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Does Information Change Attitudes Towards Immigrants?*

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Abstract

Strategies aimed at reducing negative attitudes towards immigrants are at the core of integration policies. A large literature shows that misperceptions about the size and characteristics of immigrants are common. A few papers have implemented interventions to correct innumeracy regarding the size of the immigrant population, but did not detect any effects on attitudes. We study whether providing information not only about the size, but also about the characteristics of the immigrant population can have stronger effects. We conduct two online experiments with samples from the US, where we provide half of the participants with five statistics about immigration. This information bundle improves people's attitudes towards current legal immigrants. Most effects are driven by Republicans and other groups with more negative initial attitudes towards immigrants. In our second experiment, we show that treatment effects persist one month later. Finally, we analyze a large cross-country survey experiment to provide external validity to the finding that information about the size of the foreign-born population is not enough to change policy views. We conclude that people with negative views on immigration before the intervention can become more supportive of immigration if their misperceptions about the characteristics of the foreign-born population are corrected.

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

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Motivation

A large proportion of the population of the United States and Western Europe views immigration as one of the most pressing issues facing their country. For instance, more than three quarters of British citizens want to reduce immigration (Blinder 2015), while more than forty percent of Americans are dissatisfied with the level of immigration in the US (Gallup 2016). Political parties and politicians who have tapped into these concerns have gained support in the last few years, such as the Front National in France, the Northern League in Italy or Donald Trump in the United States.

Even though immigration is a central issue in many national elections, voters remain highly misinformed both about the size (Citrin and Sides 2008; Herda 2010; IpsosMori 2014) and about the characteristics of the immigrant population (Blinder 2015; Herda 2015, 2018).¹ Indeed, there is evidence that these misperceptions have been recently growing in the United States, driven by politically conservative Americans (Herda 2019b).

Both the perceived size and characteristics of the immigrant group could be a source of collective threat for natives, which may contribute to the formation of negative attitudes towards immigrants (Quillian 1995). Misperceptions about these attributes can thus exacerbate prejudice and hamper the integration of immigrants into society. While beliefs and attitudes towards immigrants only represent one of the many forces driving assimilation dynamics, their importance cannot be disregarded (Drouhot and Nee 2019).

Our paper studies the effects of an information package that corrects people's beliefs about both the proportion and the characteristics of immigrants in the United States. We test whether this information treatment can change people's beliefs about immigrants and their policy preferences regarding immigration. Our findings indicate that this infor-

¹Throughout the text, we define immigrants as people living in the country, who were not born in that country.

mation package is effective at correcting misperceptions, and it affects the immigration policy preferences of Republicans and of those who generally opposed immigration before the intervention. We present evidence that information merely about the size of the immigrant group is not enough to generate significant effects, and we conclude that correcting misperceptions about the characteristics of immigrants is a more promising intervention.

Related Literature

A large literature in economics, political science and sociology shows that people tend to over-estimate the size of minority groups (Alba, Rumbaut and Marotz 2005; Gallagher 2003; Herda 2010; Kunovich 2017; Laméris et al. 2018; Lawrence and Sides 2014; Nadeau and Niemi 1995; Nadeau, Niemi and Levine 1993; Sides and Citrin 2007). Theories of inter-group threat posit that people feel more threatened when they perceive a larger size of the minority group (Blalock 1967; Blumer 1958; Bobo and Hutchings 1996; Quillian 1995), and several empirical papers discuss whether size misperceptions could be correlated with negative attitudes towards minorities (Citrin and Sides 2008; Hjern 2007; Hooghe and De Vroome 2015; Laméris, Hipp and Tolsma 2018; Semyonov et al. 2004).

Based on this argument, we would expect that an intervention correcting people's misperceptions about the size of the immigrant group will improve attitudes towards immigrants. However, this hypothesis has been empirically tested by Sides and Citrin (2007) and Hopkins, Sides and Citrin (2019), and they have found that correcting misperceptions about the size of the immigrant group has only limited effects on people's attitudes towards immigrants.

Theories of social identification and social identity (Stets and Burke 2000) suggest that

what matters for the formation of preferences about an out-group is not the perceived size of the group, but the perceived characteristics of that group. This intuition can lead to an extension of inter-group threat theories, where the feeling of threat derives not only from the size, but also from the perceived characteristics of the minority group. The argument is supported by recent studies pointing out that group size is not the only or even the most relevant source of misperception regarding minorities, and that there is a high degree of innumeracy for other dimensions related to the characteristics of the immigrant group (Alesina, Miano and Stantcheva 2018; Blinder 2015; Herda 2015, 2018). Therefore, correcting misperceptions about the characteristics of immigrants may be a more effective way to generate a change in attitudes.

Our paper contributes to this literature by studying the effects of a comprehensive information package that corrects people's beliefs about the proportion of immigrants and their characteristics. To select the relevant characteristics of immigrants that we include in our information package, we rely on work in political science (Bansak, Hainmueller and Hangartner 2016; Blinder 2015; Hainmueller and Hopkins 2014) and sociology (Flores and Schachter 2018; Schachter 2016). Blinder (2015) investigates who people have in mind when they think of immigrants, while Hainmueller and Hopkins (2014), Flores and Schachter (2018) and Schachter (2016) employ conjoint experiments and randomize profiles of hypothetical individuals with different characteristics in order to measure support for each particular attribute. The evidence indicates that non-Hispanic white Americans prefer immigrants who are not unemployed, who speak English and who are not undocumented.² These empirical findings are supported by theoretical work indicating that

²A large literature has studied the determinants of people's attitudes towards immigrants (Alba, Rumbaut and Marotz 2005; Hainmueller, Hiscox and Margalit 2015; Scheve and Slaughter 2001). Previous studies have focused on characteristics such as age, media exposure, competition in the labor market, exposure to immigrants, education or income that are correlated with people's attitudes towards immigrants (Card, Dustmann and Preston 2012; Citrin et al. 1997; Haaland and Roth 2018; Mayda 2006;

the native-born population subjectively picks a number of characteristics to define who is an insider and who is not. This subjective sense of similarity, or *symbolic belonging* (Schachter 2016), determines natives’ conception of the “deserving” immigrant.³

Main Hypotheses

Our intervention aims to correct misperceptions about the size and the characteristics of the immigrant population. Our first hypothesis is that these misperceptions exist and that we can correct them by providing credible information. This is the first step in our theory of change, which we test in two ways. First, we study whether our experimental group updates their beliefs about the statistics we provide in the short term, and whether this update in beliefs persists one month later. Second, we estimate the treatment effects on beliefs about the size of the immigrant group and characteristics of immigrants that are directly linked to the information provided.

Our second hypothesis is that our intervention can also change beliefs about more general characteristics of immigrants that are not directly linked to the information provided. We hypothesize that people can develop more positive beliefs about immigrants if the information provided in the treatment makes them realize that immigrants living in the country are similar to the “deserving” immigrant category they have formed in their minds. This would be in line with the work on *symbolic belonging*.

Our third hypothesis is that our intervention might also affect immigration policy

Mayda and Facchini 2009). Other papers have included the real or the perceived size of the immigrant group as a key correlate (Gallagher 2003; Hjerm 2007; Hooghe and De Vroome 2015; Semyonov et al. 2004).

