ESSAYS

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EXPERIMENTAL ECONOMICS

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Abstract

In the first chapter, I investigate the impact of income inequality on anti-social behaviour through an experiment. Participants are first assigned to two different kinds of groups: equality groups, where everyone is paid the same amount for performing a simple task, and inequality groups, where people receive different amounts for completing the same task. Each participant is then told that one member of their group will have the possibility of taking some of the money earned by another group member. Contrary to the predictions made by the Becker model and the Fehr-Schmidt inequity aversion model, participants in the inequality groups steal significantly less from a fellow group member with the same income as them than people in the equality groups. The second chapter, jointly written with Christopher Roth, examines the influence of economic status on pro-social behaviour using a large experiment with a representative sample of the US population. We exogenously alter people's perceived economic status by changing where they think their household stands in the US income distribution. Half of the people who over-estimated their position in the income distribution are told that they are relatively poorer than they thought. Conversely, half of those who under-estimated their position in the income distribution are informed that they are relatively richer than they thought. Then, participants play a series of four incentivised games, which measure different social preferences, such as trust, negative reciprocity, honesty and altruism. We show that individuals who learn that they are lower (resp. higher) in the income distribution than they thought become less (resp. more) satisfied with their relative position in the income distribution, but that their behaviour remains unchanged in the four behavioural games. The third chapter, co-written with Christopher Roth and Diego Ubfal, studies whether providing information about immigrants affects people's attitude towards them. First, we use a large representative cross-country experiment to show that, when people are told the share of immigrants in their country, they become less likely to state that there are too many of them. Then, we conduct two online experiments in the US, where we provide half of the participants with five statistics about immigration, before evaluating their attitude towards immigrants with self-reported and behavioural measures. This more comprehensive intervention improves people's attitude towards existing immigrants, although it does not, on average, change people's policy preferences regarding immigration. However, Republicans become more willing to increase legal immigration after receiving the information treatment.

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Alexis Grigorieff (April 2018)

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Introduction

In this thesis, I present several experiments which shed some light on the following economic questions: How do relative concerns and social comparisons affect pro-social and anti-social behaviour? And to what extent are people's attitudes and policy preferences on immigration shaped by incorrect beliefs about the characteristics of immigrants?

The first two chapters share a common theme, as they both investigate the impact of income inequality on pro-social and anti-social behaviour. While the first chapter provides some evidence on how people's propensity to steal can be affected by income inequality in a stylised environment, the second chapter uses the fact that people in the United States have a very biased perception of their position in the income distribution to study how their pro-social behaviour changes when they learn that they over-estimated or under-estimated their position in the income distribution. The third chapter also examines how information provision can correct people's biases and affect their behaviour, this time in the context of political preferences on immigration. In the rest of the introduction, I will give a more detailed overview of each of the three chapters, and of how they contribute to the existing literature.

The first chapter presents the results from an experiment where participants are assigned to two different kinds of groups: equality groups, where everyone is paid the same amount for performing a simple task, and inequality groups, where people receive different amounts for completing the same task. Each participant is then told that one member of their group will have the possibility of taking some of the money earned by another group member. Whereas theoretical models, such as the Becker (1968) model and Fehr and Schmidt (1999)'s inequity aversion model, predict that participants in the inequality groups should steal at least as much as the ones in the equality groups, the opposite actually occurred in the experiment. Indeed, participants in the inequality groups stole significantly less often from a fellow group member with the same income as them than people in the equality groups. In addition, I show that there are large heterogeneous treatment effects by gender, age, and social class.

This chapter contributes to the literature in several ways. First of all, there is a lack of causal evidence on the impact of income inequality and relative concerns on social preferences, although it is a topic which has garnered a lot of attention in recent years, *e.g.* Wilkinson and Pickett (2010). Moreover, existing studies only keep the *average* income constant between the equality groups and the inequality groups, but they fail to keep the *income levels* constant between the two kinds of groups. This makes it impossible for them to compare meaningfully the behaviour of people in equality groups with that of participants in the inequality groups, a shortcoming that my experimental design addresses.

In my second chapter, a co-author and I investigate the impact of economic status on pro-social behaviour using a large experiment with a representative sample of the US population. We exogenously alter people's perceived economic status by changing where they think their household stands in the US income distribution. Half of the people who over-estimated their position in the income distribution are truthfully told that they are relatively poorer than they thought. Conversely, half of those who under-estimated their position in the income distribution are informed that they are relatively richer than they thought. Then, participants play a series of four incentivized games, which measure different social preferences, namely trust, negative reciprocity, honesty and pro-sociality. We show that individuals who learn that they are lower in the income distribution than they thought become less satisfied with their relative position in the income distribution, but that their behaviour remains unchanged in the four behavioural games. Similarly, people who learn that they are relatively richer than they thought become more satisfied with their relative standing in society, but they do not change their behaviour in any of the four games. The absence of significant treatment effects is not due to a lack of statistical power, as we had the power to detect effect sizes of 0.2 standard deviations with a probability of 80 percent.

This second paper contributes to the emerging literature on relative income and social preferences. There are a few experiments, such as Piff et al. (2012);

Côté et al. (2015), where participants are first primed to feel either rich or poor, before being observed in various situations where they can act selfishly or not. However, one of the main issues with these studies is that they lack a pure control group, and they are thus unable to identify separately the effect of feeling poorer and the effect of feeling richer. Moreover, we use a large representative sample of the US population, which alleviates concerns about external validity, and we use behavioural outcomes to measure pro-social and anti-social behaviour instead of self-reported ones.

In my third chapter, two co-authors and I examine whether providing information about immigrants affects people's attitude towards them. First, we present the results from a large representative cross-country survey experiment with more than 19,000 participants in which half of the sample receives information about the share of immigrants in their country, before being asked questions about their views on immigration. Then, we also conduct two online experiments in the U.S., where we provide half of the participants with five statistics about immigration, before measuring their attitude towards immigrants with self-reported and behavioural outcomes. We find that participants in the treatment group display more favourable attitudes towards immigrants, but do not change their policy preferences. Republicans and people who are worried about immigration respond more strongly to the information treatment in terms of 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.

Our evidence extends the existing literature in various ways. First of all, we do not merely examine how giving people information about the number of immigrants affects their policy preferences on immigration, but we also provide them with information about the characteristics of current immigrants, which has been shown to be an important factor in the formation of policy preferences on immigration. We also use behavioural outcomes to measure political attitudes and policy preferences, in order to avoid potential experimenter demand effects. In particular, we introduce a novel way of measuring political preferences through real online petitions that participants can sign if they support a particular policy. Finally, the cross-country survey experiment allows us to get representative evidence from thirteen countries on the effects of information on people's attitude towards immigration, which reduces potential concerns about external validity.

Chapter 1

The Good, the Bad and the Ugly: Ethical Norms, Income Inequality and Theft

1.1 Introduction

As John Stuart Mill (1907) rightly noted, “men do not desire merely to be rich, but to be richer than other men” (p.49). Indeed, it is now well-established that people care about social comparisons, and that they resent having less than their peers (Festinger, 1962; Luttmer, 2005). However, it is still unclear to what lengths people will be ready to go in order to reduce the gap that separates them from richer people. In particular, are people willing to break some of the ethical norms that they usually abide by in order to achieve this goal? And how does inequality affect people's moral behaviour towards others? This article presents the results of an experiment which addresses these two questions, by examining whether the presence of income inequality makes people more (or less) likely to steal from other participants.

The experiment is structured as follows. Each participant is first assigned to a group of six people. Each group is either an inequality group or an equality group. In an inequality group, the participants all perform the same task but are paid different amounts. For the work they accomplish, two of them will receive $1, two $3, and the remaining two $5. In contrast, in equality groups, everyone earns the same amount for completing the task. In total, there are three sorts of equality groups: $1-equality groups, where all the group members earn $1, $3-equality groups, where everyone earns $3, and $5-equality groups, where all six group members earn $5.

After having completed the task, everyone is informed of how much they earned, and of how much their fellow group members earned. In both equality and inequality groups, participants are then told that out of the six people in their group, two shall be chosen at random, and that those two people shall be referred to as person A and person B. Person A will have the option of taking some of the money person B earned earlier in the experiment. The other four group members will not be affected by the decision of person A. All the decisions are made using the strategy method, *i.e.* participants specify how they would act in every possible configuration of the game.

This experiment can be used to contrast the behaviour of people in equality and inequality groups. Indeed, one can compare how much someone with $\$x$ would take from a fellow group member who also earned $\$x$, in the inequality groups and in the equality groups.

Models that do not take into account relative concerns, such as the seminal Becker (1968) model of crime, predict that participants in the inequality groups should behave like those in the equality groups. On the other hand, models that allow for relative concerns, such as the Fehr and Schmidt (1999) inequity aversion model, predict that participants in the inequality groups will steal from their fellow group members more often than participants in the equality groups, in order to reduce disadvantageous inequality.

However, these predictions are not borne out in the experimental data. Participants in the inequality groups stole on average *less* from a fellow group member with the same income as them than people in equality groups. In other words, it appears that people in the inequality groups showed more solidarity with fellow group members who had earned the same income as them than people in the equality groups. This finding is particularly striking, as it shows that people are not necessarily willing to break ethical norms to reduce disadvantageous inequality. This intuition can also be formalised, by modifying the Becker model.[1]

[1]The results of the experiment cannot be meaningfully compared to the cross-country evidence on the relationship between income inequality and crime, because the latter suffers from various important shortcomings. Indeed, the cross-country findings are merely correlational, as it is particularly hard to find an exogenous shock to income inequality. Moreover, changes in income inequality are often correlated with other changes, such as income, which makes it harder to measure the effect of income inequality on crime *per se*. Finally, the available crime data does not provide any information on the income of the perpetrator or the victim, which prevents me from comparing my findings and the cross-country evidence.

Very little experimental evidence exists on the link between inequality and immoral behaviour. The most relevant study on this topic is Zizzo (2004), although his main purpose and experimental design are very different. Importantly, he does not show whether people tend to steal more (or less) when there is income inequality than when everyone is paid the same amount.

The main objective of Zizzo (2004)'s experiment was to see whether participants are willing to pay some money to reduce, redistribute or steal the payoffs of other participants, when the distribution of endowments is unequal. However, his study differs significantly from mine because he did not include a control group where everyone received the same endowment. He was therefore unable to study the effect of endowment inequality *per se* on the stealing behaviour of his participants. Moreover, expectations played a crucial role in his study, because everyone's decision was implemented, which implies that participants had to think strategically about what other group members would do, before deciding what to choose. Whereas in my experiment, participants knew that only one person's choice would be implemented, which meant that they did not need to take into account what others would do.

Methodologically, my experimental design is closer to the handful of studies which investigated the impact of income inequality on trust, contribution in public goods games, and common resource management (Heap et al., 2013; Anderson et al., 2006, 2008). Still, the type of design used in these studies differs from mine in two crucial ways. First of all, they only keep the *average* endowment constant between the equality and the inequality groups, whereas

I also keep the income levels constant between the two kinds of groups. For instance, Anderson et al. (2006) designed an experiment to investigate the effects of endowment inequality on trust, where they kept the *average* endowment constant at \$7.50 in both the equality and the inequality groups, but where nobody actually earned \$7.50 in the inequality groups. It is therefore much harder to justify comparing the behaviour of people in the inequality groups with that of the participants in the equality groups, who all earned \$7.50. Secondly, their experiments all involve repeated interactions between participants, which is potentially problematic because participants' later decisions could be influenced by the way their first partners behaved towards them.[2] In my design, participants cannot be influenced by earlier play, because it is a one-shot game, where participants must specify their full strategy before learning the outcome of the game.

The chapter is structured as follows. In section 1.2, I present the design of the experiment. In section 1.3, I review the predictions made by two very influential models, the Becker (1968) model of criminal activity and Fehr and Schmidt (1999)'s inequity aversion model. Then, I present the dataset and the empirical results from the experiment in section 1.4, before explaining what mechanisms might be at play in section 1.5. Finally, section 1.6 concludes.

[2] It would not be sufficient either to focus on the first round of their experiments, because participants' awareness of the future rounds could affect their behaviour in the first round.

1.2 Overview of the Experiment

1.2.1 First Stage

In the first stage, participants are assigned to groups of six people each. There are two kinds of groups: inequality groups and equality groups.

- *Inequality Groups*: Participants are told that everyone in their group will perform the same task for approximately 10 minutes, but that they will not all be paid the same amount for the work they accomplished.[3] Two people shall receive $1, two will earn $3, while the last two will get $5. It is only after they have completed the task that they will learn how much they themselves earned, and how much the other members of the group made. The allocation of earnings will be made at random, and will not depend on participants' individual performance in the task.
- *Equality Groups*: There are three kinds of equality groups, which shall be referred to as $1-equality groups, $3-equality groups and $5-equality groups. In all three types of equality groups, participants are told that everyone in their group will perform the same task for approximately 10 minutes, and that the six of them will be paid the same amount for doing the work. However, in $1-equality groups, the six group members are told that they will get paid $1 for finishing the task, while in $3-equality groups, they are told that they will all earn $3, and in $5-equality groups,

[3]The task that participants need to a do is a decryption task. Please refer to appendix A for the exact instructions.

they are told that everyone will receive \$5. In all three types of equality groups, participants are paid only after having completed the task.

To summarise: after having been assigned to a group, participants are informed of whether they belong to an inequality group, a \$1-equality group, a \$3-equality group or a \$5-equality group. They then perform the task, and after having completed it, they are told how much they earned.

1.2.2 Second Stage

The second stage of the experiment is almost identical for the four types of groups. In all four of them, participants are informed that out of the six people in their group, two shall be chosen at random, and that those two people will be referred to as person A and person B. Person A will have the option of taking some of the money person B has earned in the first stage. Person A will therefore earn the amount that he or she has received for completing the task in the first stage *plus* the amount he or she decided to take from person B. On the other hand, person B will receive the amount that he or she got for completing the task in the first stage *minus* the amount that person A decided to take from him or her. The other four participants will receive the amount they earned for completing the task, and will not be affected by the decision of person A.

However, at this stage of the experiment, nobody has been picked yet to play the roles of person A and person B, and so all six members of the group have the same chance of being selected to be person A. Therefore, each person will be asked to indicate how much money (if any) they would like to take from person B, if they were selected to be person A. Given that person B has not been picked yet either, they will have to indicate how much money they would take from the various members of their group. Hence, in the inequality groups, each person will have to indicate how much they would take from someone who earned \$1, someone who earned \$3 and someone who earned \$5. Similarly, people in the \$1-, \$3- and \$5-equality groups will have to specify how much they would take from someone in their group, *i.e.* from someone who earned the same amount as them in the first stage.

1.2.3 Remainder of the Experiment

Once the second stage is over, participants will be asked to answer a series of questions on the experiment that they have just participated in. First of all, they will have to give a reason for choosing the amount they took from each of their potential partners in the second stage. After that, they will have to rate how satisfied they are with the experiment so far, and how fairly they think they have been paid for completing the task.

They will also be asked how much they think the other participants in their group would take from them, if they had the opportunity to do so. People in the inequality groups will have to specify how much they think participants with \$1, \$3 and \$5 would take from them, while participants in the equality groups will only have to say how much they believe someone with the same income as them would take from them.

Then, they will have to specify what they think would be a fair amount for the other participants to take from them. People in the inequality groups will give three answers, one for each income level, while people in the equality groups will only give one, given that all of them earned the same income.

Furthermore, people in equality groups will be asked to state how much they would take from someone who earned the same amount as them, if they were in an inequality group. Similarly, participants in inequality groups will indicate how much they would take from someone who earned the same amount as them, if they were in a corresponding equality group.

After having answered those questions, all the participants will be asked to play a few hypothetical games, such as a dictator game and a trust game. In the dictator game, participants will be asked to specify how much money they would give to someone in their group with the same income as them, if they were given an extra $6. Dictator games have been widely used in the literature to study people's other-regarding preferences (Forsythe et al., 1994), and the results from this game provide an estimate of how generous participants would be towards other people that they do not know.

For the trust game, participants will be asked to imagine that they are given an extra $4, and that they can send some of those $4 dollars to a fellow group member who earned the same income as them. The amount that they send will be multiplied by 3 and their partner will have the option of sending back some of that money. They will then have to specify how much they would send to their partner. Trust games have been shown to provide good measures of people's trust toward strangers, which could be an important factor in explaining people's behaviour in the stealing game (Naef and Schupp, 2009).

Finally, they will be asked to fill out a questionnaire asking them for some general information, such as gender, education level, yearly income etc. It is only after having collected the data from all the participants that a person A and a person B will be randomly selected in each group. The six members of each group will then be paid by following the procedure described in the second stage. On top of that, everyone who completes the experiment will receive $1.50, which can be viewed as a show-up fee for participating in the experiment.

1.3 Theoretical Predictions

There are various models which can be studied in order to get theoretical predictions on the behaviour of the participants in the experiment. In particular, Becker (1968)'s model of criminal behaviour and Fehr and Schmidt (1999)'s model of inequity aversion have been particularly influential in the economic literature and they lead to very different predictions, which can be tested using the data from the experiment.

1.3.1 The Becker Model

Becker's theory of criminal activity assumes that people rationally decide to commit an anti-social action if the benefit to them outweighs the cost.

In the context of the experiment, there is no material cost to taking money from another group member, because participants have the guarantee that they will not be punished for doing so. Furthermore, it is safe to assume that people prefer to have more money than less, which implies that the benefit of taking money from one's partner will necessarily outweigh the material cost. Therefore, it might seem that Becker's model predicts that everyone in the experiment will rob their partner of all their earnings.

However, material costs are not the only ones that can play a role in a person's decision-making. Indeed, there is a vast body of evidence showing that people can incur psychological costs on top of material costs, such as guilt, regrets etc (Gneezy, 2005; Festinger, 1962). In the case at hand, it is plausible to assume that most people consider that stealing is wrong, and therefore breaking this ethical rule will be costly to them. Indeed, Becker himself argued that some people "were constrained by moral and ethical considerations, and they did not commit crimes even when these were profitable and there was no danger of detection" (Becker, 1993).

According to this version of the Becker model, the utility function of person i can be represented as follows:

$$U_i(x_i, s_i, x_j) = v_i(x_i + s_i) - c_i(s_i, x_i, x_j) \tag{1.1}$$

- where v_i is a strictly increasing function of the total amount earned by person i, which consists of the amount earned in the first stage (x_i) plus the amount stolen by person i from person j (s_i),
- and where c_i is a function of the amount stolen by person i (s_i) from person j, of the income earned by person i in the first stage (x_i), and of the income earned by person j in the first stage (x_j). The functions v_i and c_i can be heterogeneous among participants.

Observation 1: *If participants have utility function (1.1), the behaviour of a participant i will be the same in an equality group as in an inequality group, as long as the initial income of their partner is the same.*

Proof: The utility function of person i only depends on their income and on that of person j, and not on the income of the four other group members. This implies that person i maximises the same function in an equality group as in an inequality group, and his or her behaviour must therefore be the same in an equality group as in an inequality group. □

So far, the cost function c_i has been written in a very general form, which can be different for every person, and which can take any shape. However, it is also useful to consider the case where the cost function does not depend on the amount stolen by person i from person j, but only on whether that amount is different from zero or not. In other words, let us assume that people bear a *fixed* cost for breaking the moral norm against stealing. In this case:

$$U_i = \begin{cases} v_i(x_i + s_i) - c_i(x_i, x_j) \text{ if } s_i > 0 \\ v_i(x_i) \qquad\qquad\qquad\quad \text{if } s_i = 0 \end{cases}$$

Observation 2: *If the cost function does not depend on the amount stolen, participants will either steal nothing or they will steal everything from their partner.*

Proof: Since the cost is fixed and the monetary benefit is increasing in the amount stolen, participants will always take everything they can from their partner if the benefit of stealing outweighs the cost. Otherwise, they will not take anything from their partner. □

1.3.2 The Linear Fehr-Schmidt Model

It might seem simplistic however to assume that individuals do not care about their income relative to other people, which is one of the reasons that led Fehr and Schmidt to develop a model where some people have an explicit aversion for inequitable outcomes (Fehr and Schmidt, 1999). There is now a large body of evidence showing that social comparisons play a very important role in personal well-being (Clark and Oswald, 1996), and that people also care about their payoff relative to others (Loewenstein et al., 1989). What emerges from these studies is that many people strongly dislike disadvantageous inequality, *i.e.* when others fare better than them, and to a lesser extent, advantageous inequality, *i.e.* when their payoff is above that of other people.

This type of preferences can be represented by the following utility function:

$$U_i(w) = w_i - \alpha_i \frac{1}{n-1} \sum_{j \neq i} \max\{w_j - w_i, 0\} - \beta_i \frac{1}{n-1} \sum_{j \neq i} \max\{w_i - w_j, 0\} \quad (1.2)$$

This equation states that agent i cares about his or her own monetary payoff w_i, but also about differences between his or her own payoff and the payoff of each of the other $n-1$ people in the person's reference group. In the context of the experiment, I assume that the relevant reference group is the entire group, which is composed of six people. The second term "measures the utility loss from disadvantageous inequality, while the third term measures the loss from advantageous inequality" (Fehr and Schmidt, 1999). It is also assumed that $\beta_i \leq \alpha_i$, which implies that people care more about disadvantageous than advantageous inequality. An additional assumption is that $0 \leq \beta_i < 1$, which places a constraint on how much people can care about advantageous inequality. Finally, I assume that $\beta > 5/6$ for at least some people.[4]

This baseline model can be used to make predictions regarding the behaviour of participants in the experiment.[5] The fact that people care about inequality means that they will not behave in the same way in equality groups as in inequality ones.

[4] This last assumption will be important for the comparative statics of the model.

[5] In order to get less stark predictions, it is possible to make the utility function concave in the amount of advantageous inequality, instead of having it be piecewise linear (Fehr and Schmidt, 1999). To see how this can be successfully implemented, refer to Bellemare et al. (2008).

Equality Groups

In the state of the world where a subject is person A in an \$$x$-equality group, he or she will maximise the following function:[6]

$$
\begin{aligned}
U_i &= x+s-\overbrace{\frac{4\beta(x+s-x)}{5}}^{\text{term I}}-\overbrace{\frac{\beta(x+s-(x-s))}{5}}^{\text{term II}} \\
&= x+s-\frac{6\beta s}{5}
\end{aligned}
$$

where x is the amount earned by person i in the first stage ($x = \{1,3,5\}$), and where s represents the amount that he or she takes from a fellow group member. Term II refers to the utility cost caused by the comparison of person A with person B, while term I refers to the utility cost induced by the comparison of person A with the remaining four participants in the group.

Observation 3: *If participants have utility function (1.2), then participants in \$1-, \$3- and \$5-equality groups will take all of their partner's money if* $\beta \leq 5/6$ *and nothing if* $\beta > 5/6$.[7]

In other words, for $x_i = \{1,3,5\}$:

$$
s^* = \begin{cases} 0 \text{ if } \beta > 5/6 \\ x_i \text{ if } \beta \leq 5/6 \end{cases}
$$

[6]The show-up fee of \$1.50 is not taken into account in the calculations, as its inclusion would not change any of the results. Participants who do not play the role of person A do not have to decide anything, and we therefore do not need to consider their behaviour.

[7]Note that I assume that participants will take their partner's money if they are indifferent between taking it and not taking it, *i.e.* if $\beta = 5/6$.

Proof: The function $x+s-6\beta s/5$ is strictly decreasing if $\beta > 5/6$, so s will be as low as possible (*i.e.* $s=0$). The function $x+s-6\beta s/5$ is strictly increasing if $\beta < 5/6$, so s will be as high as possible (*i.e.* $s=x$). □

Inequality Groups

Let us now focus on the predictions that the inequity-aversion model makes regarding the behaviour of participants in the inequality groups, when they are paired with someone else who earned the same income as them.

Person with $5:

Observation 4: *If participants have utility function (1.2), then participants with $5 in the inequality groups will behave like the participants belonging to $5-equality groups.*

In other words, when $x_i = 5$:

$$s^* = \begin{cases} 0 \text{ if } \beta > 5/6 \\ 5 \text{ if } \beta \leq 5/6 \end{cases}$$

Proof: Participants in inequality groups who earn $5 in the first stage maximise the following utility function:

$$\begin{aligned} U_i(s) &= 5+s-\overbrace{\frac{2\beta(5+s-3)}{5}}^{\text{Term I}}-\overbrace{\frac{2\beta(5+s-1)}{5}}^{\text{Term II}}-\overbrace{\frac{1\beta(5+s-(5-s))}{5}}^{\text{Term III}} \\ &= 5+s-\frac{6\beta(2+s)}{5} \end{aligned}$$

Term I refers to the utility cost caused by the comparison of person A with the two group members who earned \$3. Term II corresponds to the utility cost induced by the comparison of person A with the two participants who earned \$1, while term III refers to the utility cost caused by the comparison of person A with the other person whose income was \$5 in the first stage.

