)	(1.232)
State: share of immigrants	-4.356	-3.903	-23.942*	10.250	4.458
	(6.204)	(10.110)	(14.036)	(6.958)	(13.385)
State: share of immigrants	-2.186	-17.006	14.624	-16.021	14.576
who don't speak English	(13.044)	(16.499)	(29.635)	(14.984)	(23.852)
State: share of immigrants	222.432***	379.188***	429.271***	139.658	58.825
who are unemployed	(79.681)	(129.344)	(122.218)	(92.984)	(104.472)
N	794	794	796	790	796
R^2	0.124	0.075	0.099	0.141	0.195

Standard errors clustered at the state level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 18: Stability of Preferences over Time

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	There are not too many		Increase the number of		Decrease	Facilitate	Not Deport	Index
	Legal Imm	Illegal Imm	Incoming Legal Imm	Greencards	Budget to deport	Legalization	Illegals	Policy Preference
Correlation	0.754	0.827	0.705	0.803	0.701	0.764	0.812	0.918
N	342	342	342	342	342	342	342	342

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Beliefs about immigrants' characteristics			Index	No positive effect of	Immigrants produce	Index
	Crime	Unemployment	English	Belief	Removing all illegals	more advantages	Opinions
Correlation	0.723	0.578	0.651	0.769	0.724	0.757	0.822
N	342	342	342	342	342	342	342

This table summarizes the temporal correlation of people's attitude towards immigrants, their opinions about immigrants as well as their policy preferences regarding immigration, between the main experiment and the follow-up. In other words, we correlate the responses from our main experiment and those from the follow-up experiment four weeks later. We only consider the individuals in the control group.

Table 19: Stability of Treatment Effects over Time

	(1)	(2)	(3)
	Opinions 1	**Policy Preferences**	**Opinions 2**
A: Main Experiment			
Treatment	0.387***	0.123***	0.111**
	(0.044)	(0.038)	(0.047)
N	800	800	800
B: Follow-up experiment			
Treatment	0.246***	0.139***	0.136***
	(0.051)	(0.048)	(0.051)
N	699	698	698
P-value (Equality of A and B)	0.001***	0.584	0.652
Controls	Y	Y	Y

The definition of the indices is in Appendix B. All outcome variables are normalized by the mean and the standard deviation of the variable for the control group (Kling et al., 2007). In other words, the coefficients represent the effect size in terms of standard deviations away from the mean. All the specifications include the following control variables: log income, age, gender, household size, indicators for race, religion, indicators for employment status and education, the number of HITs completed, whether the respondent was born in the U.S., a question capturing people's pre-treatment worries about immigration, a dummy variable for Democrats as well as a set of prior beliefs about immigrants. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Families of outcomes: Construction of indices

First, we group our explicit outcome measures into different families of outcomes, and create an index for each family. We use the method described in Anderson (2008) to create the various indices.[45]

We define the families of outcomes as follows:

- **Opinions about Immigrants 1:** We compute an index of the beliefs that people have regarding immigrants.
  - Immigrants are more likely to commit crimes than U.S. citizens. [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]
  - Immigrants are more likely to be unemployed than U.S. citizens. [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]
  - Immigrants generally learn English within a reasonable amount of time. [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]
- **Policy Preferences:** We compute an index of people's policy preferences regarding immigration.
  - There are currently too many immigrants in the U.S.
  - There are currently too many illegal immigrants in the U.S. [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]
  - Do you think the number of legal immigrants coming to the United States each year should be increased, reduced or remain the same? [It should be increased a lot, It should be increased a little, It should remain as it is, it should be decreased a little, it should be decreased a lot.]
  - Do you think that the number of green cards available for immigrants coming to the United States each year should be increased, reduced or remain the same? [It should be increased a lot, It should be increased a little, It should remain as it is, it should be decreased a little, it should be decreased a lot.]

[45] We recode the variables such that high values correspond to negative attitudes towards immigrants (this is true for all outcomes except for people's willingness to donate money to a charity and their willingness to sign a positive petition). We normalize these variables, i.e. we subtract the mean and divide them by the standard deviation of each of the outcome variables. Then, we calculate the covariances between the variables part of the same family of outcomes and use the inverse of the covariance matrix in order to weight the outcomes. For more details see Anderson (2008).

- The government should devote a larger share of its budget to find illegal immigrants, and to deport them. [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]
- Congress should pass a bill to give some illegal immigrants living in the U.S. a path to legal status. [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]
- Which comes closer to your view about what government policy should be toward illegal immigrants currently residing in the United States? [deport all illegal immigrants back to their home country. allow illegal immigrants to remain in the United States in order to work, but only for a limited amount of time. allow illegal immigrants to remain in the United States and become U.S. citizens, but only if they meet certain requirements over a period of time.]

- **Opinion on Immigrants 2:** We compute an index of people's opinion on immigrants.
  - Suppose U.S. authorities were able to remove almost all illegal immigrants from the U.S. What effect do you think this would have on the U.S. economy? [Very positive effect, Somewhat positive effect, Neither positive nor negative effect, Somewhat negative effect, Very negative effect]
  - Over the last 10 years, immigrants have produced more disadvantages than advantages for the U.S. as a whole. [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]

- **Petition:** We compute an index of people's willingness to sign a petition:
  - **Intention to sign:** This variable takes value one for individuals saying that they want to sign the petition.
  - **Self-reported signing:** This variable takes value one for individuals saying that they did sign the petition.