³It is important to note that while our results are in line with the notion of a “deserving” immigrant category, our interventions do nothing to encourage moralizing classifications or to advocate support for only one category of immigrants. Andrews (2018) studies how the combination of expanded immigration enforcement and good/bad moralizing classifications can affect undocumented immigrants. Menjívar and Lakhani (2016) show how the process of applying for legal status can trigger enduring changes by which immigrants try to behave according to the “deserving” immigrant profile.

preferences. This third step in the theory of change will be observed if the change in people’s beliefs translates into a change in preferences regarding policy. In this case, we hypothesize that there could be important heterogeneity in results by political affiliation. On the one hand, there could be a ceiling effect given that Democrats in the US have more positive views regarding immigrants to begin with, and there is less room to change their policy preferences. On the other hand, the literature on motivated reasoning (Taber and Lodge 2006) posits that people who receive information which goes against their political convictions might be less willing to update their beliefs than people for whom the information is in line with their political orientation, which would indicate that Republicans will actually react less to positive information about immigrants.⁴

We expect that, if there is a change in policy preferences, this will mainly happen for policies regarding legal immigrants, and not for those regarding undocumented immigrants. Our hypothesis is that our experimental treatment makes people realize that immigrants are more similar to themselves than they originally thought. This will hold for a general immigrant category, and would not apply directly to the particular subgroup of undocumented immigrants, which according to the evidence cited above, is not considered part of the “deserving” immigrant category.

In the main analysis, we focus on three families of outcomes that allow us to test these hypotheses: people’s beliefs regarding the variables directly targeted by the intervention, their general beliefs about immigrants, and their policy preferences.⁵ We complement these families of outcomes with two behavioral measures: donations to a pro-immigrant charity, and willingness to sign a petition in favor of increasing the number of green cards.

⁴An influential paper has documented the existence of backfire effects (Nyhan and Reifler 2010), where people’s beliefs actually get reinforced in the face of contradictory evidence. However, recent evidence indicates that these type of backfire effects might not be so common (Guess and Coppock 2019; Wood and Porter 2019).

⁵A precise definition of the different families can be found in the Online Appendix.

The use of real online petitions and donations is novel in the literature and can be widely applied by researchers to examine people’s support for various policy proposals.

We pre-registered the experimental design, our hypotheses and our empirical specifications on the Social Science Registry before running our two online experiments. Almost all of the analyses presented in this paper were pre-specified. We explicitly mention in the paper which analyses were not part of the pre-analysis plan, which is available online.

We conduct two experiments with identical design to test our hypotheses: Experiment 1 uses an online sample from the US, which matches the US population in terms of age, gender and region of residence. Experiment 2 uses a voluntary response sample recruited on Amazon Mechanical Turk (MTurk), and includes the follow-up survey conducted four weeks after the main experiment that we use to measure persistence in beliefs.⁶

Experiment 1: TNS Global

Sample We conducted Experiment 1 using a sample of the US population, which was provided by TNS Global, a world-leading company in market research and political surveys. This sample of 1,193 people living in the United States was obtained as a non-probability quota sample to match the US population in terms of age, gender and region of residence.⁷ All of the participants completed the survey online, using a link which was provided by TNS Global.⁸

⁶Previous literature has cast doubt on whether interventions can have persistent effects on beliefs. For example, Flores (2018) finds that the effect of anti-immigrant rhetoric by political elites does not persist more than two weeks and attributes them to social desirability bias. However, Herda shows that a classroom activity can correct misperceptions among students with effects that persist five weeks later; although he does not examine whether the correction generates changes in attitudes or policy preferences (Herda 2017, 2019a).

⁷The sample is not drawn from a probability-based sample, which means that it is not representative in terms of variables not targeted by the quota.

⁸TNS provided us with 1,193 observations rather than 1,000 as we had specified in the pre-analysis plan because they made an error in a count variable, which meant that they underestimated the number of observations, and therefore accidentally provided us with a larger sample.

To participate in the experiment, people had to pass a standard attention screener at the start of the survey (Berinsky, Margolis and Sances 2014).⁹ The experiment was run at the beginning of September 2016. The characteristics of the whole sample are described in Table 1. Overall, 49 percent of participants are male, and the median age in our sample is 39, which is very close to the national average of 38. Similarly, 81 percent of our participants identify as white, while the proportion identifying as white in the US is around 77.5 percent.¹⁰ The median household income in the TNS sample is \$65,000, compared to \$56,516 for the national estimate. Finally, 66 percent of the TNS sample report being employed either part-time or full-time, which is close to the employment-population ratio for the US (60 percent). Participants in the TNS sample are more educated than the average American which is very common in online samples.

The randomization worked as expected and our samples are balanced across the treatment and control group as can be seen in Table 1.

Design

Pre-Treatment Characteristics and Prior Beliefs 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 statistics about immigration: the proportion of immigrants in the US, the proportion of undocumented immigrants in the US, the unemployment rate of immigrants, their incarceration rate,

⁹The attrition rate was very low (smaller than 2%). We find no evidence of differential attrition across treatment arms.

¹⁰Our survey question includes mutually exclusive options for White, Hispanic, Black, Asian, or other ethnicity. Therefore, our White category includes those who identify as whites, and we cannot distinguish between Hispanic and Non-Hispanic Whites.

and the proportion of immigrants who cannot speak English.¹¹ To help participants give plausible estimates for the unemployment rate and the incarceration rate of immigrants, we tell them what these rates are for US-born citizens.¹²

Information Treatment Then, only the treatment group is told the correct answers to these five questions. 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:¹³

“You estimated that X percent of immigrants are unemployed. According to the American Community Survey, around 6 percent of immigrants are unemployed.”

Post-Treatment Beliefs We then ask all participants a series of questions on their perception of legal and undocumented immigrants. We first measure people’s agreement to the following three statements that are directly linked to the information provided on the characteristics of immigrants: (i) Immigrants are more likely to commit crimes than U.S. citizens; (ii) Immigrants are more likely to be unemployed than U.S. citizens; and (iii) Immigrants generally learn English within a reasonable amount of time. We then focus on beliefs regarding the other two variables targeted by the intervention: views about the number of legal and undocumented immigrants. We ask participants to choose their agreement with the following statements: (i) There are currently too many

¹¹We chose these statistics for two main reasons. First, as we described in the related literature section, there is evidence showing that people are particularly concerned about these issues. People prefer immigrants who are not unemployed, who speak English and who are not undocumented. Second, there exists Census data on these issues, which increases the reliability of the information we provide.

¹²This comparison reduces the concern that participants might be able to translate their perception into numerical terms when asked about absolute shares (Alba, Rumbaut and Marotz 2005). Both the treatment and the control group receive this information, and the internal validity of our study is therefore not compromised.

¹³To make the treatment more salient, we also present the feedback using bar charts, where we show participants their estimate and the correct one.

immigrants in the US; (ii) There are currently too many illegal immigrants in the US. We also specifically measure people’s more general beliefs regarding immigrants. We asked them about the effects of removing undocumented immigrants from the US and whether “over the last 10 years, immigrants have produced more disadvantages than advantages for the U.S. as a whole.”

Policy Views We then obtain participants’ views on specific immigration policies that are at the core of the policy debate in the United States. First, we measure whether people think that the number of legal immigrants coming to the United States each year should be increased, reduced or remain the same, and whether they 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.

On top of these questions on legal immigration, we also measure people’s views regarding unauthorized immigration. We measure people’s agreement to the statement that “the government should devote a larger share of its budget to find illegal immigrants, and to deport them” and to the statement that “Congress should pass a bill to give some illegal immigrants living in the U.S. a path to legal status”. Finally, we measure people’s views on whether the government should “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, or 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.”

Behavioral Measures We obtain two behavioral measures, introduced in a random order. First, we give participants the option of signing an online petition in favor of

facilitating legal immigration into the US, by increasing the number of green cards available for immigrants. We created two identical petitions on the White House website, and we gave different links to participants in the treatment and control groups.¹⁴ This is a credible measure of people’s support for immigration, as it requires some effort to sign the petition (people need to create an online profile and to sign with their initials). Furthermore, this behavioral measure involves a real petition with potentially concrete consequences, which attenuates concerns about its external validity.