The utility function of participants with \$5 in the inequality groups is strictly decreasing if $\beta > 5/6$, so s will be as low as possible (*i.e.* $s = 0$). The utility function is strictly increasing if $\beta < 5/6$, so s will be as high as possible (*i.e.* $s = 5$). $\square$

Person with \$3:

Observation 5: *If* $\beta \leq 5/6$, *participants with \$3 will take all of their partner's income, but if* $\beta > 5/6$, *they will take \$2. Therefore, people with \$3 in inequality groups take on average more money from somebody with the same income as them than people in \$3-equality groups, since the former always take at least \$2 from their partner.*

In other words, when $x_i = 3$:

$$s^* = \begin{cases} 2 \text{ if } \beta > 5/6 \\ \\ 3 \text{ if } \beta \leq 5/6 \end{cases}$$

Proof: The analysis is slightly more complicated for people who earned \$3 in an inequality group, because their position in the income distribution could change depending on how much they take from their partner.

If they take less than \$2, *i.e.* $s \le 2$:

$$\begin{aligned} U_i(s) &= 3+s-\frac{2\alpha(5-(3+s))}{5}-\frac{\beta(3+s-(3-s))}{5}-\frac{2\beta(3+s-1)}{5} \\ &= 3+s-\frac{2\alpha(2-s)}{5}-\frac{4\beta(1+s)}{5} \end{aligned}$$

This function is strictly increasing if $1-4\beta/5+2\alpha/5>0$, which is always the case given that $\beta<1$ and $\alpha \ge 0$, and thus $s=2$.

If people with \$3 take more than \$2 (*i.e.* $s>2$), their final payoff will be greater than that of all of the other group members. Therefore, their utility function will be represented by:

$$\begin{aligned} U_i(s) &= 3+s-\frac{2\beta(3+s-5)}{5}-\frac{\beta(3+s-(3-s))}{5}-\frac{2\beta(3+s-1)}{5} \\ &= 3+s-\frac{6\beta s}{5} \end{aligned}$$

This function is clearly strictly increasing if $\beta \le 5/6$ and decreasing if $\beta > 5/6$. Moreover, it is important to note that the utility function is continuous at $s=2$, *i.e.* $\lim_{s\to 2^-} U_i(s) = \lim_{s\to 2^+} U_i(s)$, which implies that people will either take \$2 or \$3 from their partner, depending on the value of β. □

Person with \$1:

Observation 6: *People who earned \$1 in an inequality group will take everything they can from someone else in their group with \$1. People who earned \$1 in the inequality groups will take more on average from someone with the same income as them than people in \$1-equality groups.*

In other words, when $x_i = 1$:

$$s^* = 1 \quad \forall \beta$$

Proof: People who earned \$1 in an inequality group maximise the following utility function:

$$\begin{aligned} U_i(s) &= 1 + s - \frac{2\alpha(5-(1+s))}{5} - \frac{2\alpha(3-(1+s))}{5} - \frac{\beta(1+s-(1-s))}{5} \\ &= 1 + s - \frac{2\alpha(6-2s)}{5} - \frac{2\beta s}{5} \end{aligned}$$

which is clearly strictly increasing for all values of α and β. Indeed, the function is strictly increasing if $1 + 4\alpha/5 - 2\beta/5 > 0$, which is always satisfied given that $\beta < 1$ and $\alpha \geq 0$. Therefore, participants who earned \$1 in the inequality groups will always take everything they can from someone else in their group with \$1, whereas participants in \$1-equality groups will only do so if $\beta \leq 5/6$. □

In sum, people with \$1 or \$3 in the inequality groups will take more money from their partner than people in the corresponding equality groups. This behaviour is prompted by people's natural aversion to disadvantageous inequality. On the other hand, people at the top of the income distribution do not suffer from disadvantageous inequality, and thus there will not be any discernible difference between the behaviour of participants in \$5-equality groups and that of people who earned \$5 in the inequality groups. In other words, people will only start to pay attention to the well-being of others once they are themselves not in a position where they suffer from disadvantageous inequality.

1.3.3 Summary

The two models presented in this section lead to very different testable restrictions. On the one hand, there is the Becker model, which implies that people will behave in the same way in the equality and the inequality groups, and that people will either steal nothing or everything they can. On the other hand, the Fehr-Schmidt model predicts that people in the inequality groups should steal more often than their counterparts in the equality groups, because people care about disadvantageous inequality and their position in the income distribution.

1.4 Empirical Analysis

1.4.1 Description of the Data

Amazon Mechanical Turk

The experiment was conducted on Amazon Mechanical Turk (MTurk), an online crowdsourcing marketplace developed in 2005 by Amazon.com, Inc. This platform is now commonly used by academics to conduct online experiments, as it provides a cheap and efficient way of recruiting participants (Paolacci and Chandler, 2014).

One of the main advantages of Mechanical Turk is that the pool of workers available on MTurk is very large and much more representative of the US population than the student samples commonly used in lab experiments. Indeed, college students constitute a fairly homogeneous group which is typically younger, more educated, and poorer than the general population, which could potentially undermine the generalisability of the results.

It is also important to note that MTurk participants produce high-quality data (Mason and Suri, 2012; Horton et al., 2011; Buhrmester et al., 2011), that they tend to be more attentive to instructions than college students (Hauser and Schwarz, 2015), and that they are less likely to fail attention checks. Indeed, participants on MTurk have an incentive to pay attention to the answers they provide, because researchers can negatively affect MTurkers' approval rating if the quality of the data is too low. Workers care about having a high approval rating because it allows them to complete more tasks. Only workers with an overall rating of more than 95 percent were allowed to take part in the study, which makes it more likely that the data is reliable (Peer et al., 2014).

Furthermore, many lab experiments have now been replicated using MTurk samples, and MTurk participants behave in the same way as traditional subjects in a wide variety of economic games (Goodman et al., 2013; Horton et al., 2011; Klein et al., 2014; Paolacci et al., 2010). In particular, their behaviour is consistent with that observed in lab experiments for the coinflip game (Suri et al., 2011), the trust game and ultimatum game (Amir et al., 2012).

One criticism often levied against MTurk is that one does not know where people on MTurk come from (Krupnikov and Levine, 2014), and that it is easy for them to misreport their nationality. However, while this was a significant problem several years ago, it is no longer the case. Indeed, Amazon made some drastic changes to its account verification procedure in 2013, which led to major shifts in the composition of the workforce (Ipeirotis, 2013). Indeed, it disabled the majority of non-US accounts, and implemented a much stricter account verification procedure, which required both new and existing workers to provide their social security number, their full legal name, their real address etc. Workers who did not comply with these new rules saw their accounts deactivated.[8] Moreover, one can also exclude participants whose IP address is not in the US, which further reduces the risk of getting non-American participants.

Another concern that is often raised is that MTurk users can communicate with one another about the studies they participate in, which could compromise the internal validity of the whole experiment (Chandler et al., 2014). However, it is important to note that the main forums used by MTurk workers, such as Turkernation.com, explicitly forbid workers from discussing the answers they give, and have moderators who enforce those rules. To make sure that participants did not share any information online, I looked for posts mentioning the study on the most popular MTurk forums. The posts I found did not reveal any crucial information about the treatment, and most of them simply consisted in a general appraisal of the experiment. This indicates that communication among participants does not seem to be a big concern for this

[8]Amazon implemented this more stringent verification procedure because the IRS required them to get accurate information about their workers for tax purposes.

study.

Sample Characteristics

The experiment was conducted on Mechanical Turk in two waves: the first one on the 11th-12th March 2014, and the second one on the 5th September 2017.[9] In total, 193 people successfully participated in the first wave, and 584 in the second one. 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 a thousand tasks on MTurk for the first wave, and 500 tasks for the second wave. These restrictions are important in order to get high-quality data, as demonstrated by Peer et al. (2014). For each wave, the data collection process took less than two hours, which minimised the risk of participants colluding or obtaining information about the different treatments.[10]

The attrition rate was less than one percent, which is very low for online experiments. This can be explained by two facts. First, the average wage per participant was above $16.50 per hour, which is significantly higher than the average wage on MTurk, which is only $4.80 according to Mason and Suri (2012). Second, participants found the experiment engaging, based on the feedback they provided. Nobody tried to complete the experiment more than once.

[9]The second wave was conducted in order to increase statistical power, and to carry out the heterogeneity analysis. Note that the experimental instructions were identical in both waves. In the statistical analysis, I include wave fixed effects, which do not alter the results.

[10]The first wave was conducted in two phases: on the 12th March 2014, 100 observations were collected in the span of one hour, and approximately the same number of surveys were completed the following day in less than two hours.

As shown in Table 1.1, 57% of the sample is male. The median age is 35 years, while the median age in the US is 38 (CIA, 2015). Moreover, the median personal income of the sample is between $30,000 and $34,999, which is very similar to the median personal income for the US as a whole ($31,099 in 2016). 54 percent of our sample has at least a bachelor's degree, which is significantly higher than the national average of 33 percent (Census Bureau, 2015). Furthermore, 10 percent of the MTurk sample is unemployed, whereas the unemployment rate for the US is closer to 5 percent (Bureau of Labor Statistics, 2015). Overall, the sample is fairly representative of the US population in terms of income and age, but the level of education and the unemployment rate of respondents tend to be higher than the national average.

It is also important to note that the characteristics of the participants are very similar on average in the first wave and in the second one. For instance, 42% of the sample is female in the first wave, while the proportion of women in the second wave is only one percentage point higher. Similarly, 54% of people in the first wave identify as liberal, while the percentage of liberals in the second sample is only 3 percentage points higher. A formal test of the differences between the two samples indicates that most characteristics are balanced in the two waves, although there are a few statistically significant differences in terms of income and employment status.[11] For the remainder of the analysis, I will only present the results for the full sample.[12]

[11] These results are available upon request.

[12] The results will be shown without and with controls, which include wave fixed effects.

Table 1.1: Summary Statistics

	Full Sample	1st Wave	2d Wave
	Average	Average	Average
Female	0.43	0.42	0.43
Year of birth	1979	1979	1980
Income between \$0 and \$19,999	0.29	0.29	0.29
Income between \$20,000 and \$39,999	0.27	0.24	0.30
Income between \$40,000 and \$59,999	0.22	0.24	0.21
Income above \$60,000	0.21	0.23	0.20
Liberal	0.56	0.54	0.57
Conservative	0.26	0.27	0.26
Graduated high school or less	0.11	0.09	0.12
Some college, no degree	0.23	0.22	0.24
Associate degree	0.12	0.12	0.12
At least bachelor's degree	0.54	0.56	0.52
Employed full-time	0.64	0.62	0.65
Employed part-time	0.14	0.14	0.15
Unemployed and looking for a job	0.10	0.11	0.09
Unemployed but not looking for a job	0.05	0.06	0.04
Retired or other	0.07	0.07	0.07

Note: All the variables are dummy variables, except for "Year of birth".

Randomisation Check

Table 1.2 shows to what extent the control group and the treatment group differ in terms of observable characteristics for the whole sample, the first wave, and the second wave. Overall, the full sample is well-balanced, and there are no imbalances at the five percent significance level. The F-test for the regression of the treatment dummy on all of the control variables also shows that there are no major concerns with imbalances between the control in the treatment group. This claim is also supported by the fact that the attrition rate was particularly low in the experiment, which minimises any risks of differential attrition. It is therefore legitimate to consider that the random assignment to the different treatments worked as expected.

Table 1.2: Balance Table

	Full Sample			1st Wave			2d Wave		
	Treatment	Control	P-value	Treatment	Control	P-value	Treatment	Control	P-value
Female	0.43	0.42	0.76	0.37	0.43	0.34	0.45	0.42	0.37
Year of birth	1980	1979	0.09	1980	1976	0.02	1980	1979	0.53
Personal income	35981	37115	0.50	27602	31753	0.21	38656	38800	0.94
Social class	41	41	0.81	36	41	0.08	43	42	0.52
Financial satisfaction	2.88	2.88	0.97	2.72	2.85	0.44	2.93	2.89	0.71
Degree	0.52	0.56	0.19	0.46	0.54	0.22	0.54	0.57	0.42
Liberal	0.56	0.54	0.45	0.58	0.57	0.79	0.56	0.53	0.47
Employed full-time	0.65	0.62	0.36	0.55	0.53	0.76	0.68	0.65	0.38
Joint F-test			0.62			0.31			0.72

Note: The p-values are computed using robust standard errors.

1.4.2 Overview of the Data

Analysis of the Pooled Data

In order to have a general impression of the participants' behaviour, it is useful to look first at the pooled data, without distinguishing between the various treatments. The following plot shows in percentages how much each participant would take from a fellow group member who earned the same income as them.

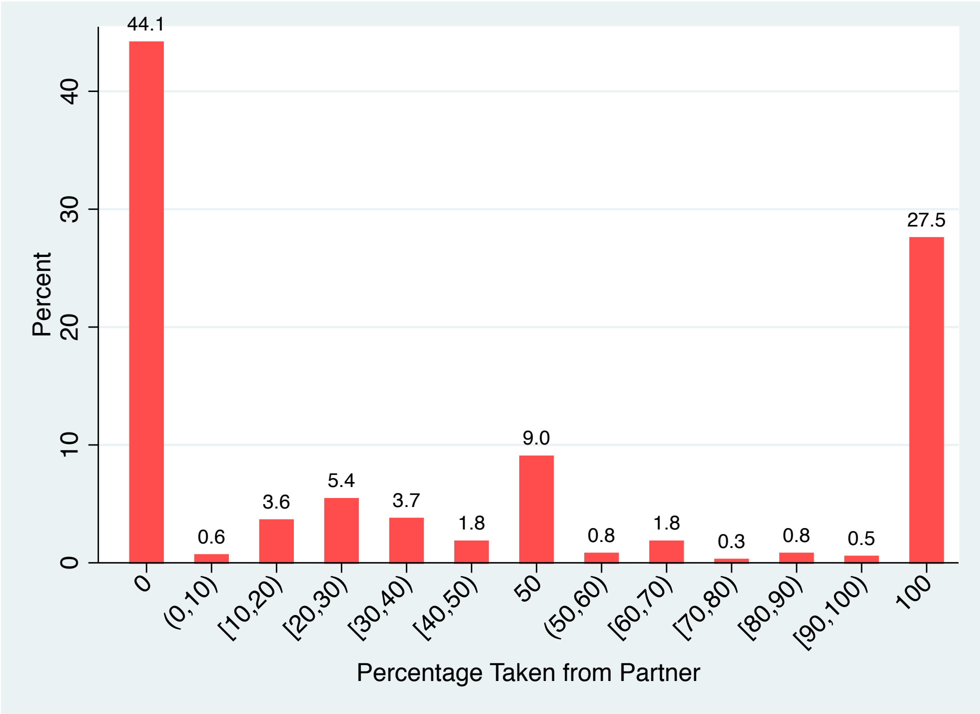

Figure 1.1: Percentage taken from a fellow group member who earned the same income

It appears that slightly more than 44% of participants chose not to take anything from their partner, while around 28% decided to steal everything they could. Approximately 9% of the people opted to take half of their partner's earnings. It is also interesting to note that 78% of those who decided to take some but not all of their partner's income chose to steal less than fifty percent of the other person's earnings. On average, people took 38.5% of their partner's income, while the median value is of 20%. What is striking about these results is how polarised they are: 72 percent of the sample chose either to take nothing or to take everything.

However, people's opinions on what would constitute a fair amount to steal from someone with the same income as them are much more homogenous. Indeed, around 60% of participants think that the only fair option is to take nothing, while a mere 4.5% contend that it is fair to steal everything. Finally, around 12% of people would consider it fair to take half of their partner's income.

So, although only 4.5% of people would deem it fair to take all of their partner's income, 28% of the sample chose nevertheless to do it. Moreover, 34% of the participants decided to take at least part of their partner's income even though they later indicated that the fair thing to do would have been not to steal anything. Finally, 38% of the total sample chose to steal more than the fair amount that they had indicated. This shows that a large proportion of the participants chose to act in a way that did not conform to their ethical standards.

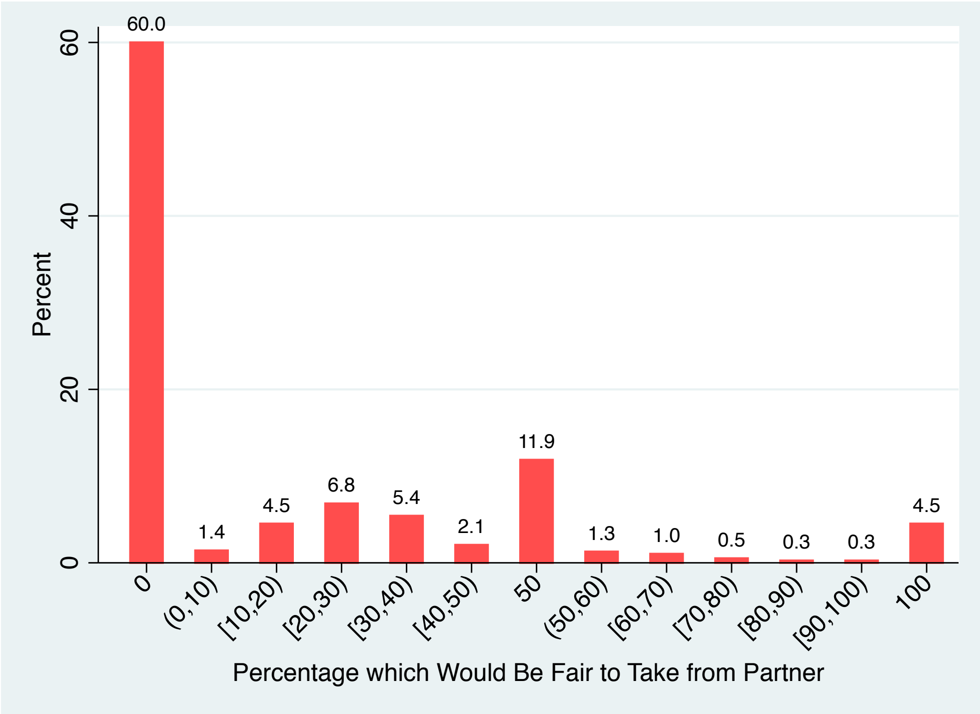

Figure 1.2: Percentage which would be fair to take from a fellow group member who earned the same income

1.4.3 Manipulation Checks

It appears very clearly in Table 1.3 that people in the inequality groups are less satisfied with the experiment as a whole, and that they consider that they have not been paid fairly compared to the participants in the equality groups. The effect are large in magnitude and they are all statistically significant at the one percent level, irrespective of whether controls are included or not.

Table 1.3: Manipulation Checks

	(1)	(2)	(3)	(4)
	Satisfaction with the experiment	Satisfaction with the experiment	Paid fairly	Paid fairly
Inequality treatment	-0.218***	-0.246***	-0.345***	-0.348***
	(0.0805)	(0.0816)	(0.0529)	(0.0538)
Constant	5.949***	-2.981	3.640***	13.63***
	(0.0564)	(8.394)	(0.0306)	(5.239)
Observations	777	760	777	760
Controls	No	Yes	No	Yes
Sample	Full Sample	Full Sample	Full Sample	Full Sample

Note: The estimating equation is: $y_i = \pi_0 + \pi_1 Treatment_i + (\Pi^T \mathbf{X_i}) + \varepsilon_i$, where y_i is the outcome variable, $Treatment_i$ is an indicator variable equal to 1 if the participant is in an inequality group, and $\mathbf{X_i}$ is a set of control variables. The control variables included in the regressions are gender, year of birth, perceived social class, individual income, employment status, educational level, political orientation, and a dummy indicating the wave of the experiment. Robust standard errors are in parentheses. Significance levels: * 10%, ** 5%, *** 1%

It is also insightful to examine how people's perception of the experiment varied depending on the amount that they received for completing the task. As shown in Table 1.4, participants in the inequality groups who only earned $1 are the least satisfied with the experiment, and they are much more likely to state that they were paid unfairly than people in $1-equality groups.

People in $3- and $5-inequality groups are also more inclined to say that they were not paid fairly compared to their counterparts in $3- and $5-inequality groups, and this difference is statistically significant at the 5 percent level. However, it is interesting to note that their overall satisfaction level with the experiment is not statistically different from that of people in $3- and $5-equality groups.

Table 1.4: Manipulation Checks for all Income Levels

	Satisfaction with the experiment	Paid fairly
Panel A: People who Earned $1:		
Inequality treatment	-0.476***	-0.727***
	(0.157)	(0.104)
Constant	5.767***	3.542***
	(0.105)	(0.0554)
Observations	244	244
Controls	No	No
Sample	Full Sample	Full Sample
Panel B: People who Earned $3:		
Inequality treatment	-0.135	-0.200**
	(0.125)	(0.0821)
Constant	5.985***	3.606***
	(0.0847)	(0.0535)
Observations	265	265
Controls	No	No
Sample	Full Sample	Full Sample
Panel C: People who Earned $5:		
Inequality treatment	-0.107	-0.188**
	(0.132)	(0.0760)
Constant	6.094***	3.778***
	(0.103)	(0.0472)
Observations	268	268
Controls	No	No
Sample	Full Sample	Full Sample

Note: Robust standard errors are in parentheses. Significance levels: * 10%, ** 5%, *** 1%

1.4.4 Main Hypothesis

In the theoretical section, I showed that Becker's model of crime and Fehr and Schmidt's inequity aversion model make very different predictions. More specifically, the Fehr-Schmidt model predicts that people in the inequality groups will steal more from their partners than people in the equality groups, while the Becker model implies that there will not be any differences in behaviour between the two groups. However, as the following histogram indicates, this is not what is observed in the data. Indeed, the percentage of people stealing nothing is *higher* in the inequality groups than in the equality groups. Furthermore, there are *fewer* people stealing everything in the inequality groups than in the equality ones.

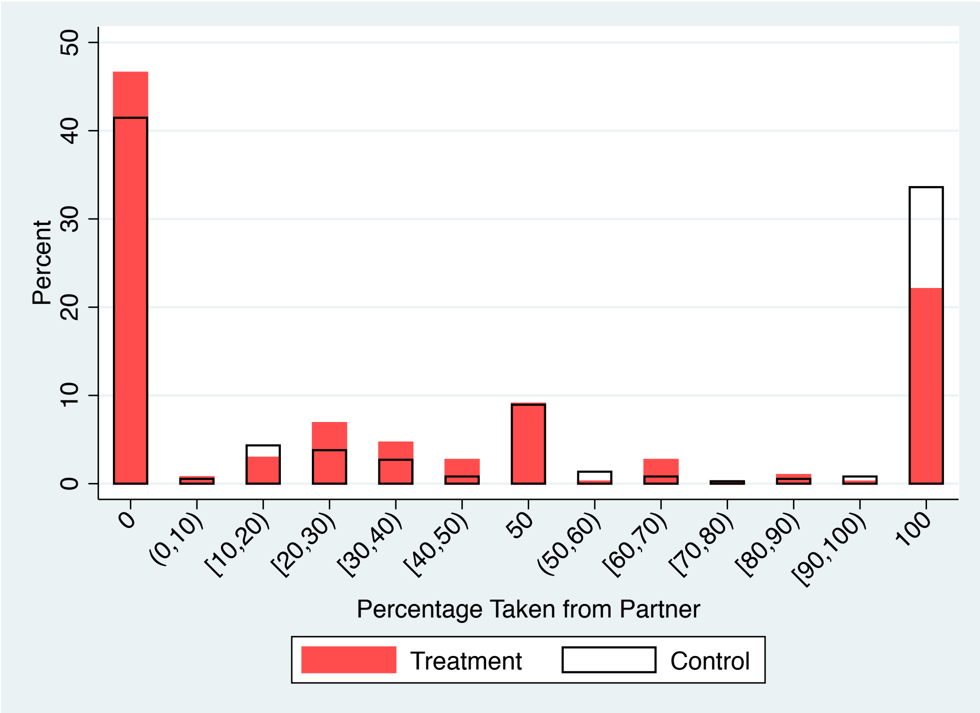

Figure 1.3: Percentage taken from a fellow group member who earned the same income, by treatment

This observation is confirmed in Table 1.5, which shows the results of regressing the amount of money taken by participants from a group member with the same income (in percent) on the treatment dummy. Specifically, I estimate the following equation:

$$y_i = \pi_0 + \pi_1 Treatment_i + (\Pi^T \mathbf{X_i}) + \varepsilon_i$$

where y_i is the outcome variable, $Treatment_i$ is an indicator variable equal to 1 if the participant is in an inequality group, and $\mathbf{X_i}$ is a set of control variables.[13] The average proportion that people in the equality groups take from their partners is 43.4%, while it is only 34.1% for people in the inequality groups, and this difference is highly statistically significance, whether control variables are included or not. This is particularly striking because the direction of the effect is the opposite of what was predicted by the models presented in the theoretical section.