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” (American Immigration Council 2016), in case they receive the ten dollars. Since people need to forgo some of their own money in order to support the pro-immigrant NGO, this behavioral measure may be deemed more credible than self-reported measures as a valid indicator of participant’s preferences (Bonica 2019).

After the behavioral measures, participants complete a standard attention check, whose purpose is to assess how attentive participants were in the experiment. Then, we ask participants in the treatment group to estimate again the same five statistics for which we had elicited priors, so that we can test if they update their beliefs and how well they remember the information. Finally, respondents complete a questionnaire on demographics including variables such as gender, age, education and income.

¹⁴Only participants with a link can see the petition until at least 150 people sign it, after which it becomes public. Moreover, if the petition reaches 100,000 signatures in 30 days, it is entitled to get an official reply from the White House.

Main Results: Experiment 1

Next, we explore the effects of information treatment by comparing the behavior of people in the treatment group with that of people in the control group, estimating the following equation:¹⁵

$$y_i = \pi_0 + \pi_1 Treatment_i + \Pi^T \mathbf{X}_i + \varepsilon_i$$

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 more positive attitudes towards immigrants. We present all results controlling for the covariates X_i , which we pre-specified for the balance test.¹⁶

We account for multiple hypothesis testing by adjusting the p-values using the “sharpened q-value approach”.¹⁷ For each table, we also create an index of the outcomes, which we regress on the treatment indicator.

Changes in Beliefs about Characteristics Targeted by the Intervention In this section, we show that participants in the treatment group strongly update their beliefs about the characteristics of immigrants targeted by the intervention, which is in line with our first hypothesis.

In Figure 1, we show the average estimates that treated participants gave before and after receiving the correct information. It is clear that, before the treatment, participants had biased beliefs about immigration. Their estimates were on average consistently

¹⁵Robust standard errors are used throughout the analysis.

¹⁶Among pre-specified covariates, we include measures of prior beliefs, which are missing for less than 2 percent of our respondents. We impute their values using the set of pre-specified controls displayed in the balance table. In an earlier working paper version of the paper (Grigorieff, Roth and Ubfal 2016), we show that results remain very similar when we do not include the covariates X_i in the regression.

¹⁷For each family of outcomes, we control for a false discovery rate of 5 percent (Anderson 2008).

higher than the actual values.¹⁸ For example, people over-estimated the percentage of immigrants in the US by more than 20 percentage points.

Similarly, Panel A of Table 2 shows that, compared to control group respondents, treated respondents are less likely to report that immigrants commit more crimes than US citizens, that they take too much time to learn English, and that their unemployment rate is higher than that of natives. All of these results are statistically significant; the effect sizes are large and correspond to more than half of the gap between Democrats and Republicans.¹⁹ Furthermore, treated participants are less likely to state that there are too many legal and undocumented immigrants in the US. These effects are statistically significant; their effect size is 0.10 and 0.24 standard deviations for legal and undocumented immigrants, respectively.

Changes in General Beliefs about Immigrants In line with our second hypothesis, the information treatment also has an effect on how people perceive immigration generally, as shown in Table 3. People in the treatment group are less likely to say that immigrants have produced more disadvantages than advantages for the US as a whole over the last ten years. This result is significant at the one percent level, and the effect size is 0.14 standard deviations. While treated respondents are also more likely to say that there would be no positive effects from removing undocumented immigrants, the coefficient (0.09 standard deviations) is only marginally significant.²⁰

¹⁸In Grigorieff, Roth and Ubfal (2016), we show that more educated people, males and people who live in zipcode areas with a small share of immigrants tend to have less biased beliefs about the share and characteristics of immigrants. These findings are consistent with previous research on the determinants of innumeracy in the US (Alba, Rumbaut and Marotz 2005; Laméris et al. 2018; Nadeau, Niemi and Levine 1993) and Europe (Herda 2010).

¹⁹On average, Republicans have a significantly more negative view of immigrants than Democrats for all our outcomes. This is in line with evidence that immigration enforcement is higher in states with a larger share of Republican constituents (Moinester 2018).

²⁰We asked respondents some additional questions on the respective contributions of legal and undocumented immigrants, for which we find consistent effects. These estimates are reported in Grigorieff, Roth and Ubfal (2016).

Policy Preferences We next examine whether the information provision also affects people’s views about immigration policy, the last step in our theory of change.

In Table 4, we observe that treated respondents become more likely to be in favor of increasing the number of legal immigrants (0.13 standard deviations). However, we see no effects on their views about the number of green cards to issue every year, or on the legalization of immigrants. Similarly, participants’ views on deporting undocumented immigrants and on the budget that should be devoted to it are not significantly affected by the treatment. Overall, we find that the index of policy preferences is not affected for the average participant in our experiment.

Petition and Donations In Table 5, we show that there is no treatment effect on the probability to sign the online petition on the White House’s website in favor of increasing the number of green cards available for immigrants.²¹ Similarly, approximately the same fraction of people in the treatment and control group reported both intending to sign and having signed the petition.²²

Finally, we also find no statistically significant effects on people’s willingness to donate to a pro-immigration charity, the American Immigration Council. The effect is 0.07 standard deviations, with a 95 percent confidence interval that includes 0 (the confidence interval includes effects between -0.04 and 0.18).

The lack of significant effects on the two behavioral outcomes for the average respondent are in line with the lack of effects on policy preferences reported above.

²¹About 10 percent of our sample actually ended up signing the petition. This means that we had sufficient variation to detect treatment effects.

²²The number of people who reported having signed the petition (25%) is higher than the number of signatures, which can partly be explained by the fact that signing the petition was a multi-stage process. People who signed the petition received a confirmation email containing 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. People’s intention to sign the petition and their self-reported signature are strongly correlated with their self-reported support for increasing the number of green cards for immigrants.

Summary Overall, our first experiment shows that when people are provided with information about the size and the characteristics of immigrants, they update their beliefs regarding the characteristics directly targeted by the intervention and more general beliefs regarding legal immigrants. However, there are no significant changes in policy preferences.

Experiment 2: MTurk Panel with Follow-up Survey

Sample

We replicated our first experiment on Amazon Mechanical Turk (MTurk), an online labor marketplace developed by Amazon.com, which is commonly used by academics to recruit participants for online experiments. The pool of workers on MTurk is a voluntary response sample, but still more representative of the US population than student samples typically used in laboratory experiments. 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. There is some concern about the MTurk sample due to the rising prevalence of bots, and MTurkers' high levels of experience. However, our findings that the MTurk sample and the sample from TNS global yield very similar results, reassures us of the data quality and alleviates concerns about the peculiarities of each of the samples.

The experiment was run in March 2016. In total, 802 participants completed it. Less than 10 people dropped out after the treatment, which means that the attrition rate was less than two percent. Table 1 summarizes the characteristics of the sample. Overall, 55 percent of participants are male. The median age in our sample is 35, while the

median age in the US is 38. Moreover, the median income in our sample is \$45,000, compared to \$56,516 for the general population. Similarly, 78 percent of our participants identify as white, while the proportion identifying as white in the US is 77.5 percent. The proportion of unemployed people in our sample (8%) is slightly higher than in the general population (5%), and the proportion of employed people (76%) is also larger than in the general population (60%). Participants in the MTurk sample are younger, more likely to be in the labor force (since both the share employed and unemployed are larger than in the general population) and more likely to be Democrat.

Four weeks after our main experiment, we re-invited everyone who had completed the main experiment for a follow-up survey. The proportion of participants who completed both the main experiment and the follow-up is 88 percent. This high re-contact rate indicates that it is possible to construct panels on MTurk with relatively low attrition. The recontact rates are very similar for treatment and control groups, and statistically indistinguishable (p -value = 0.708). The randomization worked and our samples are balanced across treatment and control groups for both the sample in the main experiment and the sample that completed the follow-up (Table 1).

Design

The design of Experiment 2 is almost identical to that of Experiment 1. There are only a few differences that we discuss below.

Incentives and Attention Check In Experiment 2, we incentivize the pre-treatment questions about immigrant characteristics. Participants receive 10 cents for each question (this is 8 percent of the participation fee) if their estimate is within three percentage points of the official value, which we obtained from the American Community Survey.

Moreover, to avoid having participants look up the answers online, we only give them 25 seconds to answer each question. We do not include an attention check in Experiment 2. However, we find that response times are similar to those of Experiment 1, indicating that respondents were not less attentive in this experiment.²³

Follow-Up Study The key innovation in Experiment 2 is that we conduct a follow-up study four weeks after the main experiment. This allows us to examine whether the treatment effects persist over time. We ask people the same set of self-reported questions on immigration as the ones they answered in the main experiment, and we also ask them to estimate the same five statistics about immigration.²⁴ This allows us to see whether people in the treatment group remember the information provided.²⁵

Results: Experiment 2

Main Survey For the main survey outcomes, we find fairly similar results to Experiment 1. We formally test the equality of treatment effects across the two samples, and only find a few cases where we reject the equality of coefficients. The main differences compared to the findings from Experiment 1 are as follows: First, we find a larger and statistically significant effect of the treatment on donations (although the confidence interval overlaps with that of Experiment 1). MTurkers in the treatment group donated on average 36 percent more (\$0.42 more) to the American Immigration Council than MTurkers in the control group. As shown in Column 5 of Table 5, this represents an effect of 0.22 standard deviations. Second, we find a stronger effect on beliefs about

²³Another piece of evidence indicating that MTurk data are of high quality is a very high correlation (of around 0.8) between responses in the follow-up and in the main survey among control group participants.

²⁴We provide the same incentives for each correct answer as in the main experiment.

²⁵We randomize the order of the sections in the survey. Half of the sample estimate the five statistics first, and then answer the set of self-reported questions on immigration, while the other half answer the self-reported questions first. We find no significant order effects.

characteristics directly targeted by the intervention, as illustrated in Table 2.

Follow-up Study We leverage the follow-up study to shed light on the persistence of the effects of the information provision on beliefs and policy views. We first test the extent to which MTurkers in the treatment group remember the information four weeks after the main experiment. In Figure 2, we show that estimates are still fairly accurate four weeks after the treatment. For instance, the average estimate of the proportion of immigrants is 15 percent in the follow-up, whereas the true value is 13 percent.²⁶ Moreover, we find that those respondents who were the most biased are the ones who update their beliefs the most, even in the follow-up. We observe a clear linear positive relationship between the revision of beliefs (the difference between priors in the main experiment and posteriors measured one month later) and the size of the initial bias in the treatment group.²⁷

We also show in Panel C of Table 2 that the effects on qualitative questions measuring beliefs about immigrants targeted by the intervention persist four weeks after the treatment, that they are statistically significant, and that they remain fairly large (about 0.2 of a standard deviation). We see slightly larger treatment effects on policy preferences (mostly around 0.1 of a standard deviation) in the follow-up. However, they are not statistically different from those in the main experiment.

Summary Our findings in Experiment 2 confirm, with a different sample, what we found in Experiment 1. More interestingly, the follow-up experiment shows that the average effects of the information package on beliefs about immigrants are not ephemeral.

We now explore whether the null average effects on policy preferences are masking im-

²⁶People in the control group do not update their beliefs in the follow-up, indicating that they did not make the effort to look up the information we provided to the treatment group.

²⁷It is worth noting that our measures of beliefs and attitudes towards immigrants are strongly correlated with people's self-reported policy preferences regarding immigration. These results were not pre-specified and are available upon request.

portant heterogeneity across different groups of participants.

Heterogeneous Treatment Effects: Experiments 1 and 2

In this section we study heterogeneity in treatment effects across different subgroups. We focus on the indices for our main five families of outcomes. Since the requirements in terms of statistical power are higher, and given that results are similar for our two experiments, we pool both samples and present effects for the pooled sample.²⁸ We estimate the following equation, where $interaction_i$ refers to the pre-specified group of interest, X_i is a vector of pre-determined characteristics; π_1 captures the magnitude of the heterogeneity in treatment effects, π_2 measures the effect for the omitted group, and $\pi_1 + \pi_2$ gives the treatment effect for the studied group:

$$y_i = \pi_0 + \pi_1 Treatment_i \times interaction_i + \pi_2 Treatment_i + \pi_3 interaction_i + \Pi^T \mathbf{X}_i + \varepsilon_i$$

Political Affiliation In Panel A of Table 6, we show that people who self-identify as Republican or people who neither identify as Democrat nor as Republican respond more strongly to the information treatment than people who identify as Democrat.²⁹ At the bottom of the table, we report the p-value from the test for the null hypothesis that there is no treatment effect for Republicans. We reject the null hypothesis at the five percent level in all cases: Republicans exhibit statistically significant improvements in all our main families of outcomes. The heterogeneity result can be partly explained by the fact that Republicans have more negative values for all outcomes to begin with, which

²⁸Table A1 presents disaggregated results.

²⁹Republicans represent 28% of the pooled sample (32% of the TNS sample and 23% of the MTurk sample), while the share of Democrats is 45% in the TNS sample and 58% in the MTurk sample. Both the share and observable characteristics of Republicans and Democrats in treatment and control groups are well balanced.

implies that the information treatment is actually stronger for them.³⁰

Initial Attitudes towards Immigrants In Panel B of Table 6, we show that participants from the TNS and MTurk samples who are particularly worried about immigration tend to respond more strongly to the treatment. While not all interaction coefficients are statistically significant in this case, we see a consistent pattern of larger effects.

Overall, we do not find evidence in favor of motivated reasoning theories or self-confirmation bias: Republicans and participants who initially have more negative views on immigrants update their beliefs and policy preferences more than people who have more positive attitudes towards immigrants.

Other Sources of Heterogeneity In Panel C of Table 6, we examine whether participants who have a high level of trust in official statistics respond more strongly to information. Overall, we find no consistent evidence in this direction.³¹

Persistence of Heterogeneous Effects We find a consistent pattern of heterogeneous treatment effects in the follow-up (See Panel C of Table A1).³² Even four weeks after the treatment, the effects are stronger for Republicans, especially regarding their policy preferences.

³⁰In Grigorieff, Roth and Ubfal (2016), we employ a machine learning algorithm to identify the most significant sources of heterogeneous treatment effects (Athey and Imbens 2016). The algorithm confirms that political affiliation is the factor that most strongly predicts heterogeneous responses to the treatment.

³¹In additional pre-specified analysis, we examine heterogeneous treatment effects by participants' biases in beliefs using three different definitions of biases. We find that people with larger biases in beliefs seem to respond more strongly to information. However, this effect is not statistically significant for most families of outcomes, which could be due to measurement error as we do not know how people weigh the biases for the five different statistics we measure. Results are available upon request.

³²Our results are unlikely to be driven by experimenter demand effects for two reasons: first, demand effects have been shown to be quantitatively small (de Quidt, Haushofer and Roth 2018; Mummolo and Peterson 2019) and, second, both the heterogeneity of treatment effects and the persistence of effects over time suggest that demand effects are unlikely to be causing the patterns in our data.