Table 1.5: Main Regression

	(1)	(2)
	Percent stolen	Percent stolen
Inequality treatment	-9.309***	-10.05***
	(3.052)	(3.000)
Constant	43.41***	-773.8**
	(2.313)	(306.8)
Observations	777	760
Controls	No	Yes
Sample	Full Sample	Full Sample

Note: The control variables are the same as the ones described in Table 1.3. Robust standard errors are in parentheses. Significance levels: * 10%, ** 5%, *** 1%

Moreover, this pattern can be observed at each income level, as is demonstrated

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

by Table 1.6 and in Figure 1.4. For instance, participants in \$3-inequality groups took on average 35.1 percent of their partner's income, whereas people in \$3-inequality groups stole 11 percentage points more from their partner. This difference is large and is statistically significant at the 5 percent level, whether I include controls or not. The treatment effect goes in the same direction for people who earned \$1, and is significant at the 5 percent level when controls are included in the regression. Similarly, people in \$5-inequality groups tend to steal less money from their partner than people in the corresponding equality groups, although this difference is only significant at the 10-percent level.

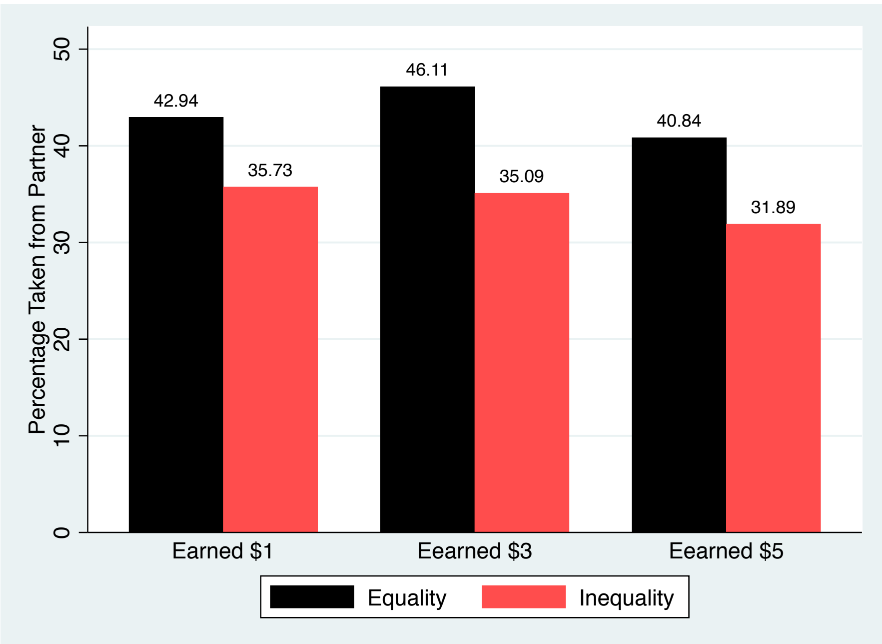

Figure 1.4: Percentage taken from a fellow group member who earned the same income, by treatment and by income category

Table 1.6: Main Regression for all Income Levels

	Percent stolen	Percent stolen
Panel A: People who Earned $1:		
Inequality treatment	-7.216	-13.54**
	(5.542)	(5.691)
Constant	42.94***	-2181.7***
	(4.063)	(536.5)
Observations	244	235
Controls	No	Yes
Sample	Full Sample	Full Sample
Panel B: People who Earned $3:		
Inequality treatment	-11.02**	-10.60**
	(5.123)	(5.233)
Constant	46.11***	-468.3
	(3.784)	(542.5)
Observations	265	261
Controls	No	Yes
Sample	Full Sample	Full Sample
Panel C: People who Earned $5:		
Inequality treatment	-8.944*	-10.41*
	(5.289)	(5.596)
Constant	40.84***	-2.081
	(4.215)	(552.2)
Observations	268	264
Controls	No	Yes
Sample	Full Sample	Full Sample

Note: The control variables are the same as the ones described in Table 1.3. Robust standard errors are in parentheses. Significance levels: * 10%, ** 5%, *** 1%

1.4.5 Heterogeneity

For all of the heterogeneity analysis, I estimate the following equation, where $Inter_i$ refers to the interacted variable, and X_i is a vector of pre-determined characteristics:

$$y_i = \pi_0 + \pi_1 Treatment_i \times Inter_i + \pi_2 Treatment_i + \pi_3 Inter_i + (\Pi^T \mathbf{X_i}) + \varepsilon_i$$

Gender

As shown in panel A of Table 1.7, women in equality groups steal significantly less from a partner with the same income as them than men in equality groups. While the average proportion stolen by women in equality groups is 32.63 percent, it is 51.49 percent for men in equality groups. However, it is interesting to note that men and women react very differently to the inequality treatment. Indeed, it is primarily men who react strongly to the treatment and who reduce their stealing when they are faced with inequality. Men in the inequality groups steal on average 15.5 percentage points less from their partner than men in the equality groups, whereas the treatment effect for women is indistinguishable from zero. This effect is statistically significant at the 5 percent level, irrespective of whether control variables are added or not.

Social Class

By comparing the behaviour of people who are above the median perceived position in the income distribution and those who are below it, it can be shown that there is a strong heterogeneous treatment effect by social class. Indeed, as shown in panel B of Table 1.7, people who consider themselves relatively less well-off steal less money from their partner in the inequality groups than in the equality groups, whereas there is no real difference in behaviour for people who think that they are in the top half of the social ladder. The heterogeneous treatment effect is significant at the 5 percent level, once pre-determined characteristics are controlled for, and is otherwise significant at the 10 percent level.

Age

In panel C of Table 1.7, it also appears that younger and older people react very differently to the inequality treatment. While young people decide to take less money from their partner in the inequality groups compared to the equality group, older people take the same amount on average from their partner whether they are in an equality group or in an inequality group. This difference in behaviour between the younger and the older generation is significant at the 1 percent level, whether I include control variables or not.

Table 1.7: Heterogeneity

	Percent stolen	Percent stolen
Panel A: Female:		
Inequality treatment=1	-15.47***	-16.82***
	(4.126)	(4.093)
female=1	-18.86***	-15.66***
	(4.552)	(4.576)
Inequality treatment=1 × female=1	14.79**	15.76***
	(6.064)	(6.004)
Constant	51.49***	-791.5***
	(3.118)	(304.8)
Observations	770	757
Controls	No	Yes
Panel B: Social Class:		
Inequality treatment=1	-15.05***	-17.11***
	(4.388)	(4.276)
Above median class=1	-6.856	-13.25**
	(4.622)	(6.227)
Inequality treatment=1 × Above median class=1	11.45*	14.06**
	(6.100)	(6.022)
Constant	46.90***	-846.1***
	(3.366)	(310.7)
Observations	777	757
Controls	No	Yes
Panel C: Age:		
Inequality treatment=1	-17.73***	-18.46***
	(4.159)	(4.136)
Above median age=1	-21.28***	-21.01***
	(4.491)	(4.596)
Inequality treatment=1 × Above median age=1	17.39***	18.65***
	(6.017)	(6.002)
Constant	53.56***	75.40***
	(3.180)	(16.11)
Observations	777	757
Controls	No	Yes

Note: The set of control variables is gender, year of birth, perceived social class, individual income, employment status, educational level, political orientation, and a dummy indicating the wave of the experiment. In each panel, all the control variables are included, except the one which is interacted with the treatment variable. Robust standard errors are in parentheses. Significance levels: * 10%, ** 5%, *** 1%

1.4.6 Analysis of the Inequality Groups

The Becker Model and the Fehr-Schmidt Model

It is important to first examine what predictions the Becker model and the Fehr-Schmidt models make regarding the behaviour of participants in inequality groups who are matched with people who earned either more or less than them.

The Becker Model

In the inequality groups, the Becker model predicts that participants will tend to take more money from richer people than from poorer people. Indeed, the benefit of taking money is higher when person B is richer, given that the maximum amount which can be taken is higher. Moreover, it is likely that the moral cost of stealing is lower when person B is richer, because many people would consider it less morally wrong to take money from someone who already has a high income than to steal money from a poor person. This feature of the cost function also implies that participants will take more money from richer group members than from poorer ones.

Table 1.8 clearly shows that this prediction is borne out in the data. For instance, the proportion of people with \$3 who do not steal anything is 60% when their partner earned \$1 but only 26% when their partner earned \$5. Similarly, 49% of people whose income is \$1 refrain from taking anything when their partner also earned \$1, but this percentage drops to 10% when their partner earned \$5 in the first stage.

Table 1.8: Proportion of People in the Inequality Groups Taking Nothing from their Partner

	Partner B with \$1	Partner B with \$3	Partner B with \$5
Person A with \$1	49%	13%	10%
Person A with \$3	60%	45%	26%
Person A with \$5	66%	46%	46%

The Fehr-Schmidt Model

The Fehr-Schmidt model predicts that participants will always try to reach the top of the income distribution, and that they will want to take from their partner at least enough money to achieve this goal.[14] If reaching the top of the income distribution does not require taking all of their partner's income, they will decide whether to take the rest based on their aversion for advantageous inequality. If they dislike it very much ($\beta > 5/6$), they will not take more than this, but if they do not mind advantageous inequality that much ($\beta \leq 5/6$), they will take everything from their partner. These predictions are summarised in Tables 1.9 and 1.10.

Table 1.9: Optimal Behaviour of People in the Inequality Groups if $\beta \leq 5/6$

	Partner B with \$1	Partner B with \$3	Partner B with \$5
Person A with \$1	$s^* = 1$	$s^* = 3$	$s^* = 5$
Person A with \$3	$s^* = 1$	$s^* = 3$	$s^* = 5$
Person A with \$5	$s^* = 1$	$s^* = 3$	$s^* = 5$

[14]Obviously, they cannot take more than their partner's income, which might not be enough to reach the top of the income distribution.

Table 1.10: Optimal Behaviour of People in the Inequality Groups if $\beta > 5/6$

	Partner B with \$1	Partner B with \$3	Partner B with \$5
Person A with \$1	$s^* = 1$	$s^* = 3$	$s^* = 4$
Person A with \$3	$s^* = 1$	$s^* = 2$	$s^* = 2$
Person A with \$5	$s^* = 0$	$s^* = 0$	$s^* = 0$

For people who earned \$5 in the first stage, these predictions are partly consistent with the experimental data, since 60% of them decided to take either everything or nothing from their partner, no matter whether person B earned \$1, \$3, or \$5. However, the Fehr-Schmidt model fares badly for the other income levels. Indeed, it can only explain 17% of the behaviour of people who earned \$3, and 23% of the behaviour of people who earned \$1.

Types of Participants in the Inequality Groups

We can also use the behaviour of participants in the inequality groups to infer the rules that participants might use to decide how much money to take from their partner.

For instance, people who chose not to take any money from someone with the same income as them could have different reasons for doing so. Some of them might just oppose the idea of taking money from someone else, even if that person had earned more money than them. On the other hand, some people might view the option of taking some money from some richer person as a way of creating a fairer distribution of resources. In that case, they will take up to half of the difference in income between themselves and a richer partner, but nothing from people who earned less or as much money as them. People taking more than half of the difference in income between them and their partner cannot claim to be redistributing earnings fairly, because they would just be creating inequality that would favour them.

In all the inequality groups, 27.45% of people stated that they would not take anything from their partner, no matter how much money their partner earned in the first stage. However, it would be a mistake to assume that this implies that a quarter of the participants oppose the appropriation of someone else's earnings in all circumstances. Indeed, when the participants' income is \$5, it is impossible to distinguish people who are concerned about redistribution and those who oppose appropriation, because both types of participants would not take any money from any of their potential partners.

However, for the lower income levels, it is possible to distinguish between the two types of people. As shown in Table 1.11, 17.90% of those who did not earn \$5 in the first stage chose not to take any money from any of the other participants, which indicates that they oppose any form of appropriation of money in the context of the experiment. On the other hand, 40.08% of those who did not earn \$5 in the first stage performed some form of income redistribution: they did not take anything from people who had earned less or as much as them, and took no more than half of the difference in income between them and their richer partner. Of those 40.08%, 41.7% chose to take exactly half of the difference in income between them and richer group members.

When considering the whole sample of people in the inequality groups, these two types can explain the behaviour of 57.98% of participants. People who maximise their profits represent another 18.87% of the total sample. It is therefore possible to account for 76.85% of the data with only three types of behaviour. Moreover, 4.41% of people chose to take exactly half of the amount earned by their partner, irrespective of whether their partner had earned \$1, \$3 or \$5.

Table 1.11: Types of Participants

Type	Proportion
Always takes everything	18.87
Never takes anything	17.90
Redistributes the money	40.08
Takes half	4.41
Total explained	81.26

1.5 Alternative Explanations

One of the main results from this experiment is that people in the inequality groups tend to steal less from someone in their group with the same income as them than people in the equality groups. This finding is somewhat surprising because it is at odds with the predictions made by the models presented in the theoretical section. However, I will now show that this result can be rationalised by modifying one of the assumptions of the Becker model. I will also provide alternative explanations for why participants in the inequality groups are less likely to steal from their partners than people in the equality groups.

1.5.1 Main Prediction

The basic Becker model predicted that there should not be any discernible differences between the two kinds of groups, which is clearly not the case empirically. However, one important assumption was that the moral cost of stealing was identical for people in equality and inequality groups. There are a few reasons why this assumption might not be warranted, and why one might expect the cost of stealing to be higher for people in the inequality groups than in the equality ones.

Indeed, one could argue that the participants in the inequality groups were more aware than people in the equality groups that it would be particularly unfair to steal money from someone with the same income as them. They themselves had suffered from the injustice of the payment mechanism, and they might not have been willing to add to this injustice by stealing money themselves.

There is some clear evidence that they resented being paid different amounts for doing the same task. For one, they were significantly less satisfied than people in the equality groups about the experiment in general. When asked to rank their satisfaction with the experiment from 1 (Very dissatisfied) to 7 (Very satisfied), people in the inequality groups reported a satisfaction of 5.73 on average, whereas it was of 5.95 for participants in the equality groups. A

Mann-Whitney-Wilcoxon test shows that the difference in the distributions is highly significant (p-value below 0.005).

People in the inequality groups also viewed the experiment as more unfair than their counterparts in the equality groups. Indeed, the fairness level reported by participants in the equality groups was 3.64, whereas it was only of 3.29 in the inequality groups. A Mann-Whitney-Wilcoxon test confirms that the difference between the groups is highly significant (p-value of less than 0.0001). Moreover, only 4 percent of people in equality groups said that the experiment was unfair or slightly unfair, while in inequality group, the proportion reached 16 percent.

It is therefore clear that many participants in the inequality groups felt that they had not been treated in a fair way in the first stage of the experiment. Seeing what effects this type of injustice could have on them, they might have been more aware of the effects that stealing would have on others. People in the equality groups, on the other hand, were not treated unfairly in the first stage. So, when came the moment to decide whether to steal from their partner, they were not particularly sensitive to the effects their actions could have on others. Another way of putting it is that the moral cost of stealing was higher for people in the inequality groups than in the equality groups.

This analysis therefore shows that, if one abandons the assumption that the moral cost of stealing is the same for participants in both equality and inequality groups, it is possible to explain why people in the inequality groups tend to steal less often than people in the equality groups. Indeed, there are good reasons to believe that the cost of stealing could have been higher for the participants in the inequality groups than in the equality groups.

1.5.2 Other Explanations

The modified Becker model gives a global explanation for why people in the inequality groups are less likely to steal from someone with the same income as them than people in the inequality groups. However, a different approach would be to focus on each income level ($1, $3 and $5), and see why people in the inequality groups might have chosen to steal less at each of these income levels. Indeed, the reasons why someone with $1 might refrain from stealing from someone else with $1 could be quite different from the reasons why someone with $5 might choose not to steal from someone else with $5.

For instance, people in the inequality groups who had received only $1 might have been more inclined to think that it would be particularly wrong to steal money from someone who had earned so little compared to the others. Participants in the $1-equality groups all received the same income, and stealing money from someone with an income of $1 no longer felt like stealing from the poorest member of the group. Therefore, it would have been psychologically less costly to take money from someone with $1 in the equality groups than in the inequality groups.

On the other hand, people who had earned $3 or $5 in the inequality groups might have the impression that they were particularly fortunate not to have received only $1 for performing the task, and therefore they felt that they could be more magnanimous towards others, including people who earned the same amount as them. People in the equality group did not feel particularly relieved that they did not earn $1, because that was never an option, and they therefore did not feel the need to be kinder towards fellow group members.

Another explanation would be that the strategy method actually changed the way the second stage was viewed by people in the inequality groups. Indeed, in the inequality groups, participants with $3 or $1 knew that there was a possibility that they could take some money from someone who had earned $5. Having the option of taking money from richer partners could have been seen as a way of making the income distribution fairer. As long as people did not take more than half of the difference in income between them and their richer partner, they could rightly view their action as a way to achieve some form of redistribution. However, if people started thinking about redistribution when they were asked how much they would take from richer participants, then they would have been less inclined to take money from someone who had earned the same amount as them, because it would have been impossible to justify it on redistributive grounds.

On the other hand, people in the equality groups did not have the option to make the distribution of income fairer, and therefore they could not have taken such considerations into account when making their decision. One could therefore argue that the framing of the experiment was not identical for inequality and equality groups, which could have led to the phenomenon observed in the experiment.

1.6 Conclusion

The main objective of this paper was to examine experimentally how relative concerns affected people's moral behaviour, and more specifically, whether disadvantageous inequality would make people more likely to break certain ethical norms. The experiment itself can be summarised as follows. Participants are first assigned to groups of six people, which can be of two different kinds: equality groups, where everyone is paid the same amount for performing a simple task for 10 minutes, and inequality groups, where people receive different amounts for completing the same task.

Each participant is then told that one member of their group (referred to as Person A) will have the possibility of taking some of the money earned by another group member (Person B). The probability of being picked to be either person A or person B is the same for all of the members of the group. All of them must therefore indicate how much they would take from a fellow group member, if they were chosen to be Person A.

Based on this data, it is possible to see whether there are significant differences in behaviour between the participants in the inequality and the equality groups. Whereas theoretical models, such as the Becker model and the Fehr-Schmidt inequity aversion model, predict that participants in the inequality groups should steal at least as much as the ones in the equality groups, the opposite actually occurred in the experiment. Indeed, participants in the inequality groups stole significantly less often from a fellow group member with the same income as them than people in the equality groups. I show however that it is possible to rationalise this behaviour by changing one of the assumptions made by the Becker model.

This paper contributes to the experimental literature in two ways. It is the first study which examines experimentally whether the presence of income inequality makes people more willing to steal from other participants. Indeed, whereas previous research had focused on exploring people's stealing behaviour when there are endowment/income differences, this experiment can be used to contrast the behaviour of participants in inequality groups with the behaviour of participants in equality groups.

Moreover, the experimental design improves on the one commonly used in the literature to study the impact of endowment/income inequality on behaviour. Indeed, whereas previous experiments only kept the average income constant between equality and inequality groups, my experimental design keeps the income levels constant between the two kinds of groups.

Overall, the results from this experiment show that people who face injustice may be unwilling to take advantage of other people in the same position as them, because of a sense of solidarity with them. This finding opens up a new line of research, whose objective would be to better understand under which circumstances solidarity can emerge between people, and whether this phenomenon would occur in other social situations.

Chapter 2

How Does Economic Status Affect Pro-Social Behaviour? Representative and Experimental Evidence from the US[1]

2.1 Introduction

It is a well-established fact that people care about their economic status (Duesenberry, 1949; Frank, 1985; Veblen, 1899). Indeed, many people would prefer to live in a society where their relative income is high but their absolute income is low, rather than in a society where the opposite is true (Solnick and

[1]This chapter was written with Christopher Roth.

Hemenway, 1998). Changes in relative income affect not only people's well-being (Easterlin, 1974; Luttmer, 2005; Clark and Oswald, 1996), but also their consumption (Kuhn et al., 2011), their behaviour in the labour market (Card et al., 2012b) and their redistributive preferences (Cruces et al., 2013; Karadja et al., 2014).

In order to understand people's behaviour in a social context, it is crucial to understand how social comparisons affect their behaviour towards others (Fehr and Schmidt, 1999). More specifically, it is important to examine whether pro-social behaviour is endogenous to economic circumstances and whether people's relative economic position affects their pro-social behaviour. In this paper, we present the results from an online experiment with a representative sample of the US, in which we exogenously vary people's perceived economic status before measuring their social preferences.

The experiment is structured as follows. First, participants are asked to specify how much their household earned before taxes in 2015, and where they think their household stood in the income distribution in 2015. We then use US Census data to calculate people's actual position in the income distribution. Half of the people who over-estimated their position in the income distribution are told that they are relatively poorer than they thought, and that they are closer to the bottom of the income distribution. Conversely, half of those who under-estimated their position in the income distribution are informed that they are relatively richer than they thought, and that they are closer to the top of the income distribution. Then, all of the participants play four incentivized economic games, which measure different social preferences, namely trust, negative reciprocity, honesty and pro-sociality.

Since people who under-estimate their position in the income distribution are different from people who over-estimate it, we conduct the empirical analysis separately for the two groups. Specifically, we compare the behaviour of under-estimators who learn that they are relatively richer than they thought with the behaviour of under-estimators who are not told anything. Conversely, we evaluate whether over-estimators who receive the treatment behave differently from over-estimators who do not get the treatment.

We show that individuals who learn that they are relatively poorer than they thought do not change their behaviour in any of the four games, although they do report feeling less satisfied with their position in the income distribution. Conversely, people who learn that they are relatively richer report being more satisfied with their relative standing in the income distribution, but this does not translate into a change in behaviour in any of the four games. These results imply that changes in perceived relative income may not play such an important role in determining people's pro-social behaviour.

We contribute to the nascent literature on relative income and social preferences. This literature finds in some instances a positive correlation between status and pro-sociality (Korndörfer et al., 2015) and in others a negative correlation (Piff et al., 2012; Almås et al., 2015). For instance, Piff et al. (2012) conduct a priming experiment, in which participants were asked either to compare themselves to the richest people in the US, or to the poorest people. Then, participants were shown a jar full of candy, and they were told that it was for children in a neighbouring laboratory, but that they could take some if they wanted. Piff et al. (2012) argue that the self-reported number of candy taken from the jar is a good measure of unethical behaviour, and they show that participants who were primed to feel richer report taking more candy from the jar than participants who were primed to feel poorer. Unfortunately, they are not able to disentangle whether their effect comes from people feeling richer or from people feeling poorer, since they lack a pure control group.

Similarly, Côté et al. (2015) provide evidence that high income inequality increases rich people's selfishness relative to poor people. They show that if people are primed on the high inequality in their state rich people behave more selfishly than poor people in a real-stakes giving experiment. Just like Piff et al. (2012) they cannot separately pin down whether their result is driven by feelings of poverty or feelings of affluence.

All in all, the existing literature is limited by the fact that it is mostly correlational and that many outcomes are self-reported. By combining an experimental variation in social class with a behavioral outcome measure, our study fills this gap in the literature. Self-reported measures could be particularly problematic if social class affects people's propensity to lie when there is no incentive to tell the truth. To circumvent this issue, we only use incentivised behavioural measures of social preferences in our experiment.

Furthermore, most of the existing literature simply notes that rich people behave less pro-socially than poor people, but they do not examine how positive or negative shocks to social status affect social preferences. We, on the other hand, are able to analyse the effects of positive and negative shocks separately. Our study is also the first one to examine the effects of economic status on negative reciprocity.

We also add to the literature on absolute income shocks and social preferences. Indeed, an absolute income shock often also implies a relative income shock, and it is usually impossible to disentangle the effect of the absolute income shock from that of the relative income shock. Fisman et al. (2015) examine the effect of the recent financial crisis on pro-sociality by conducting the same social preference experiment before the crisis, and after the crisis. They show that students who took part in the study after the crisis behave more selfishly than those who participated before the crisis. Setting aside the identification issues they face, they cannot say how much of their effect is driven by absolute income shocks as opposed to relative income shocks. Our evidence suggests that relative income shocks might play a lesser role in determining pro-social behaviour than shocks to absolute income.

Furthermore, we contribute to the literature on random survey experiments, especially those focusing on income comparisons and relative income. For example, Cruces et al. (2013) conduct a large survey experiment where they tell households in Argentina their true position in the income distribution and show that it affects their preferences for redistribution. Specifically, people who learn that they are relatively poorer than they thought become more supportive of redistribution, although they see no effect on redistributive preferences for households who learn that they are relatively richer than they thought. On the other hand, Karadja et al. (2014) show that Swedish households who learn that they are higher in the income distribution than they thought demand less redistribution, and become more supportive of the conservative party. Similarly, Cassar and Klein (2015) manipulate respondents' relative earnings and show that it affects their preferences for redistribution with an incentivized task. In particular, they find that individuals with lower relative earnings are more likely to redistribute money from participants with higher payoffs to participants with lower payoffs. Our evidence complements these studies, by showing that pro-social behaviour does not appear to be affected by changes in people's perceived position in the income distribution.

The paper is structured as follows. In section 2.2, we outline the experimental design. In section 2.3, we describe the data collection process and give an overview of the data. In section 2.4, we present the empirical results. In section 2.5, we examine potential mechanisms which could explain our findings. Section 2.6 concludes.

2.2 Experimental Design

The participants are first asked how much their household earned in 2015. We then ask them where they think their household stood in the income distribution. The exact question they answered was: "According to the 2015 American Population Survey, what percentage of US households earned less than your household?" If participants correctly guess the percentage (within three percentage points), they receive a bonus payment of 10 cents.

Using Census data on the US household income distribution, we can determine whether participants accurately evaluated their position in the income distribution. This allows us to give participants in the treatment group some information on their actual standing in the income distribution. Subjects in the control group, on the other hand, do not receive any information.

We divide participants into two groups: those who over-estimate their position in the income distribution, and those who under-estimate it.[2] For clarity's sake, we call participants who over-estimate their position in the income distribution over-estimators, and participants who under-estimate their position under-estimators.

Half of the over-estimators do not receive any information about the accuracy of their estimate, while the other half receive the following message:

> *Actually, you overestimated your relative position in the income distribution. In reality, you are relatively poorer than you thought. In other words, you are closer to the bottom of the income distribution than you thought. You currently earn significantly less than what you would need to be at the position you thought you occupied.*

Similarly, half of the under-estimators do not receive any information about

[2] Very few people guessed their exact position in the income distribution, and we therefore discard those observations completely, except when mentioned otherwise.

the accuracy of their estimate, while the other half receive the following message:

> *Actually, you underestimated your relative position in the income distribution. In reality, you are relatively richer than you thought. In other words, you are closer to the top of the income distribution than you thought. You currently earn significantly more than what you would need to be at the position you thought you occupied.*

Unlike Cruces et al. (2013) and Karadja et al. (2014), we do not give participants their exact position in the income distribution, but we only tell them whether their estimate is biased upwards or downwards. Indeed, we wanted to avoid a situation where people who have the most extreme biases drive the treatment effects, just because they receive the largest relative income shocks. In many ways, people with extremely inaccurate views on their position in the income distribution are different from the average person, and it is therefore important to make sure that they are not more likely to be affected by the treatment than other participants. We address this concern by making sure that the treatment only varies by the direction of the bias, but not by its magnitude.

All participants are then asked how satisfied they are with their position in the income distribution. This question allows us to check, separately for over-estimators and under-estimators, whether the treatment has any effect on the participants' economic satisfaction. We also ask participants in the treatment group to give us a new estimate of their position in the income distribution, now that they have received some information about the accuracy of their estimate. This question enables us to see to what extent participants updated their beliefs regarding their position in the income distribution, after receiving the treatment. These two questions are used as manipulation checks in the analysis, to make sure that our information treatment changed people's perception of their relative income.

After the manipulation checks, our respondents take part in four games, which are randomly ordered. First, participants play the coinflip game (Fischbacher and Föllmi-Heusi, 2013). In this game, people have to toss a coin four times in private, and report how many times "Heads" came up. For each "Heads" that they report, they receive an extra ten cents. Participants therefore have a financial incentive to over-report the number of times "Heads" came up. We cannot detect lying at the individual level, but we can detect it at the population level, by comparing the distribution of reported outcomes with the theoretical distribution.

The coinflip game is a standard measure of honesty, which has been used in numerous studies (Abeler et al., 2014; Houser et al., 2012; Fischbacher and Föllmi-Heusi, 2013). There is also some evidence showing that this behavioural measure is externally valid. For instance, Cohn and Maréchal (2016) have found that cheating in the coinflip game significantly predicts whether students misbehave at school, while Dai et al. (2016) show that fare-dodging behaviour in public transportation is correlated with dishonesty in the lab.

Second, participants play the role of the sender in the trust game (Berg et al., 1995). In this game, there are two players, referred to as person A and person B. All of our respondents (except for one) play the role of person A. Person A and person B start with $50 each. Then, person A can choose to send some money to person B. Person B receives three times the amount sent by person A. Then person B has to choose how much money to send back to person A. Once all the responses are collected, we randomly match one participant who played the role of person A with the participant who played the role of person B, and we implement their choices. Only these two subjects are paid according to their choices. In order to understand the participants' behaviour in the trust game, we also ask them how trustworthy they think person B would be. Specifically, we ask them: "What amount do you think will Person B send back to you?"

Researchers commonly use the trust game to get a behavioural measure of trusting behaviour (Berg et al., 1995). Indeed, there is some evidence that people's behaviour in the trust game is a good predictor of their actual level of trust. For instance, Fehr et al. (2003) use representative data from Germany to show that people who send more money to their partner in the trust game also report being more trusting, while Bellemare and Kröger (2007) reproduce this result using representative data from the Netherlands.[3]

[3] Glaeser et al. (2000) argue that the first mover's behaviour is a better measure of trustworthiness than trust. It is important however to note that their sample was not representative of the general population, given that they conducted their study with students from Harvard. Moreover, Sapienza et al. (2007) argue that homogeneous samples (like Harvard undergraduates) behave differently compared to more diverse samples (like representative samples), which explains why Fehr et al. (2003) and Glaeser et al. (2000) find slightly different results. Since we use a representative sample of the U.S. population, we are confident that the trust game captures the participants' level of trust.

Third, participants play the role of the second mover in the ultimatum game (Güth et al., 1982). In this game, there are two players, referred to as person C and person D. All of our participants (except for one) play the role of person D. At the beginning of this game, person C receives \$100, while person 2 receives nothing. Then, person C has to make an offer to person D on how to split the \$100. Person 2 chooses either to accept the offer made by person C, or to refuse it. If person D refuses the offer, both players receive nothing. If person 2 accepts the offer, each player receives the amount specified in the offer. Our respondents have to specify the minimum amount that person C would have to offer them, in order for them to accept their offer. Once all the responses are collected, we randomly match one participant who played the role of person D with the participant who played the role of person C, and we implement their choices. Only these two subjects are paid according to their choices.

The ultimatum game is often used to measure negative reciprocity, which is the tendency for people to punish those who behave unfairly towards them. Fehr and Gächter (2000) argue that negative reciprocity is fundamental in explaining the way people interact with one another, and that it is a phenomenon which is observed throughout the world. Indeed, Roth et al. (1991) find that a large share of people reject low offers in the ultimatum game in a series of experiments conducted in Israel, Japan, the United States and ex-Yugoslavia. People are therefore willing to forgo some money in order to punish their partner for behaving selfishly. Furthermore, Dohmen et al. (2009) find that negative reciprocity increases the likelihood of being unemployed, which further demonstrates that negative reciprocity has some important real-life implications.

Fourth, participants play a dictator game. In this game, there are two players, whom we shall refer to as person E and person F. All of our participants (except for one) play the role of person E. At the beginning of the game, person E receives $100, while person F receives nothing. Then, person E can choose how much money to give to person F. Once everyone has completed the survey, we randomly choose one participant in our survey who played the role of E to have their choice implemented and we randomly choose one participant to play the role of F. Only these two subjects are paid according to their choices. The dictator game is the most commonly used measure of pro-sociality (Hoffman et al., 1996; Engel, 2011). Indeed, behaviour in the dictator game has been correlated with pro-social behaviour in the field (Becker et al., 2015). For instance, Franzen and Pointner (2013) show that participants who gave more money to their partner in a dictator game were more likely to return letters which contained money that was not intended for them, while Benz and Meier (2008) find that people who are more generous in a dictator game also donate more to charity outside of the lab.

At the end of the experiment, we ask participants to fill out a questionnaire containing typical demographic questions, such as gender, age, ethnicity, political orientation, household size etc. The exact instructions we use in the experiment can be found in appendix B.

2.3 Description of the Data

2.3.1 Representative Sample

The experiment was conducted through Time-Sharing Experiments for the Social Sciences (TESS), which gives access to a representative sample of the U.S. population of more than 50,000 households.[4]

To ensure that the panel is truly representative, participants are recruited using a dual sampling method, which combines the traditional random digit dialling method with an address-based technique. Approximately ten percent of the people contacted accept to join the panel. It is also important to note that people who have not been invited to join the panel are not allowed to become part of the panel. This selection method makes the TESS sample one of the most representative samples of the US population available to researchers, and it has been used for numerous academic studies, such as Allcott and Taubinsky (2015); Fong and Luttmer (2009); Rabin and Weizsäcker (2009); Allcott (2013).

One of the main advantages of TESS is that the pool of participants is very large and much more representative of the US population than the convenience samples commonly used in lab experiments. Indeed, student samples constitute a fairly homogeneous group which is typically younger, more educated, and poorer than the general population, which could potentially under-

[4]TESS uses the online panel "KnowledgePanel" developed by GfK (formerly Knowledge Networks).

mine the generalisability of the results. In our experiment, we give participants some feedback on their position in the household income distribution, and it therefore makes more sense to have a representative sample of the whole US population, for whom the information would be particularly relevant.

Moreover, it is important to note that participants in the TESS pool cannot take part in more than one academic study per week, and on average they participate in only two studies per month. This is a very low number compared to other sample providers, where workers can take part in as many studies as they want.

2.3.2 Sample Characteristics

In order to participate in the experiment, people had to be based in the United States, and to be at least 26 years old. We decided to impose that restriction in order to avoid getting a lot of very young people in our final sample. The treatment would not have been very meaningful to them as many of them are still financially dependent.

The experiment was run in July 2016. In total, 1506 participants completed our experiment. This represents approximately 60 percent of all the people who started the survey. An attrition rate of 40 percent is larger than what is commonly observed in lab experiments, but it is comparable to the attrition rate of other TESS experiments.

Table 2.1 summarises the characteristics of the whole sample. For the analysis, we exclude participants who took less than 2 minutes or more than 30 minutes to complete the study, as this indicates that they did not pay attention to the experiment. We also exclude the few participants who correctly guessed their position in the income distribution, since we will not analyse their behaviour in the paper.

Overall, 51 percent of participants are male, which is almost equal to the proportion of men in the general US population. The median age in our sample is 54, while the median age of people over 25 in the US is 44 years old. The median household income in the TESS sample is $65,000, compared to $56,516 for the national estimate (Census Bureau, 2016). The average household size is 2.6 in our sample, which is equal to the national average according to the 2010 Census (Census Bureau, 2010). 73 percent of our participants identify as white, while the proportion of white people in the US is 80 percent (CIA, 2015). It therefore appears that our sample differs from the general population in certain respects, but it is important to remember that we restricted our sample to people over the age of 26, which could explain why we observe these differences for certain characteristics.

Table 2.1: Summary Statistics

Variable	Mean	Std. Dev.	N
Bachelor degree	0.363	0.481	1511
Male	0.514	0.5	1511
White	0.732	0.443	1511
Income	93133	283756	1489
Log income	10.805	1.485	1489
Size of the bias	-3.22	23.306	1436
Age	52.665	15.013	1511
Perceived position	51.712	25.106	1507
Black	0.101	0.302	1511
Hispanic	0.1	0.3	1511
Full-time employed	0.583	0.493	1511
Unemployed	0.114	0.319	1511

2.3.3 Descriptive Statistics

Participants are divided into two groups, one composed of people who over-estimate their position in the income distribution (over-estimators), and the other of people who under-estimate their position (under-estimators). In total, 720 participants (47.8 percent) thought that their position in the income distribution was higher than it actually was, while 753 (50 percent) thought the opposite.[5]

The general behaviour of participants in the different games is in line with the existing evidence, and is summarised in Table 2.2 and in Figures 2.1 to 2.4. In the coinflip game, people report having obtained 2.15 "Heads" on average, which indicates that many people choose not to lie about the outcome of the game.[6] This is consistent with the findings of Abeler et al. (2014), who show that lying costs tend to be very high. Similarly, we find that participants give on average 43 percent of their total endowment in the dictator game, which is only marginally higher than the average donation found in Engel (2011)'s meta-study on the dictator game.

Table 2.2: Summary Statistics: Behavioral Measures

Variable	Mean	Std. Dev.	N
Dishonesty	2.154	0.914	1511
Trust	22.627	13.482	1475
Negative reciprocity	41.17	19.367	1496
Dictator giving	43.123	18.533	1467

In the ultimatum game, a large fraction of participants (33%) demand from the first mover that they split the money equally, while very few people demand more than half of the money, all of which is consistent with the existing

[5] The remaining 33 participants gave a correct estimate of their position in the income distribution. We exclude these observations from the main sample, as they cannot be used for the analysis.

[6] If everyone truthfully reported the outcome, the average number of "Heads" obtained would be 2.

evidence (Oosterbeek et al., 2004). In the trust game, participants send on average $23 to the other person out of the $50 available. Interestingly, approximately 10 percent of them send the full amount to their partner, while only around 7 percent choose to send $5 or less. Overall, this pattern of behaviour is in line with the existing literature on trust games (Johnson and Mislin, 2011).

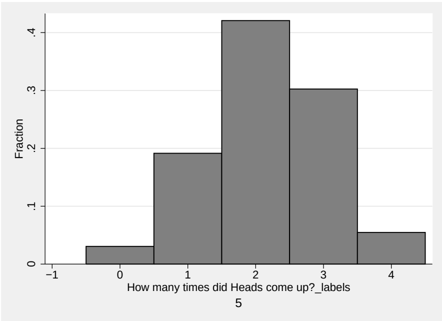

Figure 2.1: Distribution of the number of reported Heads in the coinflip game.

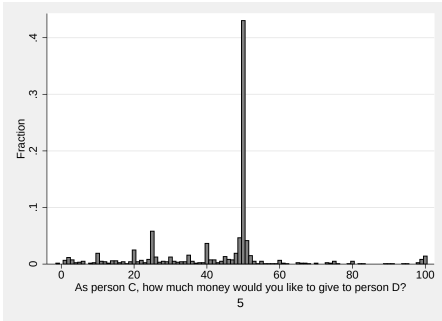

Figure 2.2: Distribution of the amount given by participants to their partner in the dictator game.

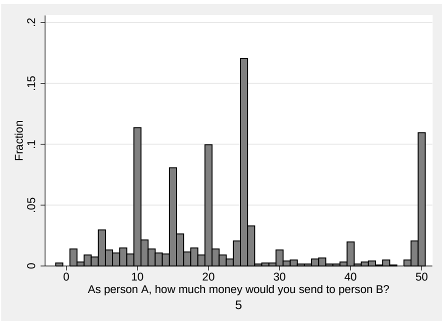

Figure 2.3: Distribution of the amount sent by the participants to their partner in the trust game.

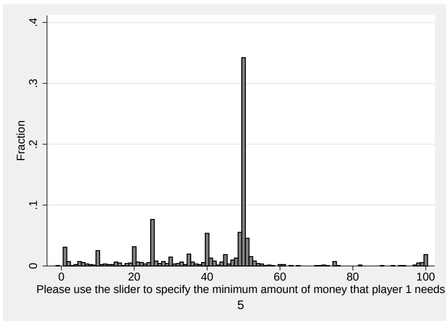

Figure 2.4: Distribution of the amount demanded by participants in the ultimatum game.

2.4 Empirical Analysis

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.[7]

2.4.1 Baseline Balance

To make sure that the randomisation worked as expected, we perform a baseline balance test, separately for over-estimators and under-estimators. Specifically, we check that participants in the treatment group and in the control group have similar characteristics on average, by regressing each pre-determined variable on the treatment indicator.[8]

As Tables 2.3 and 2.4 show, the pre-determined characteristics are mostly balanced, both for over-estimators and under-estimators. For under-estimators, we find no imbalances for any of the pre-determined variables. For over-estimators, we find that people in the treatment group are slightly less educated and slightly poorer than people in the control group. It is natural to find a couple of slight imbalances when testing for 13 different outcomes, and we show that all of the results in the main analysis are robust to the inclusion of these pre-determined characteristics as control variables.

[7] `https://www.socialscienceregistry.org/trials/1369`

[8] The list of pre-determined variables that we use to check for balance is: age, household size, employment status, religion, educational level, gender, log income, ethnicity, political orientation, belief about their position in the income distribution.

Table 2.3: Balance: Over-Estimators

	Treatment	**Control**	**P-value**
Household size	2.35	2.50	0.152
Graduated high school	0.28	0.34	0.064*
Bachelor degree	0.31	0.32	0.712
Male	0.50	0.52	0.533
White	0.73	0.70	0.351
Income	51082.33	53007.73	0.580
Log income	10.04	10.31	0.045**
Size of the bias	15.83	15.25	0.617
Age	54.19	54.36	0.887
Perceived position	55.91	56.69	0.666
Black	0.10	0.12	0.413
Hispanic	0.12	0.11	0.821
Full-time employed	0.47	0.46	0.803
Observations	334	386	
F-test of joint significance	1.07		

Note: Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.4: Balance: Under-Estimators

	Treatment	**Control**	**P-value**
Household size	2.79	2.78	0.955
Graduated high school	0.25	0.24	0.722
Bachelor degree	0.39	0.42	0.518
Male	0.51	0.54	0.416
White	0.74	0.75	0.846
Income	115572.84	145010.11	0.304
Log income	11.33	11.39	0.302
Size of the bias	-19.32	-19.53	0.864
Age	51.08	51.34	0.799
Perceived position	45.99	47.74	0.333
Black	0.08	0.09	0.623
Hispanic	0.10	0.08	0.292
Full-time employed	0.70	0.68	0.482
Observations	370	383	
F-test of joint significance	0.50		

Note: Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Moreover, it is important to note that the pre-determined variables do not jointly predict the treatment assignment, and this holds true both for over-estimators and under-estimators, as shown in Tables 2.3 and 2.4. Indeed, when we regress the treatment indicator on the set of pre-determined variables, we find that the joint F-test is far from significant for each regression. This provides some additional evidence that there are no important imbalances between treatment and control groups, both for over-estimators and under-estimators.

2.4.2 Methodology

Given that people who over-estimate their position in the income distribution receive a different treatment from those who under-estimate it, we conduct all of the analysis separately for each group. To examine how shocks to relative income affect economic satisfaction and pro-social behaviour, we estimate the following equation:

$$y_i = \gamma_0 + \gamma_1 T_i + \varepsilon_i$$

where y_i is the outcome of interest, and T_i is the treatment indicator, which is equal to one if the respondent received some information about their relative income, 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.

2.4.3 Manipulation Checks

Before examining the impact of the treatment on social preferences, we must make sure that the treatment did affect people's perception of their position in the income distribution, as well as people's economic satisfaction. Participants who learn that they are relatively richer than they thought should update their estimate of their relative position upwards, and they should also become happier about their position in the income distribution. Conversely, people who learn that they are relatively poorer than they thought should update their estimate of their relative position downwards, and we would expect them to become less satisfied with their position in the income distribution.

First, we examine whether our treatment affects people's beliefs about their position in the income distribution. We show in Table 2.5 that under-estimators who receive the treatment significantly update their belief in the expected direction by about 14 percentage points, which represents a change of 30 percent. Conversely, over-estimators who receive the treatment significantly update their prior downwards by about 21 percentage points, which corresponds to a change of almost 38 percent. These effects are very large and they are all significant at the one percent level, which provides some evidence that the treatment altered people's perception of their relative income.

Table 2.5: Manipulation Check: Update in Beliefs about Relative Income

	Beliefs about relative position	
	Overestimators	**Underestimators**
Treatment	-21.306***	14.607***
	(1.02)	(.894)
Prior Belief	56.326	46.881
N	716	753

Note: Robust standard errors are in brackets. The outcome variable of interest is what participants think their percentile in the income distribution is. The treatment indicator takes the value one for individuals receiving information about their position in the income distribution.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In Table 2.6, we show that over-estimators who learn that they are relatively poorer than they thought become significantly less happy with their position in the income distribution than the relevant control group. Indeed, the difference between the two groups is very large (approximately 0.30 of a standard deviation), and it is significant at the five percent level, whether we include controls or not.

Conversely, under-estimators who learn that they are relatively richer than they thought are significantly happier with their position in the income distribution than the control group. The difference between the two groups is large (approximately 0.25 of a standard deviation), and significant at the five percent level when we do not include controls.

These results suggest that our treatment was very effective in changing people's economic well-being. Our findings are in line with the existing evidence, which shows that well-being and job satisfaction are strongly affected by people's relative income (Card et al., 2012b; Luttmer, 2005). Interestingly, we find that both negative and positive shocks to relative income affect people's satisfaction with their position in the income distribution, whereas Card et al. (2012b) only found an effect of negative relative income shocks on job satisfaction.

Table 2.6: Manipulation Check: Economic Satisfaction

	Satisfaction with Relative Position	
	Overestimators	Underestimators
Panel A: Main		
Treatment	-0.313**	0.240**
	(0.137)	(0.112)
Panel B: With Controls		
treatment	-0.298**	0.184*
	(0.123)	(0.099)
N	720	753
Control Mean	3.972***	2.990***

Note: Robust standard errors are in brackets. The outcome variables of interest are people's satisfaction with their position in the income distribution. The treatment indicator takes the value one for individuals receiving information about their actual position in the income distribution. In Panel A results are displayed without any control variables. In Panel B we control for education level, gender, age, belief about their position in the income distribution, ethnicity, and their unemployment status.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2.4.4 Main Results

Over-Estimators

The first two panels of Table 2.7 summarise how a negative shock to relative income affects social preferences. The first panel reports the results from regressing the outcome variables on the treatment indicator, without any controls, while the second panel shows the results when control variables are included.[9]

Overall, we find that people who learn that they are relatively poorer than they thought do not change their behaviour in any of the four games. The fact that we do not find any significant treatment effects is not due to a lack of statistical power, since we had the power to detect effect sizes of 0.2 standard deviations with a probability of 80 percent. We have relatively precisely estimated nulls, given that both the effect sizes and the standard errors are mostly below 0.1 of a standard deviation.

Under-Estimators

The last two panels of Table 2.7 summarise how a positive shock to relative income affects social preferences. Panel C reports the results from regressing the outcome variables on the treatment indicator, without any controls, while Panel D shows the results when control variables are included.

[9] Specifically, we include the following control variables: education level, gender, age, belief about their position in the income distribution, ethnicity, and their unemployment status.

Overall, we observe that people who learn that they are relatively richer than they thought do not change their behaviour significantly in any of the four games. With a sample of approximately 750 participants for under-estimators, we can be confident that the absence of significant effects is not due to a lack of statistical power.