Experiment 3: Cross-Country Experiment

We have provided evidence that a package including information on both the size of the immigrant group and on the characteristics of immigrants can affect beliefs for the average respondent and also policy preferences for those with more negative views on immigration. We use a third experiment to show that information on size alone is not enough to generate significant effects. Consistent with the previous literature, we find evidence from the US in this direction, and we provide external validity for these results by showing similar effects for other countries where the same experiment was conducted.

Description of the Dataset We use data from the Transatlantic Trends Survey (TATS), which is a large representative survey on political attitudes conducted every year in the US and in many other countries around the world. In particular, we focus on two waves of the survey, the 2010 and 2014 waves, which included an experiment on the effect of information about the size of the immigrant group.³³

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. The 2014 wave added Greece, Portugal, Sweden, Russia and Poland, but it did not include Canada. In most countries, participants were randomly drawn from the adult population who had access to a landline or a mobile phone.³⁴ Importantly, more than 94 percent of

³³The experiment was designed by the German Marshall Fund of the United States, and the main results were graphically reported in a non-technical way in Wunderlich et al. (2010) and Stelzenmueller, Isernia and Eichenberg (2014). The report did not include any regression or heterogeneity analysis.

³⁴In Germany and in the UK, only people with access to a landline took the survey. In Poland and Russia, participants were randomly selected from the general population, and face-to-face interviews were conducted. Response rates for phone interviews ranged from 4 percent in France, the UK and the Netherlands to 27 percent in the US. Face-to-face interviews had higher response rates: 49 percent in Russia and 40 percent in Poland (Stelzenmueller, Isernia and Eichenberg 2014; Wunderlich et al. 2010).

those who started the survey answered the main questions of interest.³⁵

Information Treatment At the start of the survey, participants are asked which issues they think are the most important ones facing their country, and how closely they follow news on immigration. Then, only participants in the treatment group are informed about the true proportion of immigrants in their country, before being asked whether they think that there are too many immigrants in their country. Thereafter, all respondents are asked a series of questions on their level of concern regarding immigration, their perception of immigrants and on the legalization of immigrants. For example, people are asked whether they are worried about legal and undocumented immigration, and whether undocumented immigrants should be given the opportunity to obtain legal status.

Results In parallel with our first hypothesis, we first check whether the information experiment embedded in the TATS affects beliefs regarding the size of the immigrant group, the only variable directly targeted by the intervention. Column 1 of Table 7 shows that this is the case. People who receive information about the share of immigrants in the US become much less likely to say that there are too many immigrants in the country (an effect of 0.33 standard deviations). We find a similar effect for the average respondent across all other countries included in the survey (Column 4).³⁶ This finding confirms that the information treatment is effective at correcting misperceptions about the size of the immigrant group.

However, we expected that this light information treatment, only correcting misperceptions about the size of immigrant group, would not meaningfully shift people's general

³⁵In order to get as representative a sample as possible for each country, we use the probability weights constructed by the Transatlantic Trends Survey. Our results are not affected in any way by the use of these weights.

³⁶Results are robust to the inclusion or not of control variables, and wave- and country-fixed effects. The sample is well balanced across the treatment and control group as is highlighted in Table A2.

beliefs about immigrants or their immigration policy preferences. In line with our expectation and the findings of the existing literature (Hopkins, Sides and Citrin 2019), Columns 2-3 and 6-7 of Table 7 show that being informed about the proportion of immigrants in the country does not make people (in the US or in the other countries) less worried about immigration. Moreover, Table 8 confirms that this treatment does not change people’s immigration policy preferences.

Finally, Panel B of Tables 7 and 8 show that the effects on general beliefs and policy preferences are not statistically larger for right-wing respondents. This stands in contrast to the heterogeneous effects we found after providing information about both the size and the characteristics of the immigrant group, which significantly affected policy preferences for respondents with more negative views on immigration.

Conclusion

The main substantive contribution of our paper is to provide novel causal evidence on the effects of misinformation about immigrants’ characteristics on people’s policy preferences. We show that providing a package of information that includes not only the size of the immigrant group, but also the characteristics of immigrants improves people’s general beliefs about legal immigrants. We also see significant effects on policy preferences for those groups with more negative attitudes towards immigration before the intervention.

Based on motivated reasoning theory, previous studies have hypothesized that a stronger perception of threat can lead to more negative attitudes and generate larger misperceptions. However, our findings provide evidence for the reverse causal mechanism: innumeracy can cause negative attitudes towards immigrants. The result that a reduction in misperceptions leads to less negative attitudes is consistent with a version of

group-threat theories that view the perceived characteristics of immigrants as the source of the threat.

Our findings have high policy relevance for at least two reasons. First, we show that targeting relevant subgroups can be essential for successful information campaigns. People with negative views on immigration (e.g., Republicans) become more supportive of legal immigration if their misperceptions about the characteristics of the foreign-born population are corrected. Second, we show that the type of information provided makes a difference, and that including objective statistics about the characteristics of immigrants can reduce social distance and thereby increase support for immigration. Interventions focused on information only about the size of the immigrant group have not been effective at affecting policy preferences.

The effects of information on beliefs persist after one month, which indicates that information campaigns can have an effect that is not ephemeral. However, we do see an important reduction in the size of the treatment effect after one month. Therefore, we believe that, over a longer time horizon, the effects on beliefs could disappear due to imperfect memory and the impact of competing pieces of information. To persistently shift beliefs, political organizations would need to run information campaigns repeatedly.

Future research should complement our work in at least two ways: first, it is important to grasp whether the effects of information on political attitudes depend on the credibility of the agent who provides the information (e.g., the government, the media). Second, it is crucial to understand how people process factual information compared to emotionally loaded content.³⁷ Answering these questions will be necessary to find the most effective ways of fighting people's misinformation on important issues, such as immigration.

³⁷Flores (2018) shows that altering the source of negative statements about immigrants does not have differential effects on attitudes, but changing the polarity of the message does, since he only finds an effect for negative messages.

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Tables

Table 1: Balance Table: TNS and MTurk

	Experiment 1			Experiment 2			Experiment 2: follow-up		
	Treatment	Control	P-value	Treatment	Control	P-value	Treatment	Control	P-value
Income	62465	62083	0.834	49183	49003	0.930	49548	48538	0.645
Log Income	10.328	10.352	0.860	10.565	10.556	0.874	10.584	10.533	0.398
Age	40.769	40.303	0.541	35.263	34.389	0.264	35.706	34.883	0.334
Male	0.493	0.494	0.978	0.588	0.532	0.112	0.605	0.515	0.017
Household Size	2.875	2.978	0.201	3.624	3.540	0.389	3.565	3.523	0.691
Hispanic	0.042	0.045	0.763	0.039	0.033	0.662	0.040	0.032	0.601
Black	0.072	0.084	0.435	0.063	0.095	0.103	0.051	0.082	0.101
White	0.829	0.792	0.096	0.793	0.775	0.543	0.797	0.787	0.744
Christian	0.635	0.639	0.908	0.427	0.396	0.383	0.412	0.404	0.811
Full-time employed	0.517	0.519	0.928	0.583	0.565	0.613	0.590	0.567	0.537
Part-time employed	0.152	0.134	0.383	0.180	0.182	0.968	0.181	0.187	0.829
Unemployed	0.094	0.103	0.607	0.078	0.130	0.016	0.073	0.114	0.067
At least bachelor	0.488	0.479	0.748	0.449	0.481	0.364	0.466	0.509	0.261
Born in the US	0.948	0.945	0.782	0.951	0.941	0.530	0.946	0.939	0.662
Worried about immigration	2.768	2.805	0.469	2.759	2.839	0.202	2.791	2.830	0.563
Belief English: Prior	37.032	37.279	0.871	32.687	30.470	0.138	32.335	30.154	0.172
Belief Unemployed: Prior	24.592	23.971	0.649	22.403	18.240	0.002	21.704	18.404	0.023
Belief Share Immigrants: Prior	34.619	34.785	0.900	22.165	23.378	0.283	21.467	23.241	0.134
Belief Share Undoc. Immigrants: Prior	25.470	25.376	0.943	14.078	13.727	0.745	13.568	13.439	0.910
Belief Crime: Prior	18.211	19.306	0.389	12.846	11.857	0.312	12.440	11.604	0.416
Democrat	0.448	0.450	0.938	0.576	0.588	0.718	0.585	0.585	0.999
Republican	0.338	0.303	0.192	0.229	0.238	0.774	0.223	0.249	0.431
Neither Republican nor Democrat	0.214	0.247	0.176	0.195	0.174	0.440	0.192	0.167	0.383

We present the balance test for our samples from Experiment 1 (TNS sample) and Experiment 2 (MTurk sample main survey and follow-up).

Table 2: Main Effects: Beliefs about Targeted Immigrants' Characteristics

	Opinion: Crime (z)	Opinion: Unemployment (z)	Opinion: English (z)	Too many: legal imm. (z)	Too many: undoc. imm. (z)	Index (z)
A: Experiment 1						
Treatment	0.269*** (0.047)	0.310*** (0.052)	0.310*** (0.053)	0.105* (0.046)	0.242*** (0.049)	0.275*** (0.031)
FDR-adjusted p-value	[0.001]	[0.001]	[0.001]	[0.004]	[0.001]	
Observations	1193	1193	1193	1193	1193	1193
Scaled Effect	0.690	2.574	0.567	0.410	0.367	0.701
B: Experiment 2						
Treatment	0.185*** (0.056)	0.518*** (0.062)	0.381*** (0.064)	0.259*** (0.056)	0.273*** (0.052)	0.368*** (0.040)
FDR-adjusted p-value	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	
Observations	800	800	800	800	800	800
Scaled Effect	0.207	1.204	0.457	0.285	0.253	0.471
C: Experiment 2: follow-up						
Treatment	0.117† (0.063)	0.304*** (0.067)	0.208** (0.067)	0.142* (0.061)	0.180** (0.058)	0.213*** (0.043)
FDR-adjusted p-value	[0.026]	[0.001]	[0.002]	[0.010]	[0.002]	
Observations	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, Liebman and Katz 2007). In other words, the coefficients represent the effect size in terms of standard deviations away from the mean. We recode the variables such that high values correspond to positive attitudes towards immigrants. The scaled effect is the treatment effect divided by the average difference in the answers given by Democrats and Republicans in the control group. In Panel A, we display the results from Experiment 1. In Panel B, we display the results from Experiment 2. In Panel C, we show results from the follow-up experiment from Experiment 2. We include the following control variables: log income, age, gender, household size, indicators for race, religion, employment status and education, whether the respondent was born in the US, a question capturing 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. † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3: Main Effects: General Beliefs about Immigrants

	No positive effect of removing undocumented immigrants (z)	Immigrants produce more advantages (z)	Index: Opinions (z)
A: Experiment 1			
Treatment	0.091 [†] (0.049)	0.143** (0.048)	0.117** (0.040)
FDR-adjusted p-value	[0.032]	[0.006]	
Observations	1193	1193	1193
Scaled Effect	0.206	0.354	0.277
B: Experiment 2			
Treatment	0.055 (0.056)	0.187*** (0.052)	0.121* (0.047)
FDR-adjusted p-value	[0.195]	[0.001]	
Observations	800	800	800
Scaled Effect	0.057	0.176	0.119
C: Experiment 2: follow-up			
Treatment	0.127* (0.061)	0.150* (0.054)	0.139* (0.050)
FDR-adjusted p-value	[0.019]	[0.011]	
Observations	694	694	694

All outcome variables are normalized by the mean and the standard deviation of the variable for the control group (Kling, Liebman and Katz 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 Democrats and Republicans in the control group. In Panel A, we display the results from Experiment 1. In Panel B, we display the results from the main part of Experiment 2. In Panel C, we show results from the follow-up experiment from Experiment 2. We include the same list of controls as in Table 2. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets. [†] $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 4: Main Effects: Policy Preferences

	Increase the number of legal immigrants (z)	Increase the number of green cards (z)	Decrease the budget to deport (z)	Faciliate legalization (z)	Not deport undocumented immigrants (z)	Index
A: Experiment 1						
Treatment	0.125*	0.048	0.055	0.003	0.080	0.052
	(0.050)	(0.052)	(0.048)	(0.051)	(0.051)	(0.036)
FDR-adjusted p-value	[0.069]	[0.424]	[0.424]	[0.687]	[0.312]	
Observations	1193	1193	1193	1193	1193	1193
Scaled Effect	0.267	0.082	0.089	0.004	0.161	0.086
B: Experiment 2						
Treatment	0.163*	0.119*	0.062	0.034	0.039	0.060
	(0.059)	(0.059)	(0.059)	(0.060)	(0.058)	(0.047)
FDR-adjusted p-value	[0.032]	[0.096]	[0.419]	[0.511]	[0.511]	
Observations	800	800	800	800	800	800
Scaled Effect	0.197	0.125	0.067	0.034	0.033	0.060
C: Experiment 2: follow-up						
Treatment	0.188**	0.121*	0.122*	0.128*	0.023	0.116*
	(0.062)	(0.061)	(0.059)	(0.061)	(0.064)	(0.044)
FDR-adjusted p-value	[0.013]	[0.049]	[0.049]	[0.049]	[0.167]	
Observations	694	694	694	694	694	694

See Notes to Tables 2 and 3. † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 5: Main Effects: Online Petition and Donation

	(1)	(2)	(3)	(4)	(5)
	Intention to sign	Self-report: Sign	Actual Sign-up	Index: Petition	Donation (z)
A: Experiment 1					
Treatment	-0.031	0.021	0.002	-0.005	0.067
	(0.053)	(0.055)	(0.019)	(0.050)	(0.056)
FDR-adjusted p-value	[1]	[1]	[1]		
Observations	1193	1193	1193	1193	1193
Scaled Effect	-.04	.03	-	-.01	0.171
Control mean	0	0	0.112	0	0
B: Experiment 2: Main					
Treatment	0.061	-0.069	-0.036 [†]	-0.004	0.222*
	(0.063)	(0.054)	(0.019)	(0.054)	(0.082)
FDR-adjusted p-value	[.271]	[.271]	[.212]		
Observations	800	800	800	800	800
Scaled Effect	.09	-.15	-	0	0.363
Control mean	0	0	0.106	0	0

Outcome variables in Columns 1 and 2 are normalized by the mean and the standard deviation of the variable for the control group (Kling, Liebman and Katz 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 Democrats and Republicans in the control group. In Panel A, we display the results from Experiment 1. In Panel B, we display the results from Experiment 2. We include the same list of controls as in Table 2. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets. [†] $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 6: Heterogeneous Effects: Pooled