Table 2.7: Main Effect

	(1) Dishonesty	(2) Dictator	(3) Trust	(4) Negative Reciprocity
Panel A: Overestimators				
Treatment	0.066	1.019	0.617	1.125
	(0.068)	(1.401)	(0.993)	(1.449)
N	720	699	705	714
Control Mean	2.137	43.772	22.251	40.784
Panel B: Overestimators With Controls				
Treatment	0.086	1.983	1.043	1.704
	(0.068)	(1.390)	(1.001)	(1.450)
N	720	699	705	714
Control Mean	2.137	43.772	22.251	40.784
Panel C: Underestimators				
Treatment	-0.003	-0.532	0.769	0.514
	(0.067)	(1.374)	(1.015)	(1.418)
N	753	732	734	744
Control Mean	2.133	42.229	22.126	40.442
Panel D: Underestimators With Controls				
Treatment	-0.009	-0.431	0.884	0.727
	(0.066)	(1.350)	(0.990)	(1.402)
N	753	732	734	744
Control Mean	2.133	42.229	22.126	40.442

Note: Robust standard errors are in brackets. The outcome variables of interest are the reported number of "Heads" in the coinflip game (dishonesty), the amount people take from their partner in the stealing game, the amount sent in the trust game as well as the amount demanded in the ultimatum game. The treatment indicator takes the value one for individuals receiving information about their actual position in the income distribution. Panels A and B describe the results for individuals overestimating their relative position. Panels C and D show the results for respondents under-estimating their relative position. Panels B and D include the following controls: education level, gender, age, belief about their position in the income distribution, ethnicity, and their unemployment status.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Pro-Sociality Index

In order to test whether people who learn that they are relatively poorer (resp. richer) than they thought become slightly more pro-social generally, we construct an index composed of their behaviour in the trust game, the honesty game and the dictator game. We normalize these outcomes and use them to create an unweighted index which can be interpreted as a measure of pro-sociality. We did not include the ultimatum game in the index, because it is not entirely clear whether demanding a large share of the pie is more pro-social than asking for a small share. Indeed, demanding a more equal split could be seen as pro-social, because it enforces a general norm of fairness. At the same time, people who learn that they are richer than they thought might become more willing to accept lower offers, because they do not want their partner to end up with nothing, which could also be deemed pro-social.[10] When we regress this index on the treatment indicator, we find that neither negative nor positive shocks to perceived economic status change people's pro-social behaviour, as shown in Table 2.8.

[10] Including the ultimatum game in the index does not affect the results in any way.

Table 2.8: Main Effect: Indices

	Prosociality Index	
	Overestimators	Underestimators
Treatment	0.017	0.015
	(0.048)	(0.049)
N	689	724
R^2	0.000	0.000

Note: Standard errors in parentheses.

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

2.4.5 Heterogeneous Treatment Effects

We also examine whether there are heterogeneous treatment effects caused by the information treatment. For all of the heterogeneity analysis, we only look at the four main outcome measures and the index of pro-sociality. Specifically, we estimate the following equation, where $Inter_i$ refers to the interaction variable:

$$y_i = \pi_0 + \pi_1 Treatment_i \times Inter_i + \pi_2 Treatment_i + \pi_3 Inter_i + \Pi_4^T \mathbf{X_i} + \varepsilon_i$$

First, we explore potential heterogeneity by people's belief about their relative position in the income distribution. In Tables 2.9 and 2.10, we show that people who have different beliefs regarding their position in the income distribution do not appear to react differently to the information treatment, irrespective of whether they over-estimate or under-estimate their relative income.

Similarly, we examine whether people who think that they earn more than the median household react differently from people who believe the opposite. Interestingly, we do not observe any differences in the way these two subgroups behave in the four games, both for over-estimators and under-estimators.

Finally, we do not observe any heterogeneous treatment effects by income level, as the third panel of Tables 2.9 and 2.10 clearly demonstrates.

Table 2.9: Heterogeneous Effects: Over-Estimators

	(1) Dishonesty	(2) Dictator	(3) Trust	(4) Negative Reciprocity
Panel A: Belief about Position				
Treatment	-0.254	-3.250	1.123	3.489
	(0.171)	(4.032)	(2.502)	(3.881)
Treatment × Belief about position	0.006**	0.079	-0.005	-0.037
	(0.003)	(0.065)	(0.044)	(0.065)
Belief about position	-0.001	0.008	0.094***	-0.004
	(0.002)	(0.047)	(0.029)	(0.042)
N	716	696	702	710
Panel B: Above Median Income				
Treatment	-0.060	0.918	1.573	3.013
	(0.104)	(2.202)	(1.393)	(2.206)
Treatment × Belief: above median income	0.222	0.292	-1.297	-3.322
	(0.137)	(2.850)	(1.951)	(2.933)
Belief: above median income	-0.039	1.134	4.095***	0.502
	(0.095)	(1.953)	(1.336)	(1.966)
N	720	699	705	714
Panel C: Log Income				
Treatment	0.058	-15.046	-6.322	-7.082
	(0.402)	(9.659)	(5.856)	(9.353)
Treatment × Log Income	0.003	1.630*	0.724	0.820
	(0.039)	(0.916)	(0.571)	(0.895)
Log Income	-0.023	-1.149	-0.017	-1.537**
	(0.031)	(0.701)	(0.493)	(0.690)
N	698	678	684	694

Note: Robust standard errors are in brackets. The outcome variables of interest are the reported number of "Heads" in the coinflip game (dishonesty), the amount people take from their partner in the stealing game, the amount sent in the trust game as well as the amount demanded in the ultimatum game. The treatment indicator takes the value one for individuals receiving information about their actual position in the income distribution. All Panels the results for individuals over-estimating their relative position. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.10: Heterogeneous Effects: Under-Estimators

	(1) Dishonesty	(2) Dictator	(3) Trust	(4) Negative Reciprocity
Panel A: Belief about Position				
Treatment	-0.025	5.229	1.593	3.108
	(0.145)	(3.344)	(2.117)	(3.267)
Treatment × Belief about position	0.000	-0.120**	-0.014	-0.051
	(0.003)	(0.060)	(0.043)	(0.061)
Belief about position	-0.002	0.164***	0.120***	0.145***
	(0.002)	(0.040)	(0.029)	(0.037)
N	753	732	734	744
Panel B: Above Median Income				
Treatment	-0.010	0.114	0.370	1.146
	(0.090)	(1.976)	(1.295)	(1.935)
Treatment × Belief above median income	0.009	-1.278	1.123	-1.227
	(0.135)	(2.687)	(2.044)	(2.822)
Belief above median income	-0.107	4.085**	3.805***	5.302***
	(0.091)	(1.863)	(1.409)	(1.888)
N	753	732	734	744
Panel C: Log Income				
Treatment	-0.428	31.444	11.090	7.226
	(1.200)	(21.540)	(15.836)	(23.081)
Treatment × Log income	0.037	-2.811	-0.899	-0.580
	(0.105)	(1.888)	(1.404)	(2.028)
Log income	-0.030	2.524**	2.591**	2.290*
	(0.076)	(1.264)	(1.105)	(1.255)
N	753	732	734	744

Note: Robust standard errors are in brackets. The outcome variables of interest are the reported number of "Heads" in the coinflip game (dishonesty), the amount people take from their partner in the stealing game, the amount sent in the trust game as well as the amount demanded in the ultimatum game. The treatment indicator takes the value one for individuals receiving information about their actual position in the income distribution. All Panels the results for individuals over-estimating their relative position. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2.4.6 Determinants of the Size of the Bias

In the following analysis, we try to understand why people have biased beliefs about their position in the income distribution. We examine which characteristics at the individual level are significantly correlated with people's bias about their position in the income distribution.

In particular, we regress the dummy variable, $overestimator_i$, on a vector of individual-specific variables, $\mathbf{X_i}$:[11]

$$overestimator_i = \alpha_0 + \Gamma_0^T \mathbf{X_i} + \varepsilon_i$$

The vector of individual-specific variables, X_i, consists of the following variables: gender, age, log income, household size, ethnicity (dummies for White, Black, Hispanic, and Asian), religion (dummy for Christian), political orientation (taking value one for Republicans and zero otherwise), employment status (dummies for people who are unemployed, part-time employed, and employed full-time), and education (dummy for a person with at least a bachelor degree).

In Table 2.11, we observe that richer people are more likely to under-estimate how high they are in the income distribution, and so do people who live in large households and who are employed full-time. On the other hand, more educated individuals are more likely to over-estimate their position in the in-

[11]This is not exactly the analysis which was pre-specified. Indeed, the original plan was to regress the size of the bias on the explanatory variables.

come distribution.

Table 2.11: Determinants of the Bias about the Position in the Income Distribution

	(1) Overestimator
Male	-0.031
	(0.024)
Household size	-0.023**
	(0.009)
Log income	-0.127***
	(0.009)
White	0.072
	(0.049)
Black	0.074
	(0.060)
Hispanic	0.091
	(0.061)
Age	0.001
	(0.001)
Degree	0.054**
	(0.027)
Christian	-0.015
	(0.026)
Republican	-0.012
	(0.026)
Employed full-time	-0.130***
	(0.034)
Unemployed	-0.015
	(0.049)
Constant	1.884***
	(0.122)
N	1450
R^2	0.184

Note: Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.5 Conclusion

In this paper, we show that exogenous changes in perceived economic status do not significantly affect people's social preferences. We find that people who learn that they are relatively poorer than they thought become less satisfied with their position in the income distribution, do not send more more (or less) money to their partner in a trust game, that they do not become more (or less) generous in a dictator game, that they do not change their lying behaviour, and that they demand the same amount from their partner in an ultimatum game as the relevant control group. People who learn that they are higher in the income distribution than they thought report being more satisfied with their relative standing in the income distribution, even though this does not translate into a change in behaviour in any of the four games. Importantly, we have a large sample which is representative of the US as a whole, which attenuates concerns about the external validity of our results.

Overall, these results imply that changes in perceived relative income may not play such an important role in determining people's pro-social behaviour. Our evidence is particularly striking given previous studies which find that social class has an important effect on social preferences (Piff et al., 2012). This discrepancy demonstrates the need for more research on the role that relative concerns and social status play on social preferences.

Chapter 3

Does Information Change Attitudes Towards Immigrants? Representative Evidence from Survey Experiments[1]

3.1 Introduction

In recent years, the United States and many European countries have witnessed a surge in anti-immigrant sentiment, 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 want to re-

[1]This chapter was written with Christopher Roth and Diego Ubfal.

duce 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 a lot of support in the last few years, such as the Front National in France, or Donald Trump in the United States.

However, even though immigration is a central issue in many national elections, such as the 2016 US presidential election or the EU referendum in the UK, voters remain highly misinformed about the topic (IpsosMori, 2014; Citrin and Sides, 2008; Blinder, 2015). For example, people consistently overestimate 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. It is therefore crucial to understand whether people would change their attitude towards immigrants if they received accurate information about immigration.

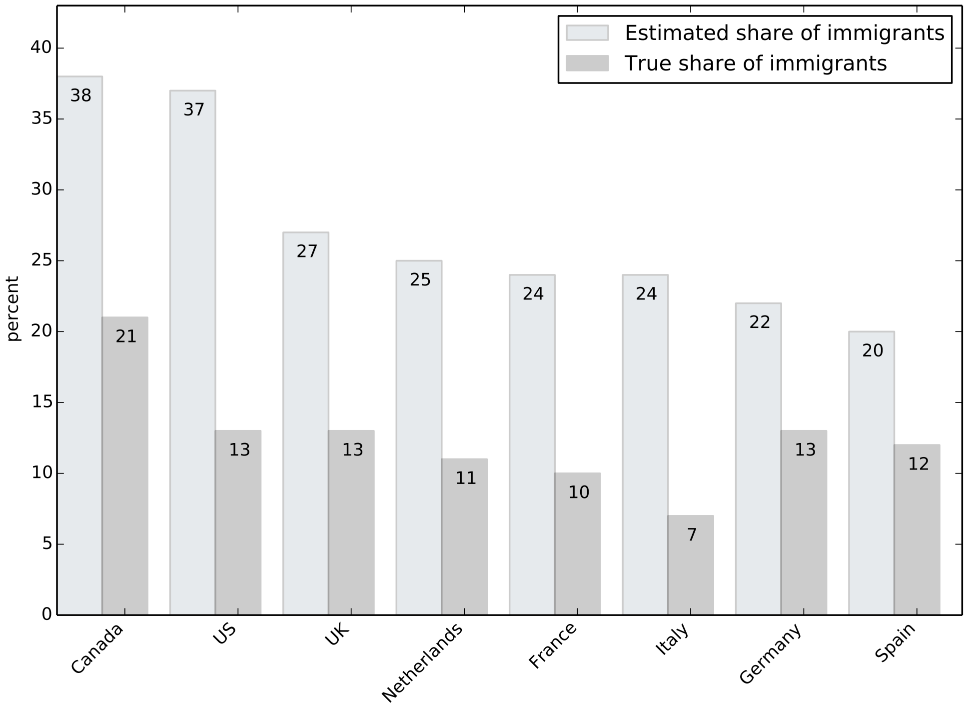

Figure 3.1: Biases in beliefs about the share of immigrants in different OECD countries. Source: 2010 wave of the Transatlantic Trends Survey.

To answer this question, we present the results from three studies. First, we analyze a large cross-country survey experiment conducted in thirteen countries around the world, including the United States, Canada, 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. 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, although they do not become less worried about immigration generally.

This could be due to the fact that they only received information about the proportion of immigrants, and not about their characteristics. People care deeply about the kind of immigrants living in their country, and they often have very inaccurate beliefs on the crime rate of immigrants, their ability to speak the local language, and their integration in society more generally. It is therefore important to understand whether a more comprehensive information treatment could change people's opinions of immigrants, and affect their policy preferences regarding immigration. We conduct an additional experiment in order to test this hypothesis.

We implement our experiment with two large samples from the US. The first sample is composed of 1,200 observations, and it is representative of the US population in terms of age, gender and region of residence. The second sample consists of 800 people recruited on Amazon Mechanical Turk (MTurk), who were re-surveyed four weeks after taking part in the main experiment. This allows us to examine whether treatment effects persist over time.

The experiment is structured as follows: First, we provide half of the participants with five general facts about immigration in the US: (i) the share of immigrants, (ii) the share of illegal immigrants, (iii) the unemployment rate and (iv) the incarceration rate of immigrants, and (v) the share of immigrants who cannot speak English. Then, we ask all participants to complete a questionnaire on their beliefs about immigrants and their policy preferences regarding immigration. We also obtain two behavioral measures of their attitude towards immigrants, first by asking them how much money they want to donate to a pro-immigrant charity, and then by asking them whether they are willing to sign a real petition on the White House website in favor of increasing the number of available green cards.[2]

We find that the information treatment improves people's impression of immigrants, and that it moderately increases people's willingness to donate money to a pro-immigrant charity.[3] Moreover, people in the treatment group become slightly more willing to increase the number of legal immigrants (0.13 of a standard deviation), which is completely driven by Republican respondents. However, respondents' policy preferences regarding illegal immigrants remain on average unchanged. We also find that participants who receive the information treatment are not more likely to sign the petition in favor of increasing the number of green cards, and they are as likely to be in favor of deporting all illegal immigrants as the control group.[4] This evidence indicates that, while providing information can change how people perceive immigrants, it might not be enough to significantly change their policy preferences.

In our follow-up survey with the MTurk sample, we ask participants the same

[2]We pre-specified our empirical strategy and our hypotheses in two pre-analysis plans, which were registered on the Social Science Registry website prior to running the experiment with each of the two samples. `https://www.socialscienceregistry.org/trials/1092`

[3]For the donation measure, the effect size of the treatment effect varies with the sample. In the MTurk sample, the treatment effect is fairly large and statistically significant (0.22 of a standard deviation), whereas in the TNS sample, the treatment effect is small and not statistically significant (0.07 of a standard deviation).

[4]These effects are precisely estimated, as we have enough statistical power to detect even small effect sizes.

set of self-reported questions on immigration as the ones they answered in the main experiment. Overall, 88 percent of the original MTurk sample completed the follow-up survey, and we observe no differential attrition between the treatment and the control arm. We find that the treatment effects are very similar four weeks after the treatment. Participants who received the information four weeks earlier still remember it, have a more positive opinion of immigrants, and are more supportive of increasing the number of incoming legal immigrants. Moreover, their policy preferences regarding illegal immigrants stay unchanged.

We hypothesize that people's attitudes towards immigrants become more positive after the information treatment because participants realize that existing immigrants tend to be more law-abiding, employed, and fluent in English than they originally thought. People care strongly about the characteristics of immigrants, and Hainmueller and Hopkins (2014) and Bansak et al. (2016) show that there is a consensus among Americans and Europeans that immigrants should speak the local language, should not be unemployed and should be in the country legally. Our treatment changes people's beliefs on these key characteristics.

Across all of our different samples, we find evidence that people who identify as right-wing and who have more negative views on immigration respond more strongly to the information treatment. In our US samples, we find that not only do participants who self-identify as Republicans develop a more positive opinion of immigrants, but they also become more likely to support pro-immigrant policies, even four weeks after they received the information treatment. Similarly, in the cross-country experiment, respondents who self-identify as right-wing change their attitudes more strongly after being told the share of immigrants in their country compared to people not identifying as right-wing.

Finally, we examine which characteristics predict how biased people's beliefs are about immigrants. We find that people who are more educated have much less biased beliefs about immigration, which is consistent with the evidence showing that education can reduce the level of political misinformation among the general public (d'Hombres and Nunziata, 2016). Moreover, people who live in areas with a larger share of immigrants have more biased beliefs, which suggests that people's beliefs on immigration are heavily influenced by what they experience at a local level.

Our paper adds to the literature examining whether people's political attitudes respond to information (Kuklinski et al., 2000; Gilens, 2001; Lawrence and Sides, 2014).[5] Overall, there is mixed evidence on the impact that information has on people's policy preferences and their behavior (Elias et al., 2015). For example, Cruces et al. (2013) and Karadja et al. (2014) find that informing people about their position in the income distribution changes their redistributive preferences, while Kuziemko et al. (2015) observes that giving people information about the level of inequality in the US does not change their redistributive preferences.

[5] For an overview on the related literature on persuasion, see DellaVigna and Gentzkow (2010).

Our paper is most closely related to concurrent work by Hopkins et al. (2016). They conduct experiments with representative samples of the American population, where they tell a random subset of their participants the proportion of immigrants in the US. They find that the information they provided has no significant effect on people's policy preferences.

Our survey experiments extend the work by Hopkins et al. (2016) in several ways. First, we provide people with a more comprehensive information treatment by also giving them statistics about the characteristics of immigrants. Second, we employ behavioral measures to assess the impact of information on people's political preferences, instead of relying solely on self-reported measures. Third, our follow-up experiment allows us to show that the treatment effects persist over time. This is important as experimenter demand is likely lower in the follow-up, where no additional treatment was administered. Fourth, the cross-country survey experiment 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.

We also contribute to the literature on the determinants of people's attitudes towards immigrants (Hainmueller et al., 2015; Scheve and Slaughter, 2001; Algan et al., 2012; Bisin et al., 2008). Previous studies have focused on characteristics such as age, media exposure, competition in the labor market, exposure to immigrants, education or income to explain people's attitude towards immigrants (Facchini et al., 2009; Card et al., 2012a; Dustmann and Preston, 2001, 2006; Mayda and Facchini, 2009; Mayda, 2006; Citrin et al., 1997; Dustmann et al., 2016; Halla et al., 2016). In concurrent work, Facchini et al. (2016) conducted a large experiment in Japan, where they showed that reading various short articles on the potentially beneficial effects of immigration improved people's views on immigration. Our paper shows that misinformation about the proportion and the characteristics of immigrants also plays an important role in shaping people's views on immigrants.

This chapter proceeds as follows: in section 3.2, we outline the evidence from the cross-country survey experiment. In section 3.3, we present the design of the online experiment and describe our two samples. The results from the online experiment are described in section 3.4. Finally, section 3.5 concludes.

3.2 Cross-Country Experiment

We first analyze a large-scale cross-country experiment in which half of the participants are informed about the share of immigrants in their country, before being asked questions about their stance on immigration.

3.2.1 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 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 on people's attitude towards immigration.[6]

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.[7] 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.[8] Importantly, more than 94 percent of those who started the survey answered the main questions of interest, which means that attrition is not an issue for this experiment.[9]

[6]The 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 et al. (2014). Indeed, the report did not include any regression analysis nor heterogeneity analysis.

[7]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. The response rate for phone interviews ranged from 4 percent in France, the UK and the Netherlands to 27 percent in the US.

[8]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. For face-to-face interviews, the response rate was significantly higher: 49 percent in Russia and 40 percent in Poland (Wunderlich et al., 2010; Stelzenmueller et al., 2014).

[9]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. 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.

3.2.2 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 assigned to 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?

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

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 undocumented immigrants. For example, people are asked whether they are worried about legal and illegal immigration into their country, whether immigrants increase crime and whether illegal immigrants should be given the opportunity to obtain legal status.

3.2.3 Results

Main Results

As Figure 3.2 and Table 3.1 clearly show, people who receive information about the share of immigrants in their country become much less likely to say that there are too many immigrants in their country, and they become more likely to say that there are not many immigrants. The probability of saying that there are too many immigrants is 11.3 percentage points lower for those who receive the information treatment, while the probability of saying that there are not many immigrants is 15.7 percentage points higher.[10]

[10]The results are robust to the inclusion 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 A10.

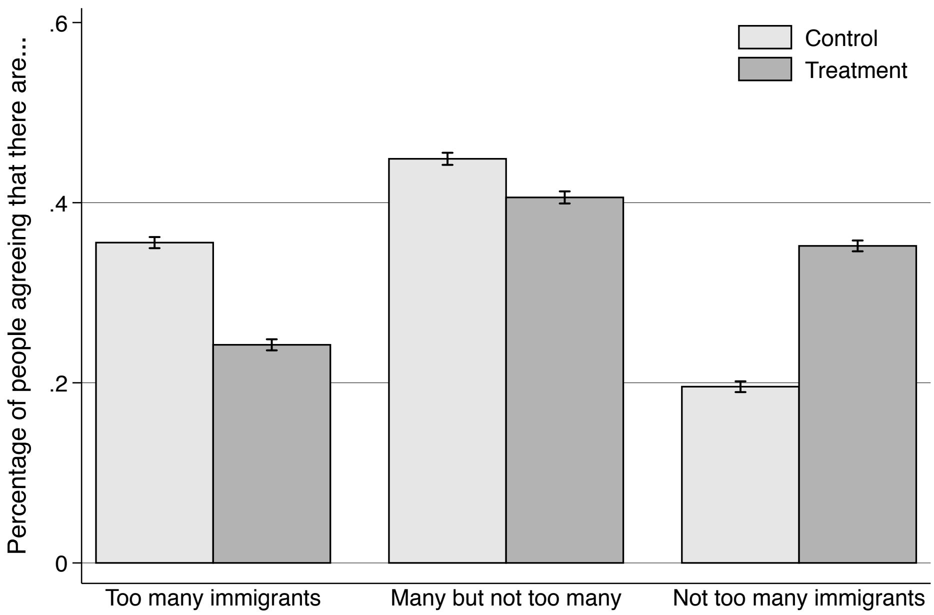

Figure 3.2: This figure presents the distribution of answers given by the treatment group and the control group to the question on whether there are too many immigrants in this country.

Table 3.1: Main Result: Transatlantic Trends Survey

	Percentage of people saying yes to: "There are too many immigrants"		
Treatment	-0.1182***	-0.0957***	-0.1039***
	(0.0093)	(0.0115)	(0.0124)
Treatment × Right-wing		-0.0519***	
		(0.0189)	
Right-wing		0.1434***	
		(0.0143)	
Treatment × Negative View			-0.0967***
			(0.0294)
Negative View			0.3044***
			(0.0218)
N	19407	19407	11604
R^2	0.059	0.075	0.117
Country Fixed Effects	Y	Y	Y

Note: We present the results of the experiment embedded in the Transatlantic Trends survey. The outcome variable takes value one if individuals agree that there are too many immigrants in their country, and value zero otherwise. The treatment variable is equal to 1 if the person received information about the proportion of immigrants in their country. All specifications include country fixed effects. Robust standard errors are displayed in parentheses.

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

We expected that this light information treatment would not meaningfully shift people's policy preferences regarding immigration. In line with our expectation, Tables A7 to A9 of Appendix C show that being informed about the proportion of immigrants does not make people less worried about immigration, and it does not change people's policy preferences regarding undocumented immigrants. The treatment effects are precisely estimated, and they are in line with Hopkins et al. (2016), who find that giving people information about the share of immigrants does not affect their policy preferences.

Heterogeneous Treatment Effects

We exploit several sources of heterogeneity that the cross-country experiment offers. First, we examine heterogeneity in the response to the information treatment across countries. In Figure 3.3, 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. The magnitude of the treatment effects varies a lot by country. We observe the largest effect sizes for countries where a larger share of people think that there are too many immigrants, such as Greece, Italy, the UK, and the US.

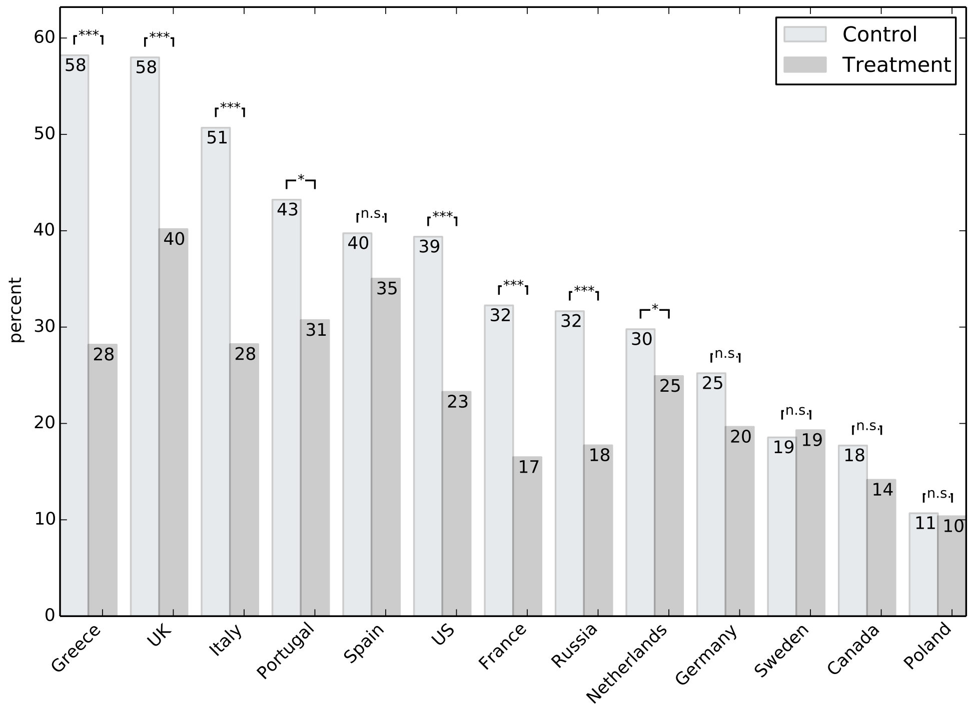

Figure 3.3: Cross-country evidence: the effect of information on the probability of saying that there are too many immigrants.

It is particularly important to understand whether people with more negative or more positive initial views respond more strongly to the information intervention. Therefore, we evaluate whether there are heterogeneous treatment effects.[11] We estimate the following equation, where $inter_i$ refers to the interaction variable, and where $Treatment_i$ is equal to 1 if the person received

[11]For all of the heterogeneity analysis, we use either questions which were asked before the treatment, or pre-determined characteristics, such as political orientation. The choice of variables for the heterogeneity analysis in this sample is motivated by our findings in the online experiments presented below.

the information treatment:

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

In Column 2 of Table 3.1, we examine heterogeneous treatment effects by people's political orientation. We 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. We find that treated individuals who self-identify as right-wing react more strongly to the treatment.

We also observe 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.[12,13] The treatment effect is twice as large for this group, as can be seen in Column 3 of Table 3.1.

3.3 Online Experiment

The cross-country experiment shows that informing people about the proportion of immigrants in their country makes them less likely to state that there are too many immigrants, although it does not make them less worried about immigration. However, people are not only concerned about the number of

[12] We create 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.

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

immigrants in their country, they also care about the characteristics of these immigrants, and whether they integrate into society. It is therefore important to understand whether a more comprehensive information treatment could improve people's opinions of immigrants, and affect their policy preferences regarding immigration. To test this hypothesis, we designed an experiment which provides not only information about the share of immigrants, but also on the characteristics of existing immigrants, namely their unemployment rate, their incarceration rate, and the proportion of immigrants who cannot speak English.

We conducted this experiment using two different samples, each with its own advantages. TNS Global provided us with an online sample of 1193 US citizens, representative of the general population in terms of age, gender, and region of residence. TNS Global was well suited for our experiment, since they had already provided the samples for the Transatlantic Trends Surveys. The other sample was obtained through Amazon Mechanical Turk (MTurk), which enabled us to collect follow-up data to test whether the treatment effects would persist over time.

3.3.1 Experimental Design

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 statistics about immigration: the proportion of immigrants in the US, the proportion of illegal immigrants in the US, the unemployment rate of immigrants, their incarceration rate, and the proportion of immigrants who cannot speak English.[14,15]

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.[16] In the MTurk sample, 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 MTurk participants look up the answers online, we only give them 25 seconds to answer each question.[17]

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 immi-

[14] We chose these statistics for two main reasons. First, there is some evidence showing that people are particularly concerned about these issues. Recent evidence by Bansak et al. (2016) and Hainmueller and Hopkins (2014) suggests that people prefer immigrants who are not unemployed, who speak English and who did not enter illegally. Second, there exists Census data on these issues, which increases the reliability of the information we provide.

[15] For a complete description of the experimental design, please refer to the pre-analysis plan, which is available at `https://www.socialscienceregistry.org/trials/1092/history/7106`.

[16] Both the treatment and the control group receive this information, and the internal validity of our study is therefore not compromised.

[17] TNS Global faced some implementation constraints which prevented them from incentivizing the belief questions, and from imposing a time limit to the participants.

grants:[18] *"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 participants a series of questions on their perception of legal and illegal immigrants, as well as on their policy preferences regarding immigration. For instance, we ask them whether they think that there are too many immigrants in the US, whether legal immigration should be reduced and whether immigrants have a negative impact on American society as a whole.

We also use two behavioral measures to assess whether the treatment changed our participants' attitude towards immigrants and their policy preferences.[19] 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.[20] Only 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. 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,

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

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

[20] The text used for the petition can be found online at the following URL: `https://petitions.whitehouse.gov//petition/facilitate-legal-immigration-us-1`.

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.[21]

Once the behavioral measures are over, participants from the TNS sample have to complete an attention check, whose purpose is to assess how attentive participants were in the experiment.[22] 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 how well they remember the information that we gave them. Finally, respondents complete a questionnaire on demographics including variables such as gender, age, education and income.

[21]Donations to NGOs with clear ideological inclinations and in particular campaign contributions have been used previously to measure political preferences (Perez-Truglia and Cruces, 2016).

[22]The attention check was not included in the experiment with the MTurk sample.

Follow-Up Study

To examine whether the treatment effects persisted over time, we conducted a follow-up study four weeks after the main experiment, using the MTurk sample. We asked people the same set of self-reported questions on immigration as the ones they answered in the main experiment, and we also asked them to estimate the same five statistics about immigration.[23] This allows us to see whether people in the treatment group remember the information provided.

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.[24]

3.3.2 Description of the Samples

TNS Global

We conducted our experiment using a representative sample of the US population, which was provided by TNS Global, a world-leading company in market

[23] See the pre-analysis plan for a complete description of the follow-up study.

[24] 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 depend on their choices in the main experiment. 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.

research and political surveys. We obtained a sample of 1193 people living in the United States, which is representative of the US population in terms of age, gender and region of residence. All the participants completed the survey online, using a link which was provided by TNS Global.[25]

To participate in the experiment, people had to pass an attention screener at the start of the survey (Berinsky et al., 2014).[26] The experiment was run at the beginning of September 2016. The characteristics of the whole sample are described in Table 3.2.

[25]TNS provided us with 1193 observations rather than 1,000 as we had specified in the pre-analysis plan due to a technical problem.

[26]The attrition rate in our experiment with TNS was extremely low. Only 18 participants (*i.e.* less than 2 percent of the sample) dropped out of the experiment after the initial screener was administered and only 9 participants (less than 1 percent of the overall sample) dropped out after the treatment was allocated. We also find no evidence of differential attrition across treatment arms.

Table 3.2: Balance Table: MTurk and TNS

	MTurk Sample			MTurk Follow-up Sample			TNS: Representative Sample		
	Treatment	Control	P-value	Treatment	Control	P-value	Treatment	Control	P-value
Income	49193	49003	0.926	49193	49003	0.926	62465	62083	0.834
Log income	10.564	10.556	0.879	10.362	10.449	0.404	10.328	10.352	0.860
Age	35.235	34.389	0.280	34.559	34.116	0.590	40.769	40.303	0.541
Male	0.587	0.532	0.119	0.576	0.527	0.161	0.493	0.494	0.978
Household size	3.626	3.540	0.381	3.556	3.504	0.605	2.875	2.978	0.201
Hispanic	0.039	0.033	0.657	0.038	0.033	0.675	0.042	0.045	0.763
Black	0.064	0.095	0.105	0.062	0.094	0.097	0.072	0.084	0.435
White	0.792	0.775	0.555	0.777	0.767	0.737	0.829	0.792	0.096
Christian	0.425	0.396	0.405	0.417	0.392	0.471	0.635	0.639	0.908
Full-time employed	0.582	0.565	0.634	0.571	0.559	0.747	0.517	0.519	0.928
Part-time employed	0.181	0.182	0.981	0.177	0.180	0.932	0.152	0.134	0.383
Unemployed	0.078	0.130	0.016	0.077	0.129	0.014	0.094	0.103	0.607
At least bachelor	0.450	0.481	0.381	0.441	0.476	0.322	0.488	0.479	0.748
Born in the US	0.951	0.941	0.535	0.933	0.932	0.946	0.948	0.945	0.782
Worried about immigration	2.760	2.839	0.213	2.753	2.833	0.202	2.768	2.805	0.469
Belief English: Prior	32.501	30.174	0.122	32.038	30.051	0.187	36.032	36.279	0.871
Belief Unemployed: Prior	21.988	18.240	0.006	21.777	18.094	0.006	23.592	22.971	0.649
Belief Share Immigrants: Prior	21.697	23.033	0.239	21.528	22.911	0.220	33.619	33.785	0.900
Belief Share Illegal Immigrants: Prior	13.819	13.650	0.874	13.633	13.605	0.979	24.470	24.376	0.943
Belief Crime: Prior	12.711	11.716	0.310	12.592	11.673	0.344	17.211	18.306	0.389
Democrat	0.577	0.588	0.748	0.566	0.582	0.639	0.448	0.450	0.938

Note: We present the balance test for our samples from MTurk and TNS.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

MTurk: Main Experiment

We also conducted our 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 (Paolacci and Chandler, 2014). The pool of workers on MTurk is more representative of the US population than student samples.

Moreover, MTurk participants have been shown to be more attentive to instructions than college students (Hauser and Schwarz, 2015), and to give high-quality answers. To guarantee that the data we obtain are reliable, we only allowed workers who had an overall rating of more than 95 percent and who had completed more than 500 tasks on MTurk to take part in our study.[27]

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 3.2 summarizes the characteristics of the sample.

MTurk: Follow-Up

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

[27]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.

percent. This high re-contact rate indicates that it is possible to construct panels on MTurk with relatively low attrition, which is an additional advantage of the platform. The recontact rates for the treatment group and the control group are very similar, and statistically indistinguishable (p-value = 0.708). The overall sample composition remained more or less unchanged compared to the main experiment, as shown in Table 3.2.

3.4 Results

We pre-registered the experimental design, our hypotheses and our empirical specifications on the Social Science Registry before running the experiment with MTurk and with TNS. Almost all of the analyses presented in this paper were pre-specified.[28]

3.4.1 Baseline Balance for the MTurk and TNS Samples

In Table 3.2, we examine in how far the control group and the treatment group differ in terms of observable characteristics for the MTurk and the TNS samples. Overall, both samples are well balanced. We find a few small imbalances for the MTurk sample, and we therefore show our main results controlling for these pre-determined characteristics.[29,30] Including control variables im-

[28] We explicitly mention in the paper which analyses were not part of the part of the pre-analysis plan. The full pre-analysis plan can be accessed at `https://www.socialscienceregistry.org/trials/1092/history/7106`.

[29] The results without controls are very similar and can be found in the online appendix: `https://www.dropbox.com/s/7iym86pc4we3udl/onlineappendix_new.pdf?dl=0`

[30] 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

proves the precision of the treatment effect estimates compared to the specifications without controls, but barely changes the coefficient estimates.

3.4.2 Main Results

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 compare the behavior of people in the treatment group with that of people in the control group, by estimating the following equation:[31]

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

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

We account for multiple hypothesis testing by adjusting the p-values using the "sharpened q-value approach" (Benjamini et al., 2006; Anderson, 2008).[32] For each table, we also create an index of the outcomes, which we regress on the

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.

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

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

treatment indicator, as specified in the pre-analysis plan.

Changes in Beliefs

We hypothesize that the treatment might affect people's attitude towards immigrants and their policy preferences through a change in their beliefs about immigrants. In this section, we show that participants in the treatment group strongly updated their beliefs about immigrants after having received the information treatment, which constitutes the first step in the hypothesized causal chain. In figure 3.4, we show the average estimates that participants in treatment group gave before receiving the correct information, and after the treatment, for the MTurk sample and the TNS sample.

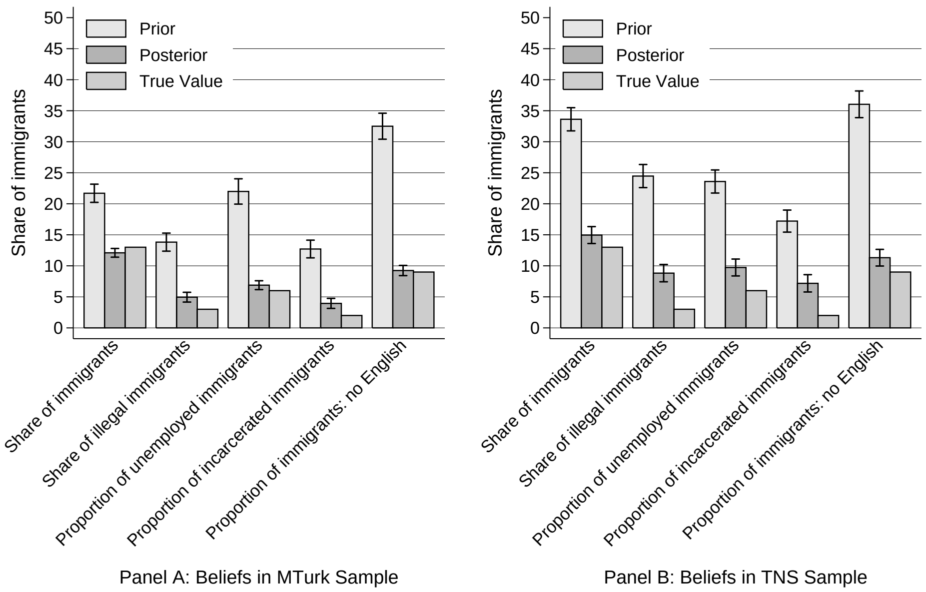

Figure 3.4: This figure presents the prior and posterior beliefs about the statistics about immigrants. On the left-hand side, in Panel A we show results for the MTurk sample. On the right-hand side, in Panel B we show results for the TNS sample. The figures display the means as well as the 95% confidence intervals.

It is clear that, before the treatment, participants had biased beliefs about immigration. Their estimates were on average consistently higher than the actual values. For instance, people in the TNS sample over-estimated the percentage of immigrants in the US by more than 20 percentage points, while MTurkers over-estimated the share of immigrants who cannot speak English by more than 24 percentage points.

We also test the extent to which MTurkers in the treatment group remember the information four weeks after the main experiment. In Figure 3.5, we show that people's estimates four weeks after the treatment are still fairly accurate. For instance, the average estimate of the proportion of immigrants is 15 percent in the follow-up, whereas the true value is 13 percent.[33]

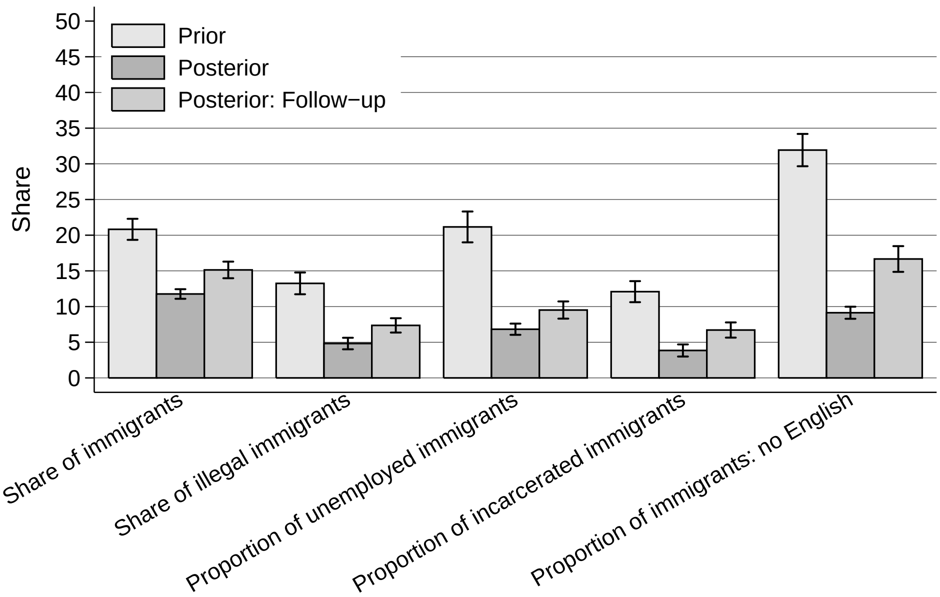

Figure 3.5: This figure presents prior and posterior beliefs for the sample who answered the four-week follow-up. We also present beliefs elicited in the four-week follow up.

Similarly, in Table 3.3, we show that, compared to the control groups, the treatment groups from both the MTurk and the TNS samples 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 and the effect sizes are large and correspond to more than half of the gap between

[33]People 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.

Democrats and Republicans.[34] We also show in Panel D of Table 3.3 that these effects persist four weeks after the treatment, that they are statistically significant, and that they remain fairly large (about 0.20 of a standard deviation effect size).

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

Table 3.3: Main Effects: Opinion about Immigrants (Part 1)

	(1)	(2)	(3)	(4)
	Opinion: Crime	**Opinion: Unemployment**	**Opinion: English**	**Index**
A: MTurk sample				
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
B: TNS sample				
Treatment	0.268***	0.309***	0.312***	0.302***
	(0.047)	(0.052)	(0.053)	(0.033)
FDR-adjusted p-value	[.001]***	[.001]***	[.001]***	
N	1193	1193	1193	1193
Scaled Effect	0.688	2.576	0.572	0.830
C: Pooled Results				
Treatment	0.238***	0.395***	0.337***	0.334***
	(0.035)	(0.039)	(0.041)	(0.026)
FDR-adjusted p-value	[.001]***	[.001]***	[.001]***	
N	1993	1993	1993	1993
D: Follow-up: MTurk				
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

Note: 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 Republicans and those who are Democrats. In Panel A, we display the results from the MTurk sample. In Panel B, we display the results from the representative sample. In Panel C, we show results pooling together the MTurk sample and the representative sample. In Panel D we display the results from the follow-up experiment from the MTurk sample. We include the following control variables: log income, age, gender, household size, indicators for race, religion, indicators for 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.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Attitudes towards Immigrants

The information treatment had an effect on how people perceive immigration generally, as shown in Table 3.4. People in the treatment group were 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 five percent level, and the effect size is around 0.15 of a standard deviation.[35]

MTurkers in the treatment group did not change their opinion as to whether removing almost all illegal immigrants from the US would have a positive or a negative impact on the economy, while TNS participants in the treatment group became slightly more likely to state that removing illegal immigrants would not have a major impact on the US 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. However, the effects are not statistically distinguishable.

We also provide some evidence that participants in the treatment group donated more money to a pro-immigration charity than participants in the control group. MTurkers in the treatment group donated on average \$0.44 more to the American Immigration Council than MTurkers in the control group. As shown in Column 4 of Table 3.4, this effect is statistically significant.

[35]We asked the TNS sample some additional questions on the respective contributions of legal and illegal immigrants, for which we find very similar treatment effects (cf. online appendix).

We find that the treatment effect on donations is weaker in the TNS sample. Indeed, participants increase their donations to the American Immigration Council only by seven percent of a standard deviation, which is not statistically significant. Still, it is worth noting that we cannot reject that the treatment effects in the MTurk sample and in the TNS sample are equal. Moreover, if we pool the two samples, we find that our information treatment led to a statistically significant increase in donations of 13 percent of a standard deviation.

Table 3.4: Main Effects: Opinion on Immigrants (Part 2)

	(1)	(2)	(3)	(4)
	No positive effect of Removing all illegal immigrants	**Immigrants produce more advantages**	**Index Opinions**	**Donation**
A: MTurk sample				
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
B: TNS sample				
Treatment	0.090*	0.139***	0.115***	0.070
	(0.049)	(0.048)	(0.040)	(0.056)
FDR-adjusted p-value	[.034]**	[.009]***		
N	1193	1193	1193	1193
Scaled Effect	0.317	0.440	0.380	0.223
C: Pooled				
Treatment	0.075**	0.155***	0.115***	0.133***
	(0.037)	(0.035)	(0.030)	(0.047)
FDR-adjusted p-value	[.02]**	[.001]***		
N	1993	1993	1993	1993
D: Follow-up: MTurk				
Treatment	0.118*	0.139**	0.129**	
	(0.061)	(0.055)	(0.050)	
FDR-adjusted p-value	[.062]*	[.01]***		
N	695	695	695	

Note: 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 Republicans and those who are Democrats. In Panel A, we display the results from the MTurk sample. In Panel B, we display the results from the representative sample. In Panel C, we show results pooling together the MTurk sample and the representative sample. In Panel D we display the results from the follow-up experiment from our MTurk sample. We include the same list of controls as in Table 3.3. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Policy Preferences

For both of our samples, we clearly see that people in the treatment group are less likely to state that there are too many legal and illegal immigrants in the US, as shown in Columns 1 and 2 of Table 3.5. These effects are statistically significant, their effect size is large (approximately 0.25 of a standard deviation), and they persist even four weeks after the main experiment. To a large extent, these results are compatible with the findings from the cross-country experiment presented in section 2. When people learn about the actual proportion of immigrants in their country, they become less inclined to say that there are too many immigrants.

Moreover, we observe that respondents who receive the information become more likely to be in favor of increasing the number of legal immigrants (0.13 of a standard deviation). However, if we look at Table 3.5 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 undocumented immigrants. Similarly, their views on the budget that should be devoted to deporting undocumented immigrants are not affected by the treatment. These effects are small in magnitude (mostly around 0.05 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 for all of our policy preferences (mostly around

0.1 of a standard deviation), and many of them are even statistically significant. However, we cannot reject that the effects on the index are statistically different for the follow-up (p=0.584).

Table 3.5: 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	Green cards	Budget to deport	Legalization	Illegal Immigrants	Policy Preference
A: MTurk sample								
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
B: TNS sample								
Treatment	0.102**	0.240***	0.127**	0.047	0.053	0.003	0.080	0.090***
	(0.046)	(0.049)	(0.050)	(0.052)	(0.048)	(0.051)	(0.051)	(0.031)
FDR-adjusted p-value	[.049]**	[.001]***	[.038]**	[.261]	[.261]	[.594]	[.134]	
N	1193	1193	1193	1193	1193	1193	1193	1193
Scaled Effect	0.40	0.36	0.27	0.08	0.09	0.01	0.16	0.18
C: Pooled								
Treatment	0.160***	0.256***	0.124***	0.061	0.056	0.004	0.060	0.100***
	(0.035)	(0.036)	(0.039)	(0.039)	(0.037)	(0.039)	(0.039)	(0.024)
FDR-adjusted p-value	[.001]***	[.001]***	[.001]**	[.148]	[.173]	[.291]	[.148]	
N	1993	1993	1993	1993	1993	1993	1993	1993
D: MTurk Follow-up								
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

Note: 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 Republicans and those who are Democrats. In Panel A, we display the results from the MTurk sample. In Panel B, we display the results from the representative sample. In Panel C, we show results pooling together the MTurk sample and the representative sample. In Panel D we display the results from the follow-up experiment from our MTurk sample. We include the same list of controls as in Table 3.3. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Moreover, in Table 3.6, we show that, for both the MTurk and the TNS samples, the treatment group is not more likely to sign the online petition on the White House's website in favor of increasing the number of green cards available for immigrants.[36] Similarly, approximately the same fraction of people in the treatment and control group reported both intending to sign and having signed the petition.[37]

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

[37]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. 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. 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.