	Targeted Beliefs immigrants	General beliefs immigrants	Policy Preferences	Donation	Petition
A: Political Affiliation					
Treatment	0.219*** (0.035)	0.016 (0.043)	-0.046 (0.052)	0.099 (0.070)	-0.126* (0.057)
Treatment × Republican	0.207*** (0.058)	0.187* (0.073)	0.201* (0.090)	0.144 (0.108)	0.302*** (0.082)
Treatment × Neither Rep nor Dem	0.134* (0.061)	0.217* (0.078)	0.285** (0.094)	-0.019 (0.118)	0.157† (0.094)
Republican	-0.255*** (0.044)	-0.290*** (0.057)	-0.358*** (0.068)	-0.389*** (0.074)	-0.529*** (0.060)
Neither Rep nor Dem	-0.163*** (0.045)	-0.190*** (0.057)	-0.291*** (0.068)	-0.215* (0.083)	-0.405*** (0.067)
B: Concerned with Imm.					
Treatment	0.295*** (0.024)	0.097*** (0.029)	0.073† (0.038)	0.132* (0.047)	-0.008 (0.037)
Treatment × Concerned with immigration	0.033 (0.034)	0.058 (0.041)	0.213*** (0.052)	0.057 (0.064)	0.092† (0.053)
Concerned with immigration	-0.385*** (0.035)	-0.632*** (0.043)	-0.064 (0.052)	-0.201*** (0.060)	-0.164** (0.051)
C: Trust in statistics					
Treatment	0.308*** (0.025)	0.113*** (0.031)	0.074* (0.038)	0.142** (0.047)	-0.000 (0.037)
Treatment × Trust in statistics	0.026 (0.027)	0.063† (0.033)	0.014 (0.037)	0.076† (0.044)	0.044 (0.036)
Trust in statistics	-0.036† (0.019)	0.023 (0.023)	-0.061* (0.026)	-0.151*** (0.029)	-0.118*** (0.025)
P-value (Tr + Tr× Republican)	0.000	0.001	0.035	0.003	0.003
P-value (Tr + Tr× Neither Rep nor Dem)	0.000	0.000	0.002	0.402	0.681
P-value (Tr + Tr× Concerned)	0.000	0.004	0.000	0.013	0.162
P-value (Tr + Tr× Trust Stat)	0.000	0.000	0.098	0.001	0.389
Observations	1994	1994	1994	1994	1994

All of the outcomes are indices. The definition of the indices is described in the Online Appendix. The outcomes from the petition question are self-reported. All outcome variables are normalized by the mean and the standard deviation of the variable for the control group (Kling, Liebman and Katz 2007). In other words, the coefficients represent the effect size in terms of standard deviations away from the mean. We include the same list of controls as in Table 2. Robust standard errors are displayed in parentheses. † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 7: Transatlantic Trends Survey: Beliefs and Worries about Immigration

	US sample			Non-US sample		
	Too many imm.	Worry legal imm.	Worry undoc. imm.	Too many imm.	Worry legal imm.	Worry undoc. imm.
Panel A: Main						
Treatment	0.334*** (0.073)	-0.172 (0.110)	-0.030 (0.125)	0.238*** (0.020)	-0.017 (0.036)	-0.030 (0.034)
Observations	1858	930	923	17549	6554	6537
Panel B: Right-wing						
Treatment	0.368*** (0.087)	-0.201 (0.142)	0.035 (0.182)	0.188*** (0.025)	-0.057 (0.046)	-0.083 (0.047)
Treatment × Right-wing	-0.088 (0.143)	0.069 (0.221)	-0.117 (0.230)	0.121** (0.042)	0.097 (0.073)	0.133 (0.068)
Right-wing	-0.325** (0.113)	0.114 (0.161)	0.602*** (0.159)	-0.290*** (0.031)	-0.292*** (0.051)	-0.319*** (0.049)
Observations	1858	930	923	17549	6554	6537

We recode the variables such that high values correspond to positive attitudes towards immigrants. All outcome variables are normalized by the mean and the standard deviation of the variable for the control group (Kling, Liebman and Katz 2007). In other words, the coefficients represent the effect size in terms of standard deviations away from the mean. The outcome in Columns 1 and 4 is available for both rounds of the survey, while the outcomes in the other columns are available for only one round. Robust standard errors are in parentheses. † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 8: Transatlantic Trends Survey: Policy Preferences

	US sample			Non-US sample		
	Immigrants can stay permanently	Immigrants can be legalized	More Refugees	Immigrants can stay permanently	Immigrants can be legalized	More Refugees
Panel A: Main						
Treatment	0.014 (0.093)	0.112 (0.104)	-0.088 (0.114)	0.028 (0.035)	-0.050 (0.035)	-0.018 (0.029)
Observations	895	899	878	6521	6521	10356
Panel B: Right-wing						
Treatment	0.010 (0.119)	0.061 (0.146)	-0.031 (0.137)	0.038 (0.047)	-0.039 (0.046)	-0.041 (0.037)
Treatment × Right-wing	-0.0003 (0.186)	0.101 (0.204)	-0.133 (0.218)	-0.023 (0.072)	-0.025 (0.069)	0.056 (0.058)
Right-wing	-0.140 (0.134)	-0.303* (0.151)	-0.449** (0.157)	-0.246*** (0.052)	-0.292*** (0.051)	-0.266*** (0.040)
Observations	895	899	878	6521	6521	10356

All outcome variables in Panels A and B are normalized by the mean and the standard deviation of the variable for the control group (Kling, Liebman and Katz 2007). In other words, the coefficients represent the effect size in terms of standard deviations away from the mean. Robust standard errors are in parentheses. † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figures

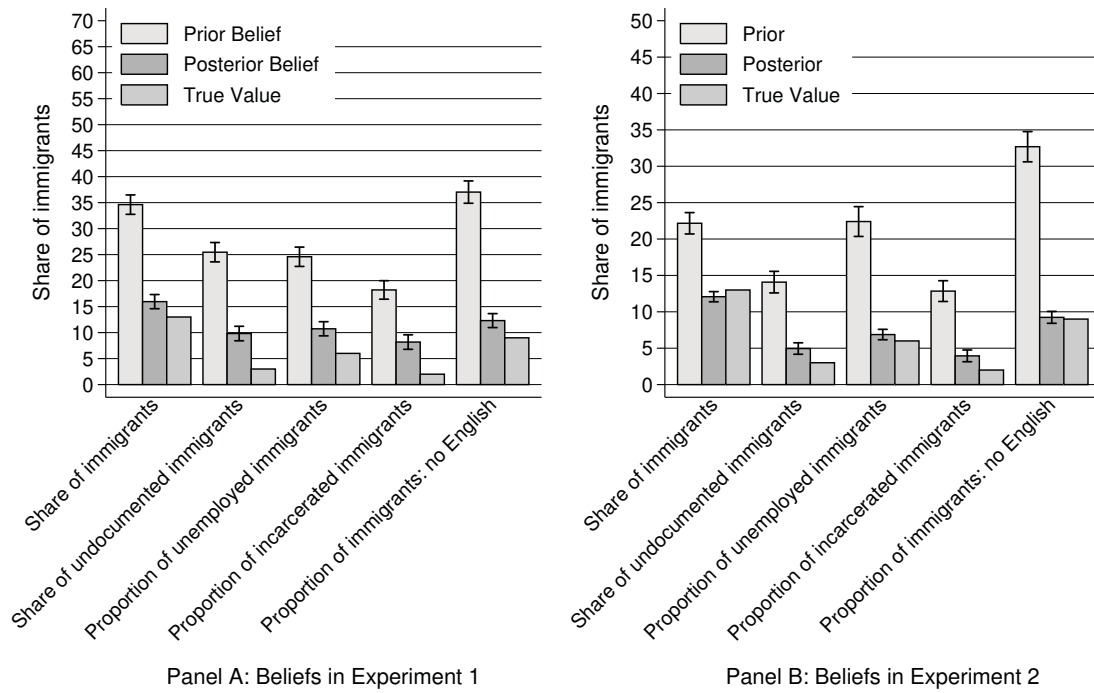


Figure 1: This figure presents the prior and posterior beliefs about the statistics regarding immigrants. On the left-hand side, Panel A presents results for the TNS sample. On the right-hand side, Panel B shows results for the MTurk sample. The figures display the means as well as the 95% confidence intervals.