Table 3.6: Main Effects: Online Petition

	(1)	(2)	(3)	(4)
	Intention to sign	**Self-report: Sign**	**Actual Sign-up**	**Index: Petition**
A: MTurk				
Treatment	0.065	-0.068	-0.036*	-0.001
	(0.064)	(0.054)	(0.019)	(0.055)
FDR-adjusted p-value	[.842]	[.183]	[.165]	
N	802	802	802	802
Scaled Effect	.11	-.16	-	0
Control mean	0	0	.106	0
B: TNS sample				
Treatment	-0.031	0.021	0.002	-0.005
	(0.053)	(0.055)	(0.019)	(0.050)
FDR-adjusted p-value	[1]	[1]	[1]	
N	1193	1193	1193	
Scaled Effect	-0.044	0.033	-	-0.007
Control mean	0	0	0.112	0
C: Pooled				
Treatment	0.004	-0.017	-0.012	-0.006
	(0.041)	(0.040)	(0.017)	(0.038)
FDR-adjusted p-value	[1]	[1]	[1]	
Control mean	0	0	.11	0
N	1993	1993	1993	

Note: 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 Republicans and those who are Democrats. In Panel A, we display the results from the MTurk sample. In Panel B, we display the results from the representative sample. In Panel C, we show results pooling together the MTurk sample and the representative sample. We include the same list of controls as in 3.3. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

To sum up, we find that, for both the TNS and MTurk samples, participants who receive the information develop a more positive attitude towards immigrants and are more willing to increase the number of legal immigrants. However, the treatment did not affect people's policy preferences regarding illegal immigrants.

3.4.3 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.[38] For all of the heterogeneity analysis, we only look at the indices for these families of outcomes. We estimate the following equation, where $inter_i$ refers to the pre-specified interaction variable, and X_i is a vector of pre-determined characteristics:[39]

$$y_i = \pi_0 + \pi_1 Treatment_i \times inter_i + \pi_2 Treatment_i + \pi_3 inter_i + \Pi^T \mathbf{X_i} + \varepsilon_i$$

[38] A precise definition of the different families can be found in Appendix C.

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

Republicans

In Panel A of Table 3.7, we show that, for the pooled sample, 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. Indeed, we observe that Republicans are more likely than other political groups to change their beliefs about immigrants, to become more supportive of policy reforms favoring immigrants, and to accept to sign a pro-immigrant petition. These effects are statistically significant, and are also quite large (0.25 of a standard deviation).[40]

Moreover, these effects are robust to using other measures of political conservatism.[41] For instance, we find that MTurkers who favored Trump or Cruz in the Republican primary respond more strongly to the information treatment. Similarly, participants from the TNS sample who intended to vote for Trump in the presidential election react more strongly to the treatment than people planning to vote for another candidate. We also find that these results are robust to simultaneously including the interaction of treatment with other variables, such as education or measures of biases in beliefs.

In Table A3 in Appendix C, we show the disaggregated results for the heterogeneous effects on policy preferences. We find that the information treatment

[40] We find that these heterogeneous treatment effects become even stronger if we focus exclusively on Democrats and Republicans, and exclude people who belong to neither party.

[41] These additional results (which were not pre-specified) are available upon request.

Table 3.7: Heterogeneous Effects: Pooled

	(1)	(2)	(3)	(4)	(5)
	Beliefs	**Donation**	**Petition**	**Policy Preferences**	**Opinions**
Panel A					
Treatment	0.286***	0.096*	-0.074*	0.055*	0.086**
	(0.031)	(0.055)	(0.044)	(0.028)	(0.035)
Treatment × Republican	0.171***	0.138	0.245***	0.162***	0.105
	(0.058)	(0.104)	(0.082)	(0.053)	(0.066)
Republican	-0.193***	-0.316***	-0.392***	-0.276***	-0.214***
	(0.044)	(0.078)	(0.062)	(0.040)	(0.050)
N	1993	1993	1993	1993	1993
Panel B					
Treatment	0.323***	0.126***	-0.010	0.086***	0.096***
	(0.026)	(0.047)	(0.037)	(0.023)	(0.029)
Treatment × Concerned about immigration	0.044	0.037	0.088*	0.119***	0.064
	(0.037)	(0.066)	(0.053)	(0.033)	(0.040)
Concerned about immigration	-0.333***	-0.238***	-0.166***	-0.478***	-0.570***
	(0.037)	(0.068)	(0.054)	(0.033)	(0.041)
N	1993	1993	1993	1993	1993
Panel C					
Treatment	0.320***	0.132***	-0.035	0.079**	0.086**
	(0.030)	(0.047)	(0.038)	(0.031)	(0.039)
Treatment × Trust in statistics	0.016	0.089**	-0.072*	-0.018	-0.047
	(0.029)	(0.045)	(0.037)	(0.030)	(0.037)
Trust in statistics	0.121***	0.209***	0.266***	0.211***	0.128***
	(0.020)	(0.032)	(0.026)	(0.021)	(0.026)
N	1993	1993	1993	1993	1993
P-value (Tr + Tr×Rep)	0.000	0.007	0.0141	0.000	.0.000
P-value (Tr + Tr× Concerned)	0.000	0.045	0.229	0.000	0.001
P-value (Tr + Tr× Trust Stat)	0.000	0.001	0.055	0.182	0.491
Controls	Y	Y	Y	Y	Y

Note: All of the outcomes are indices. The definition of the indices is in Appendix C. 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 et al., 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 3.3. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

makes Republicans more willing to increase the number of green cards and the number of incoming immigrants (0.3 of a standard deviation). Moreover, as Table A4 in Appendix C clearly shows, treated Republicans become much more likely to report intending to sign and having signed the online petition than non-Republicans. This result can be partly explained by the fact that Republicans have more negative views to begin with, which implies that the information treatment is actually stronger for them.

Initial Attitudes towards Immigrants

In Panel B of Table 3.7, we show that participants from the TNS and MTurk samples who are particularly worried about immigration tend 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. This is also in line with what we had observed in the cross-country experiment. 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.

These findings can be related to the literature on motivated reasoning (Taber and Lodge, 2006) and confirmation bias (Nickerson, 1998). According to those theories, people who receive information which goes against their political convictions should be less willing to update their beliefs than people for whom the information is in line with their political orientation. In some cases, one might even expect to observe a backfire effect (Nyhan and Reifler, 2010), where people's beliefs actually get reinforced in the face of contradictory evidence. In our experiments, we do not see any evidence for such a phenomenon, as Republicans and participants who initially have more negative views on immigrants update their beliefs and policy preferences more than people who have a positive attitude towards immigrants.

Other Heterogeneity

In Panel C of Table 3.7, 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. Overall, we find no consistent evidence that people who trust official statistics respond more strongly to the information treatment.

We also examined heterogeneous treatment effects by people's biases in beliefs with three different pre-specified measures of biases. We find that people with higher biases in beliefs seem to respond more strongly to information. However, in both of our samples, this effect is not statistically significant for most families of outcomes. This could be due to measurement error as we do not know how people weigh 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 the online appendix.[42] Moreover, we find that the size of the bias is negatively correlated with respondents' attention level. Indeed, once we restrict the TNS sample to people who passed the attention screener, the coefficients for the interaction between the treatment and our measures of biases increase. We even observe that the coefficient on the interaction between the size of the bias and treatment is positive and statistically significant for the effect on the index of beliefs about immigrants.

Lastly, we also examine whether trust in the government moderates the size of our estimated treatment effects.[43] Unlike Kuziemko et al. (2015), we do not find any evidence that the participants' level of trust in the government was affected by our information treatment. We also find no strong heterogeneous treatment effects by people's trust in the government. If anything, we find that people who trust the government more respond less to our information treatment.

Machine-Learning Approaches to Heterogeneity

When evaluating heterogeneous treatment effects there are two main challenges: first, there is a large set of covariates which could affect the response to the treatment. Second, many covariates are highly correlated, making it difficult to assess which variables are the most important ones in shaping heterogeneous responses.

[42] https://www.dropbox.com/s/7iym86pc4we3udl/onlineappendix_new.pdf?dl=0

[43] We did not specify this analysis in the pre-analysis plan.

In our pre-analysis plan, we only selected a small number of variables to be used for the heterogeneity analysis, and we observed that the largest heterogeneous treatment effects occurred between Democrats and Republicans. However, political affiliation is correlated with many other variables that we did not examine, and it could be the case that this pattern of heterogeneity was actually caused by one of these other variables. Furthermore, we might have missed some important sources of heterogeneity by only focusing on a select group of variables.

To address these concerns, we use a machine learning algorithm called Classification and Regression Trees (CART), which allows us to "partition the data into subpopulations which differ in the magnitude of their treatment effects" (Athey and Imbens, 2016). Specifically, we build a tree by sequentially splitting the training set into two groups, so as to minimize the mean squared error of the estimated treatment effect for all of the observations in the training set. We then use the cross-validation set to reduce the depth of the tree, which alleviates concerns about overfitting.[44]

In Figure 3.6, we present the trees obtained by the CART algorithm for the four indices which we used in the rest of the heterogeneity analysis. All the variables included in the balance table were used for the construction of the trees.[45] It is particularly striking to note that the algorithm picked political

[44]Here we focus on classification and regression trees as they allow us to assess which variables constitute the major source of heterogeneity with respect to the treatment, but we do not implement "honest" estimation (Athey and Imbens, 2016).

[45]To make the results easier to interpret, we dichotomize all continuous variables. We find very similar results when using continuous variables (results available upon request).

affiliation as the first variable along which to split the data for three out of the four indices. Indeed, for the petition index, the political preferences index and the second index on opinions about immigrants, the CART algorithm found that the largest reduction in the mean squared error of the estimated treatment effect was obtained by first dividing the data into Democrats and Republicans. For the first index of opinions about immigrants, the CART algorithm first chose to divide the data based on whether a person's estimate of the unemployment rate for immigrants was above the median or not, but for the former group, the next split occurred based on political affiliation. This also highlights that prior beliefs about immigration play an important role in shaping how people's perception of immigrants responds to the treatment, while for policy preferences, political affiliation is the major source of heterogeneity.

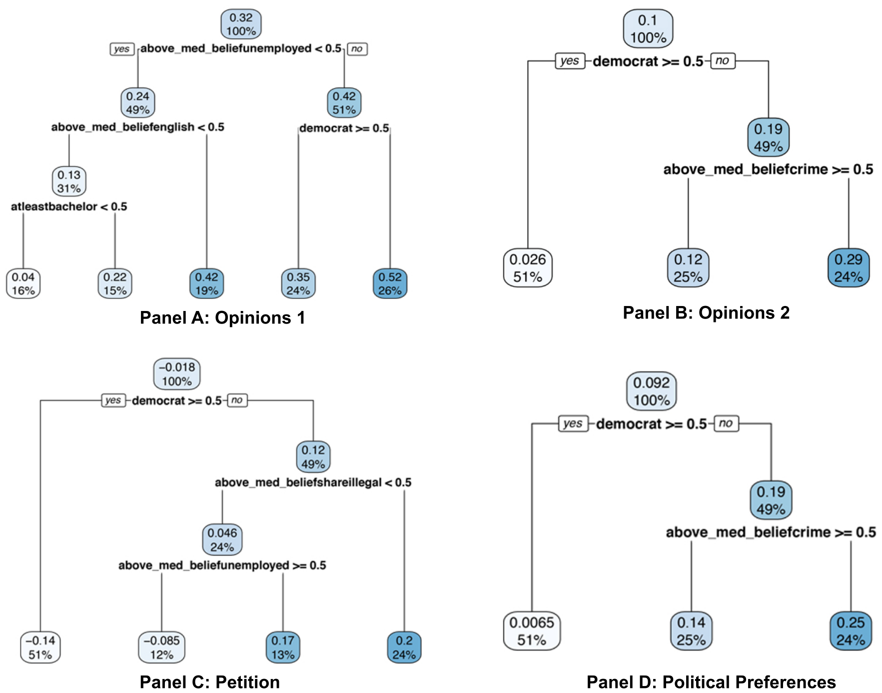

Figure 3.6: Recursive partitioning for heterogeneous causal effects (Athey and Imbens, 2016). In Panel A we display the causal tree for "Opinions 1", an index using our data on perceptions. In Panel B we display the causal tree for "Opinions 2", an index using beliefs about the effects of immigration on the US. In Panel C we show the causal tree for "Petition", an index based on people's intention to sign the petition and their self-reported signing. In Panel D we show the causal tree for "Policy Preferences", an index on people's views on immigration policy. The exact variables to construct the indices are defined in Appendix B. "Democrat" takes value one for people self-identifying as Democrats, and value zero otherwise. "above_med_beliefunemployed" takes value one for respondents with above median beliefs about the share of unemployed immigrants. "above_med_beliefenglish" takes value one for respondents with above median beliefs about the share of immigrants who cannot speak English. "above_med_beliefcrime" takes value one for respondents with an above median belief about the share of incarcerated immigrants. "above_med_beliefshareillegal" takes value one for respondents with an above median belief about the share of illegal immigrants. "atleastbachelor" takes value one for respondents with at least a bachelor degree and zero otherwise.

Persistence of Heterogeneous Effects

In Table 3.8, 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 their policy preferences and their general opinion of immigrants. There is also some indication that people who are worried about immigration still respond 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.

Table 3.8: Heterogeneous Effects: Follow-up

	(1) Opinions 1	(2) Policy Preferences	(3) Opinions 2
Panel A			
Treatment	0.221***	0.067	0.066
	(0.058)	(0.054)	(0.057)
Treatment × Republican	0.044	0.257**	0.250**
	(0.118)	(0.111)	(0.116)
Republican	-0.327***	-0.461***	-0.544***
	(0.089)	(0.084)	(0.088)
N	697	696	696
Panel B			
Treatment	0.221***	0.113**	0.109**
	(0.050)	(0.046)	(0.047)
Treatment × Concerned about immigration	0.052	0.111	0.279***
	(0.091)	(0.084)	(0.086)
Concerned about immigration	-0.497***	-0.700***	-0.841***
	(0.099)	(0.091)	(0.094)
N	697	696	696
Panel C			
Treatment	-0.200	0.170	0.329*
	(0.191)	(0.182)	(0.189)
Treatment × Trust in statistics	0.139**	-0.018	-0.073
	(0.061)	(0.058)	(0.060)
Trust in statistics	0.035	0.093**	0.152***
	(0.043)	(0.041)	(0.043)
N	697	696	696
P-value (Tr + Tr×Rep)	0.010	0.001	0.002
P-value (Tr + Tr×Concerned)	0.008	0.006	0.000
P-value (Tr + Tr×Trust Stat)	.000	0.185	0.363
Controls	Y	Y	Y

Note: All of the outcomes are indices. The definition of the indices is in Appendix C. 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. We include the same list of controls as in Table 3.3. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.4.4 Discussion

Main Effects

We hypothesize that people who update their beliefs about immigrants in the US also change their policy preferences accordingly. To support this claim, we show in Table A12 of Appendix C that our main treatment effects disappear once we include the index for people's post-treatment beliefs about immigrants as a control variable.[46] This finding corroborates our hypothesis that people's beliefs about the characteristics of immigrants are the key mechanism underlying our treatment effect.

Next, we consider different explanations for why the information treatment affects people's attitude towards immigrants, but not their policy preferences. 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 observe that temporal correlations are higher for policy preferences than for attitudes.[47] These results on the stability of preferences over time are displayed in the online appendix.[48] However, this

[46]This analysis was not pre-specified.

[47]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.

[48]`https://www.dropbox.com/s/7iym86pc4we3udl/onlineappendix_new.pdf?`

explanation cannot explain the differential effects for Republicans.

One could also argue that the information treatment changed the way participants viewed current immigrants, but not necessarily future immigrants. Indeed, we provide statistics about immigrants currently living in the United States, whose characteristics could be different from those of incoming immigrants. This could explain why participants in the treatment group seem to develop a more positive image of existing immigrants, although they are not willing to accept more immigrants into the country.

Another related explanation is that the questions on policy preferences are less directly related to the information 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. Participants only change their views when the information given is in direct contradiction with their beliefs. If the information provided is theoretically compatible with their set of beliefs, they will refrain from updating their views. This interpretation would be in line with the existing literature on belief updating, which finds that most people are reluctant to update their opinions based on new information (Taber and Lodge, 2006) and that people update their beliefs in a self-serving manner (Di Tella et al., 2015). It is possible that policy preferences would change if people received information about the effect of immigration policies.

dl=0

Heterogeneous Effects

The fact that Republicans and Democrats update their policy preferences differently after the treatment can be explained by two factors.

First, we observe that Republicans' perceptions of immigrants are more strongly affected by the information treatment than Democrats' perceptions. Once we include in our regression an index summarizing people's perceptions about immigrants post-treatment, the heterogeneous treatment effect between Democrats and Republicans becomes significantly smaller - as shown in Table A13 of Appendix C. Similarly, if we run regressions for Democrats and Republicans separately, we find that the inclusion of the measure of people's perception of immigrants significantly lowers the coefficient on the treatment indicator, both for Republicans and Democrats.

Second, we find that, for a given change in their perceptions about immigrants, Republicans update their policy preferences and attitudes towards immigrants much more than Democrats. In other words, Republicans' political preferences regarding immigration appear to be more elastic to changes in perceptions than those of Democrats. In Table A14 of Appendix C, we show the results from an IV regression for Republicans and Democrats, where we regress the main outcomes on an index of participants' perception of immigrants, which is instrumented by the treatment indicator. This IV regression confirms that effects are stronger for those whose beliefs are changed by the intervention.

3.4.5 Potential Confounds

Priming vs. Information

On the one hand, one could argue that our treatment manipulation alters people's policy preferences through genuine changes in beliefs as a result of information. On the other hand, it could be that our treatment changes people's preferences through channels other than information, such as emotional responses to our treatment or priming on the issue that immigrants are not as bad as people think they are. We leverage the data from the four week follow-up in order to distinguish between these two competing interpretations of our treatment. It seems plausible that treatment effects only persist if the effects work through genuine changes in beliefs (Cavallo et al., 2016).

Since we find evidence of (i) changes in beliefs about statistics on immigrants and (ii) persistence of some of our main treatment effects four weeks after the treatment administration it seems more likely that effects are driven by changes in beliefs.

Experimenter Demand Effects

We are confident that our results are not caused by experimenter demand effects. First of all, online studies have been shown to be less affected by experimenter demand effects (Van Gelder et al., 2010), since participants do not interact at all with the experimenter.

Since our treatment effects persist four weeks after the main experiment, experimenter demand effects are not likely to explain the patterns in our data. Indeed, it seems 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., 2016).[49]

Moreover, the patterns of heterogeneity we observe in the data are not consistent with experimenter demand effects unless demand effects are systematically stronger for Republicans compared to Democrats. We think that such an explanation of differential experimenter demand effects based on political affiliation is unlikely.

Other Potential Confounds

One might also worry that the lack of significant treatment effects on policy preferences is due to low levels of attention. In the TNS sample, participants had to complete a standard attention check at the end of the survey. We observed no difference in attention between the treatment group and the control group, and we can therefore use this measure to test for heterogeneous treatment effects by people's attention level. As can be seen in the online appendix, we find little evidence that the respondents who pay more attention to the questions react more to the information.

[49]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.

3.4.6 Determinants of the Biases

In this section, we try to understand why some people have much more biased beliefs about immigration than others, using our representative online sample.[50] This empirical analysis is related to theoretical work on how people form beliefs and stereotypes (Bordalo et al., 2016). For each type of belief, we regress the bias on a large set of demographics, such as income, education, age, as well as on a set of regional controls such as the share of immigrants in the respondent's zip code area.[51]

In Table 3.9, we provide evidence that people who live in zipcode areas with a large share of immigrants have more biased beliefs about the national share of immigrants (both legal and illegal), and about certain characteristics of immigrants, such as their propensity to commit crimes and to be unemployed. One reason why they might over-estimate the national average is that their beliefs are shaped by immigration at the local level.

[50]In this section, we do not strictly follow the pre-analysis plan. The results from the pre-specified regressions are available upon request.

[51]We use data from the 2007-2011 American Community Survey, which is to the best of our knowledge the only recent Census dataset containing data on the share of immigrants at the zip code level.

Table 3.9: Determinants of Biases in Beliefs: TNS Sample

	(1)	(2)	(3)	(4)	(5)	(6)
			Size of Bias in Beliefs about immigrants			
	Crime	No English	Unemployed	Share Immigrants	Share Illegal Immigrants	Index
Share of immigrants at respondent's zip code	0.160***	0.111	0.134**	0.243***	0.261***	0.008***
	(0.056)	(0.068)	(0.061)	(0.057)	(0.057)	(0.002)
State: share of immigrants who do not speak English	-0.102	0.364	-0.153	0.037	0.150	0.002
	(0.235)	(0.283)	(0.256)	(0.241)	(0.241)	(0.009)
State: share of immigrants who are unemployed	1.719	-0.389	1.776	1.821	1.219	0.055
	(1.217)	(1.467)	(1.323)	(1.247)	(1.247)	(0.044)
Fox News	2.911*	4.501**	2.860	1.472	1.912	0.116*
	(1.667)	(2.011)	(1.813)	(1.708)	(1.708)	(0.060)
Republican	0.899	3.219*	1.848	0.778	0.083	0.057
	(1.473)	(1.777)	(1.602)	(1.510)	(1.509)	(0.053)
Christian	2.825**	2.905*	1.639	3.999***	2.701*	0.122**
	(1.411)	(1.702)	(1.535)	(1.446)	(1.446)	(0.051)
At least bachelor	-1.549	-4.623***	0.860	-4.203***	-5.538***	-0.130***
	(1.389)	(1.675)	(1.510)	(1.423)	(1.423)	(0.050)
Log income	0.197	-0.036	0.384	0.145	-0.023	0.006
	(0.301)	(0.363)	(0.327)	(0.308)	(0.308)	(0.011)
Age	-0.047	-0.198	-0.377	0.207	0.296	-0.001
	(0.352)	(0.425)	(0.383)	(0.361)	(0.361)	(0.013)
Age squared	-0.004	0.000	0.002	-0.007	-0.007*	-0.000
	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.000)
Male	2.437*	-2.610	1.043	-3.870***	-3.047**	-0.051
	(1.323)	(1.595)	(1.438)	(1.355)	(1.355)	(0.048)
Hispanic	-3.816	-0.245	-1.147	-3.468	0.063	-0.077
	(6.765)	(8.157)	(7.355)	(6.931)	(6.930)	(0.245)
Asian	3.775	7.697	12.900*	7.564	8.473	0.346
	(6.801)	(8.201)	(7.395)	(6.968)	(6.967)	(0.246)
Black	2.998	-3.340	3.817	-0.273	8.946	0.112
	(6.436)	(7.761)	(6.997)	(6.594)	(6.593)	(0.233)
White	0.486	0.925	0.995	0.166	3.469	0.053
	(6.050)	(7.295)	(6.578)	(6.199)	(6.198)	(0.219)
Unemployed	-5.542**	-5.982**	-2.367	-2.229	-3.472	-0.168**
	(2.239)	(2.700)	(2.435)	(2.294)	(2.294)	(0.081)
Born in the US	1.100	5.739	5.049	-7.045**	1.791	0.050
	(3.328)	(4.013)	(3.619)	(3.410)	(3.409)	(0.121)
N	1131	1131	1131	1131	1131	1131
R^2	0.073	0.038	0.048	0.097	0.087	0.083

Note: Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Turning to demographics, we show that more educated people tend to have less biased beliefs about the share and characteristics of immigrants. Male respondents have less biased beliefs about the share of legal and illegal immigrants than female participants. We find no significant conditional correlation between being a Republican and the size of the biases. But, we do observe that individuals who watch Fox News have more biased beliefs about immigrants.[52]

3.5 Conclusion

We show that providing information about immigration affects people's attitude towards immigrants. Participants in the treatment group update their beliefs about immigrants, develop a more positive attitude towards them and are more willing to increase the number of legal immigrants. However, on average, participants who receive the information treatment do not become more supportive of undocumented immigrants. They do not become more willing to sign a petition in favor of immigration reform, and their self-reported policy preferences regarding illegal immigration remain broadly unchanged.

In our two online samples, 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 generally more supportive of immigration. Similarly, we observe that people who are initially more worried about immigration react more to

[52]The results on the determinants for biases in beliefs for the MTurk sample can be found in Table A11 of Appendix C.

the information, and they update their views on immigration more than people who are less worried about immigration. Using the MTurk sample, we show that the treatment effects remain similar four weeks after the main experiment.

Future research should extend 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 or other sources). Second, we need to get a better understanding of how people form their political attitudes, and how they process factual information compared to emotionally loaded content. Answering these questions will be necessary to find the most effective ways of fighting people's misinformation on important political issues, such as immigration.

Our research has potentially important policy implications. In particular, the government could disseminate information about immigrants in order to reduce people's biases. Our results on heterogeneous treatment effects suggest that targeting certain subgroups of the population could increase the effectiveness of information interventions. Specifically, our results suggest that targeting individuals with the most negative views on immigration would be the most effective way of changing people's attitudes towards immigrants.