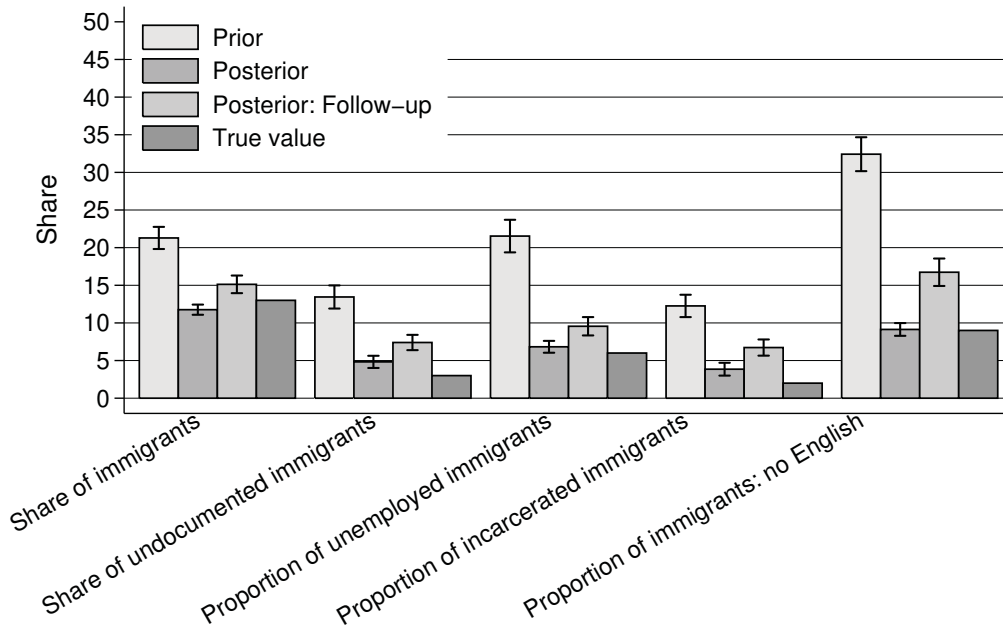


Figure 2: This figure presents prior and posterior beliefs for the sample that answered the four-week follow-up. We also present beliefs elicited in the four-week follow up.

Online Appendix (Supplementary Electronic Material)

Summary of Results in the Online Appendix

We include here two additional tables mentioned in the text. The first one presents results on heterogeneity of treatment effects for the two experiments separately, and for the follow-up survey. The second one is the balance table for the experiment embedded in the Transatlantic Trends Survey. Finally, the last section describes how we constructed each family of outcomes.

Table A1: Heterogeneous Effects: Disaggregated

	Targeted Beliefs immigrants	General beliefs immigrants	Policy Preferences	Donation	Petition
A: Experiment 1					
Treatment	0.171*** (0.046)	-0.030 (0.060)	-0.067 (0.052)	0.003 (0.084)	-0.173* (0.080)
Treatment × Republican	0.183* (0.070)	0.228* (0.092)	0.223* (0.082)	0.109 (0.132)	0.355** (0.112)
Treatment × Other Pol.	0.201* (0.077)	0.334*** (0.100)	0.228* (0.091)	0.135 (0.144)	0.246† (0.127)
Republican	-0.218*** (0.053)	-0.197** (0.069)	-0.346*** (0.062)	-0.303** (0.098)	-0.584*** (0.081)
Other Pol.	-0.202*** (0.056)	-0.186* (0.073)	-0.218*** (0.066)	-0.258* (0.099)	-0.463*** (0.087)
Observations	1193	1193	1193	1193	1193
p(Tr+ Tr × Rep)	0.000	0.004	0.013	0.267	0.020
p(Tr+ Tr × Neither Rep. nor Dem.)	0.000	0.000	0.030	0.238	0.462
B: Experiment 2					
Treatment	0.292*** (0.054)	0.061 (0.061)	-0.060 (0.060)	0.197† (0.115)	-0.063 (0.080)
Treatment × Republican	0.284** (0.097)	0.154 (0.112)	0.270* (0.111)	0.265 (0.178)	0.233* (0.111)
Treatment × Other Pol.	0.042 (0.098)	0.091 (0.121)	0.264* (0.125)	-0.230 (0.199)	0.021 (0.138)
Republican	-0.199* (0.087)	-0.268* (0.108)	-0.266* (0.110)	-0.457** (0.149)	-0.131 (0.101)
Other Pol.					
Observations	800	800	800	800	800
p(Tr+ Tr × Rep)	0.000	0.024	0.027	0.001	0.026
p(Tr+ Tr × Neither Rep. nor Dem.)	0.000	0.147	0.064	0.843	0.710
C: Experiment 2 Follow-up					
Treatment	0.255*** (0.058)	0.048 (0.065)	-0.101 (0.064)	0.166 (0.121)	-0.097 (0.085)
Treatment × Republican	0.314** (0.102)	0.229* (0.113)	0.300* (0.119)	0.205 (0.175)	0.271* (0.114)
Treatment × Other Pol.	0.097 (0.112)	0.124 (0.128)	0.326* (0.135)	-0.130 (0.205)	0.072 (0.154)
Republican					
Other Pol.	0.191* (0.096)	0.266* (0.110)	0.233* (0.115)	0.394* (0.141)	0.146 (0.106)
Observations	696	696	696	696	696
p(Tr+ Tr × Rep)	0.000	0.004	0.051	0.005	0.024
p(Tr+ Tr × Neither Rep. nor Dem.)	0.000	0.118	0.059	0.834	0.845

All of the outcomes are indices. The definition of the indices is described in the Online Appendix. The outcomes from the petition question are self-reported. All outcome variables are normalized by the mean and the standard deviation of the variable for the control group (Kling, Liebman and Katz 2007). In other words, the coefficients represent the effect size in terms of standard deviations away from the mean. We include the same list of controls as in Table 2. Robust standard errors are displayed in parentheses. † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A2: Transatlantic Trend Survey: Randomization Check

	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

This table presents means for treatment and control groups and the p-value for the test on whether these two means are statistically different for the full sample in the two waves of the TATS studied in the text. † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Families of outcomes: Construction of indices

First, we group our 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.³⁸ We define the families of outcomes as follows:

³⁸We first recode the variables such that high values correspond to positive attitudes towards immigrants for all variables. We then normalize each variable (i.e., we subtract the mean of the control group

- **Targeted Beliefs Immigrants:** Index of beliefs regarding the five variables targeted by the intervention (characteristics of immigrants and size of immigrant group). For each of these questions participants choose one of following options: [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree.]
 - Immigrants are more likely to commit crimes than US citizens.
 - Immigrants are more likely to be unemployed than US citizens.
 - Immigrants generally learn English within a reasonable amount of time.
 - There are currently too many immigrants in the US.
 - There are currently too many illegal immigrants in the US.

- **General Beliefs Immigrants:** Index of beliefs regarding variables not directly targeted by the intervention.
 - Suppose US authorities were able to remove almost all illegal immigrants from the US. What effect do you think this would have on the US 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 US as a whole. [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree.]

- **Policy Preferences:** Index of policy preferences regarding immigration.
 - 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

and divide by the standard deviation of the control group). Finally, we calculate the covariances between the variables that are part of the same family and use the inverse of the covariance matrix in order to weigh them. For more details see Anderson (2008).

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.]
- 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 US 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 US citizens, but only if they meet certain requirements over a period of time.]

- **Petition:** Index of people's willingness to sign a petition.

- **Intention to sign:** one if says that wants to sign the petition, zero otherwise.
- **Self-reported signing:** one if says that signed the petition, zero otherwise.