Conclusion

This thesis attempted to elucidate, using experimental methods, how relative concerns and social comparisons affect people's pro-social and anti-social behaviour, and whether providing people with correct information about immigrants would change their political attitudes and their policy preferences on immigration.

The first chapter showed that people display more solidarity towards those who earned as much as them in situations where people are paid different amounts for the same work than in situations where everyone is paid the same sum of money. The second chapter tackled a related issue, and provided some evidence that individuals who learn that they are lower or higher in the income distribution than they thought do not become more generous, more trusting, more honest and more reciprocal than people who are not given any information about their relative income. Finally, the third chapter examined whether providing information to people about immigrants affected their policy preferences on immigration, and concluded that the information treatment did improve people's opinions about immigrants, but that it did not change their policy preferences meaningfully. However, Republicans and individuals who are worried about immigration respond more strongly to the treatment, and not only do their opinions about immigrants improve, but their policy preferences also become more pro-immigration.

The present work could be extended in various ways. First of all, I could examine how inequality affects other types of anti-social and pro-social behaviour, using the same experimental design. This would allow me to have a more comprehensive understanding of how moral behaviour is shaped by the presence of income inequality. Moreover, it would be particularly interesting to test whether the solidarity mechanism at play in the first experiment could also be detected in less stylised settings.

Future research could also extend our understanding of how people form their political opinions on immigration, and of how these opinions can be changed. For instance, we could examine whether anecdotes and emotional content have a more long-lasting impact on people's political attitudes and policy preferences than factual information. In addition, we could study what would be the best medium to convey information on immigration or other political issues to voters. Finally, it would be valuable to conduct some field experiments on this topic in order to avoid potential experimenter demand effects.

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Appendix A

Here are the instructions that the participants saw when they took part in the experiment. The text in italics is only there to clarify which parts of the website could be seen by each kind of participant. People taking part in the study therefore did not see these comments.

Page 1: General Information

Behavioural study in economics

Welcome to this web-based experiment that will examine how people behave in various social settings. Before taking part in this study, please read the participant information sheet carefully and click "Next" once you have done so. The next page will contain the consent form which you will have to fill out to participate in the experiment.

Participant information sheet

This study is conducted by Alexis Grigorieff, a Masters student at the University of Oxford, under the supervision of Dr David Huffman, Professor Margaret Stevens and Professor Tony Atkinson. This research has been reviewed by, and received ethics clearance through the University of Oxford Central University Research Ethics Committee. No deception is involved and the study does not pose any risk to the participants.

Participation in the study typically takes 20 minutes and is strictly anonymous. Participants will first answer a series of questions, which will all have the same format. For each question, participants will be asked to decode an encrypted word. In order to decode the encrypted word, participants will be given a table showing which letter is coded by which. This task should take around 10 minutes. Afterwards, participants will be informed of how much they are going to be paid for the task they performed, as well as how much other people in their group will be paid.

Once this is over, the second stage of the experiment will begin. Participants will be asked to choose between various actions. The final amount the participants will receive will depend on the choices made by all of the participants in the second stage.

Participants will also be asked to answer a few questions about the experiment they have just participated in, as well as a set of demographic questions. Filling out the questionnaire shouldn't take more than 10 minutes.

In order to be paid, it is necessary to finish the survey and get the confirmation code. If you complete the survey and obtain the confirmation code, you will receive a fixed payment of $1.50, and on top of that however much you ended up making in the second stage.

Each person is only allowed to participate in the experiment once. If you encounter a technical problem, please do not restart the experiment, but contact the principal investigator, Alexis Grigorieff at mphil.economics.oxford@gmail.com. All responses are treated as confidential, and all the data will be pooled and published in aggregate form only.

If participants have further questions about this study or their rights, or if they wish to lodge a complaint or concern, they may contact the principal investigator, Alexis Grigorieff at mphil.economics.oxford@gmail.com.

Page 2: Consent Form

Please enter your Mechanical Turk worker ID: ____________

Consent form

- I have read the participant information sheet.
- I have had the opportunity to ask questions about the study.
- I understand that I may withdraw from the study at any time.
- I understand that this project has been reviewed and approved by the University of Oxford Central University Research Ethics Committee.
- I understand how personal data will be published and stored.
- I understand how to raise a concern or make a complaint.
- I understand that I can only participate in this experiment once.

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

☐ I agree

☐ I do not agree

Page 3: Groups

Participants who are assigned to the inequality groups will see the following page:

You have now been allocated to a group of six participants (including yourself). All six of you will now perform a decoding task that should take approximately 10 minutes to complete. Everyone in the group will perform the same task. In total, you will be asked to answer six questions. However, not everyone in the group will receive the same amount for completing the task:

- two people will get $1
- two people will get $3
- two people will get $5

Who gets what will be determined randomly.

Participants who are assigned to the equality groups will see the following page, where x is either 1, 3 or 5, depending on the kind of of equality group they are in:

You have now been allocated to a group of six participants (including yourself). All six of you will now perform a decoding task that should take approximately 10 minutes to complete. Everyone in the group will perform the same task.

In total, you will be asked to answer six questions. Everyone in the group will receive the same amount for completing the task: $\$x$.

Page 4: Example of Task

On this page, you will find an example of the kind of task you will be asked to perform. The main purpose of the task is to decode a series of encrypted words. You will be given a table, with an ordered alphabet on the first line, and a scrambled alphabet on the second line, like the one below.

A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U	V	W	X	Y	Z
S	N	B	H	F	K	C	M	X	Y	W	D	A	U	Z	E	R	J	L	T	O	Q	P	I	V	G

Then, you will be given an encrypted word, which you will need to decode. For instance, you might get the following word: AFHXS. To decode the word, you need to find each letter of the encrypted word on the second line of the table, and see which letter is just above it. This letter will be the letter of the actual word. In our example, the letter just above A is M, the letter just above F is E, the letter just above H is D, the letter just above X is I and the letter just above S is A. When you put all the letters together, you find that the decoded word is MEDIA.

Pages 5-10: Task

Here is an example of the kind of page participants see when they perform the decoding task.

You will find below a word that you need to decode. In order to decode this word, please follow the method given below. Take the first letter of the encrypted word. Find it in the second line of the table and look what letter is just above it on the first line. The corresponding letter is the decrypted letter. Do the same for all the letters in the word.

A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U	V	W	X	Y	Z
O	R	V	Q	U	W	E	X	P	N	L	D	T	A	I	Y	Z	M	G	F	C	B	H	S	J	K

Word to decode: YDOAFUQ

What is the decoded word? ____________

Page 11: Earnings

The participants will see one of the following pages, depending on what kind of group they are in, and how much they earned in the first stage.

Inequality Groups (assuming that the person earned $1)

Thank you very much for completing this task! The amount that you have currently earned is $1. In your group, one other person has earned $1, while two received $3 and two $5.

Equality Groups (where x is either 1, 3 or 5)

Thank you very much for completing this task! Like the other five members of your group, the amount that you have currently earned is $\$x$.

Page 12: Stage 2

Inequality Groups

Out of the six people in your group, two people shall be chosen at random, and we shall refer to those two people as person A and person B. Person A will have the possibility of taking some of the money person B has earned in the previous stage, where everyone had to decode a series of words.

Person A will therefore earn the amount that he or she has received for completing the decoding task plus the amount he or she decided to take from person B. On the other hand, person B will receive the amount that he or she got for completing the decoding task minus the amount that person A decided to take from him or her. The other four participants will receive the amount they earned for completing the decoding task. However, at this stage, nobody has been picked yet to play the roles of person A and person B, and so all of you have the same chance of being picked to be person A.

You will therefore be asked to indicate how much money (if any) you would like to take from person B, if you were selected to be person A. Given that person B hasn't been picked yet, person B could be anyone from your group, and you will need to indicate how much money (if any) you would take from someone who earned \$1, someone who earned \$3 and someone who earned \$5.

It is only after everyone in your group completes the survey that person A and person B will be chosen at random.

Equality Groups (where x is either 1, 3 or 5)

Out of the six people in your group, two people shall be chosen at random, and we shall refer to those two people as person A and person B. Person A will have the possibility of taking some of the money person B has earned in the previous stage, where everyone had to decode a series of words.

Person A will therefore earn the amount that he or she has received for completing the decoding task plus the amount he or she decided to take from person B. On the other hand, person B will receive the amount that he or she got for completing the decoding task minus the amount that person A decided to take from him or her. The other four participants will receive the amount they earned for completing the decoding task. However, at this stage, nobody has been picked yet to play the roles of person A and person B, and so all of you have the same chance of being picked to be person A.

You will therefore be asked to indicate how much money (if any) you would like to take from person B, if you were selected to be person A. Given that person B hasn't been picked yet, person B could be anyone from your group, and you will need to indicate how much money (if any) you would take from someone who earned \$$x$.

It is only after everyone in your group completes the survey that person A and person B will be chosen at random.

Page 13: Stage 2 (continued)

Participants in the inequality groups will see all three questions, while participants in the equality groups will only see the one relevant to them:

If you end up being chosen to take money from someone in your group who earned \$5, how much would you take? You must click on the slider to give your answer, even if you want to leave it where it is.

If you end up being chosen to take money from someone in your group who earned \$3, how much would you take? You must click on the slider to give your answer, even if you want to leave it where it is.

If you end up being chosen to take money from someone in your group who earned \$1, how much would you take? You must click on the slider to give your answer, even if you want to leave it where it is.

Page 13: Stage 3

You will now be asked to answer a few questions on the experiment you have just participated in. As long as you finish the whole survey and get the confirmation code, you will receive a fixed payment of \$1.50 plus the amount that you will have earned in the previous stage (which will be determined by your answers and those of your fellow group-members in the previous stage).

Page 14: Stage 3 (continued)

Participants in the inequality groups will see all three questions, while participants in the equality groups will only see the one relevant to them:

Please give a reason for choosing the amount you picked when you were asked how much money you would take from someone in your group with $5. One or two sentences should be enough.

____

____

____

Please give a reason for choosing the amount you picked when you were asked how much money you would take from someone in your group with $3. One or two sentences should be enough.

____

____

____

Please give a reason for choosing the amount you picked when you were asked how much money you would take from someone in your group with $1. One or two sentences should be enough.

____

____

____

Page 15: Stage 3 (continued)

How satisfied are you with the experiment so far?

☐ Very dissatisfied

☐ Dissatisfied

☐ Somewhat dissatisfied

☐ Neutral

☐ Somewhat satisfied

☐ Satisfied

☐ Very satisfied

How fairly would you say you have been paid for completing the decoding task?

☐ Unfairly

☐ Rather unfairly

☐ Rather fairly

☐ Fairly

Page 16: Stage 3 (continued)

Participants in the inequality groups will see all three questions, while participants in the equality groups will only see the one relevant to them:

Imagine now that you have been selected to have some of your earnings taken from you by another participant in your group.

How much do you think someone with $5 would take from you on average?

How much do you think someone with $3 would take from you on average?

How much do you think someone with $3 would take from you on average?

Page 17: Stage 3 (continued)

Participants in the inequality groups will see all three questions, while participants in the equality groups will only see the one relevant to them:

Imagine now that you have been selected to have some of your earnings taken from you by another participant in your group.

What would be a fair amount for someone with $5 to take from you?

What would be a fair amount for someone with $3 to take from you?

What would be a fair amount for someone with $1 to take from you?

Page 17: Stage 3 (continued)

People in the inequality will be asked the following question, where x stands for the amount that they earned in the first stage.

Imagine now that you had been allocated to a group of six people (including you) where everyone earns $*x* for performing the decoding task. Once again, one person will be picked at random (person A), and that person will have the opportunity to take some money from another member of the group, who will also be picked at random (person B). In this question, you will be asked what would you do if you were chosen to be person A.

If you end up being chosen to take money from someone in your group who earned $\$x$, how much would you take?

People in the \$1-equality groups will be asked the following question, and participants in \$3- and \$5-equality groups will be asked a variant of this question, where the relevant modifications have been made.

Imagine now that you had been allocated to a group of six people (including you) where not everyone earns the same amount for performing the decoding task. You still earn \$1, and so does another person in the group. Of the remaining four people in the group, two earn \$3, and two earn \$5.

Once again, one person will be picked at random (person A), and that person will have the opportunity to take some money from another member of the group, who will also be picked at random (person B). In this question, you will be asked what would you do if you were chosen to be person A, and person B happened to have earned the same amount as you.

If you end up being chosen to take money from someone in your group who earned \$1, how much would you take?

Page 18: Additional Questions

You will now be asked to answer some additional questions on how you would behave in a few other hypothetical situations.

People in the inequality groups will see the following page:

Consider now that you are are in the same group as you were previously in, when you performed the decoding task. Therefore, the group has, counting yourself, two people who earned \$1, two who earned \$3 and two who earned \$5.

You are given an extra \$6, while the other participants receive nothing. Everyone in the group knows that you have received \$6. You will have the possibility of giving some of those \$6 to another participant in your group, who will be picked at random.
Imagine that you are paired up with someone with \$1, how much of your \$6 would you give to that person?

Imagine that you are paired up with someone with \$3, how much of your \$6 would you give to that person?

Imagine that you are paired up with someone with \$5, how much of your \$6 would you give to that person?

People in the \$1-equality groups will be asked the following question, and participants in \$3- and \$5-equality groups will be asked a variant of this question, where the relevant modifications have been made.

Consider now that you are are in the same group as you were previously in, when you performed the decoding task. Therefore, the group has, counting yourself, six people who all earned \$1.

You are given an extra \$6, while the other participants receive nothing. Everyone in the group knows that you have received \$6. You will have the possibility of giving some of those \$6 to another participant in your group, who will be picked at random.

Imagine that you are paired up with someone with \$1, how much of your \$6 would you give to that person?

Page 19: Additional Questions (continued)

Imagine now that you are paired up with someone from your group who has earned the same amount as you in the first stage, where you decoded various words.

Imagine that you are given an extra \$4, while your partner is not given anything. Your partner knows that you haven been given an extra \$4. You then have the possibility of sending part of your \$4 to your partner. The amount that you will send will be multiplied by three, and your partner will have the option of sending back to you some of that money.

For instance, if you send \$3 and keep \$1, your partner will receive 3× \$3 = \$9. If he or she chooses to send back \$4 to you, he or she will end up with \$9−\$4=\$5, while you will get \$1+\$4=\$5.

How much of your \$4 would you send to your partner?

Page 20: Questionnaire

The experiment is almost over now. The last thing that you will be asked to do is to fill out a short general information questionnaire. This shouldn't take more than five minutes.

Which of these describes you more accurately?

☐ Male

☐ Female

☐ Other: ____________

☐ Prefer not to disclose

What year were you born in?

What is the highest level of education you have completed?

☐ 12^{th} grade or less

☐ Graduated high school or equivalent

☐ Some college, no degree

☐ Associate degree

☐ Bachelor's degree

☐ Post-graduate degree

Which of these describes your current situation most accurately?

☐ Employed full-time

☐ Employed part-time

☐ Unemployed and looking for a job

☐ Unemployed but not looking for a job

☐ Retired

☐ Other: ____________

How satisfied are you with the financial situation of your household?

☐ Very dissatisfied

☐ Dissatisfied

☐ Neutral

☐ Satisfied

☐ Very satisfied

What is your best estimate of your total personal income, before taxes and deductions, from all sources during the year ending December 31, 2013?

☐ Less than $5000

☐ From $5000 to $9,999

☐ From $10,000 to $14,999

☐ From $15,000 to $19,999

☐ From $20,000 to $24,999

☐ From $25,000 to $29,999

☐ From $30,000 to $34,999

☐ From $35,000 to $39,999

☐ From $40,000 to $44,999

☐ From $45,000 to $49,999

☐ From $50,000 to $54,999

☐ From $55,000 to $59,999

☐ From $60,000 to $64,999

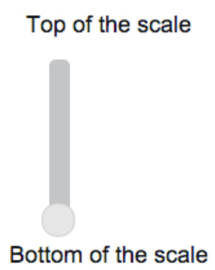
☐ From $65,000 to $69,999

☐ $70,000 or more

☐ I don’t remember

☐ Prefer not to disclose

Think of this scale as representing where people stand in the United States. At the top of the scale are the people who are the best off - those who have the most money, the most education and the most respected jobs. At the bottom are the people who are the worst off – who have the least money, least education and the least respected jobs or no job. The higher up you are on this scale, the closer you are to the people at the very top; the lower you are, the closer you are to the people at the very bottom.



What category would best describe your political orientation?

☐ Extremely liberal

☐ Very liberal

☐ Slightly liberal

☐ Neutral

☐ Slightly conservative

☐ Very conservative

☐ Extremely conservative

☐ Other: ______________

What is your average hourly wage on Amazon Mechanical Turk?

Pick the category that describes you best:

☐ Mechanical Turk is my main source of income

☐ I work on Mechanical Turk to supplement my income

☐ I work on Mechanical Turk as a hobby

☐ Other: ____________

What is the minimum hourly wage that you would be willing to work for on Amazon Mechanical Turk?

In the last 30 days, how many other behavioural experiments have you participated in?

☐ None

☐ From 1 to 5

☐ From 6 to 10

☐ From 11 to 20

☐ More than 20

All things considered, how satisfied are you with your life as a whole these days?

☐ Very dissatisfied

☐ Dissatisfied

☐ Neutral

☐ Satisfied

☐ Very satisfied

Please indicate how much you agree with the following five statements.

	Strongly disagree	Disagree	Slightly disagree	Neutral	Slightly agree	Agree	Strongly agree
In most ways my life is close to my ideal.	☐	☐	☐	☐	☐	☐	☐
The conditions of my life are excellent.	☐	☐	☐	☐	☐	☐	☐
I am satisfied with my life.	☐	☐	☐	☐	☐	☐	☐
So far I have gotten the important things I want in life.	☐	☐	☐	☐	☐	☐	☐
If I could live my life over, I would change almost nothing.	☐	☐	☐	☐	☐	☐	☐

To what extent do you agree with the following statements?

	Strongly disagree	Disagree somewhat	Agree somewhat	Strongly agree
In general, you can trust people.	☐	☐	☐	☐
Nowadays, you can't rely on anybody.	☐	☐	☐	☐
When dealing with strangers, it's better to be cautious before trusting them.	☐	☐	☐	☐

How much do you trust strangers you meet for the first time?

☐ Not at all

☐ Not much

☐ Quite a bit

☐ Very much

Page 21: Confirmation Code

The survey is practically over now! Please copy or write down the code below before submitting the survey. You will be asked to enter it on the Mechanical Turk platform before submitting the HIT. This code will serve as a proof that you have completed the survey.

A code is randomly generated for each participant.

Page 22: End of Survey

Thank you very much for completing our survey! If there are any remarks that you would like to make or clarifications that you would like to obtain, please send me an email at mphil.economics.oxford@gmail.com.

What's next?
Please go back to the Amazon Mechanical Turk website and enter the confirmation code that you received on the previous page in the appropriate box before submitting the HIT. This confirmation code will show that you have completed the survey. As a reminder, your confirmation code is: *confirmation code*.

Once I'll have gathered the data, I will randomly select (using a computer program) the person in your group who will have the opportunity to take some money from someone else in the group, as well as the person who will have some money taken from them. Everyone else in the group will be paid the amount that was shown to them after having completed the decoding task.

If you are randomly selected to take some money from your partner, you will earn the amount that you received for completing the decoding task plus the amount you decided to take from your partner. On the other hand, if you are randomly picked to have some money taken from you, you will be paid the amount you got for completing the decoding task minus the amount your partner will have decided to take from you.

In any case, you will at least get a payment of \$1.50, which corresponds to the show-up fee. (It is however likely that your payment will be much larger than this figure.) The payments will be made within 7 days.

Appendix B

Here are the instructions that the participants saw when they took part in the experiment. The text in brackets is only there to clarify which parts of the website could be seen by each kind of participant. People taking part in the study therefore did not see these comments.

Income

- What was your household income before taxes in 2015? Please enter the amount in dollars in the text box below. [text box: minimum of 0, has to be a positive integer]

  Note: A household consists of all the people who occupy a housing unit, such as a house or a flat. Income is comprised of earnings, unemployment compensation, social security, veterans' payments, survivor benefits, disability benefits, pension or retirement income,interest and dividends, alimony and child support, financial assistance from outside of the household and other income.

- According to the 2015 American Population Survey, what percentage of US households earned less than your household? Please select the appropriate percentage using the slider. If you correctly guess the percentage (within three percentage points), you will receive a bonus of 10 cents. [slider from 0 to 100, increments of 1.]

Treatment [Only for the Treatment group]

[People in the treatment group who over-estimated their relative income will receive the following message:]

> *Actually, you overestimated your relative position in the income distribution. In reality, you are relatively poorer than you thought. In other words, you are closer to the bottom of the income distribution than you thought. You currently earn significantly less than what you would need to be at the position you thought you occupied.*

[People in the treatment group who under-estimated their relative income will receive the following message:]

> *Actually, you underestimated your relative position in the income distribution. In reality, you are relatively richer than you thought. In other words, you are closer to the top of the income distribution than you thought. You currently earn significantly more than what you would need to be at the position you thought you occupied.*

[People in the treatment group who correctly estimate their relative income will receive the following message:]

> *Actually, you gave a correct estimate of your position in the income distribution.*

Manipulation Check [Only for the Treatment group]

- Now that you have received this feedback, what percentage of US households do you think earned less than you, according to the 2015 Current Population Survey? Please select the appropriate percentage using the slider. [Slider from 0 to 100, increments of 1]

Manipulation Check (1 item)

- How satisfied are you with your position in the income distribution? [Very dissatisfied, Dissatisfied, Somewhat dissatisfied, Neutral, Somewhat satisfied, Satisfied, Very satisfied]

Coinflip

For this game you need a coin. Please go and get a coin. Your coin has one side showing the head of a person, and another one showing something else. The side with the head will be referred to as "Heads", while the other side will be referred to as "Tails". In this game, you will be asked to toss your coin four times, and to count the number of times "Heads" comes up. For each "Heads" that comes up, you will receive 10 cents. For example if you toss three times

"Heads" you will receive 30 cents.

Now, please toss the coin four times, and count the number of times "Heads" comes up. Please do not toss the coin more than four times. How many times did "Heads" come up? [0, 1, 2, 3, 4]

Dictator

We will now ask you to complete a game in which there are two players, whom we shall refer to as person C and person D. You will play the role of person C. At the beginning of the game, person C receives $100, while person D receives nothing. Then, person C can choose how much money to give to person D. Once everyone has completed the survey, we will randomly choose one participant in our survey who played the role of person C, and we will implement their choice.

As person C, how much money would you like to give to person D? [Slider from 0 to 100, increments of 1.]

Negative Reciprocity

We will now ask you to complete a game in which there are two players, whom we shall refer to as person 1 and person 2. You will play the role of person 2. At the beginning of this game, person 1 receives $100, while person 2 receives nothing. Then, person 1 has to make an offer to person 2 on how to split the

$100. Person 2 chooses either to accept the offer made by person 1, or to refuse it. If person 2 refuses the offer, both players receive nothing. If person 2 accepts the offer, each player receives the amount specified in the offer. Once everyone has completed the survey, we will randomly choose one participant in our survey who played the role of person 2, and we will implement their choice. We will also choose one participant who played the role of person 1 and we will implement their choice.

Please use the slider to specify the minimum amount of money that player 1 needs to offer you in order for you to accept the offer. [slider from 0 to 100, increments of 1.]

Trust

We will now ask you to complete a game in which there are two players, whom we shall refer to as person A and person B. You will play the role of person A. Person A and person B start with $50 each. Then, person A can choose to send some money to person B. Person B will receive 3 times the amount sent by person A. Then person B will have to choose how much money to send back to person A. For example, imagine that person A sends $12 to person B, then person B will receive 3x$12=$36. If person B decides to send back $16 to person A, then person A will end up with $50-$12+$16=$54, while person B will end up with $50+$36-$16=$70. Once everyone has completed the survey, we will randomly choose one participant in our survey who played the role of person A, and we will implement their choice. We will also choose one participant who played the role of person B and we will implement their choice.

As person A, how much money would you send to person B? [slider from 0 to 50, increments of 1.]

Trustworthiness

[X represents the amount which the participant sent to his or her partner on the previous page. The X needs to be replaced with the actual amount which was sent.]

On the previous page, you specified that you will send X to person B, if you are selected to play this game. This amount will then be multiplied by 3, so person B will receive 3X.

How much money do you think person B would send back to you? [Slider from 0 to 3X, increments of 1.]

APPENDIX C

Experimental Instructions: Main Experiment

Description

This study is conducted by researchers from Bocconi University and the University of Oxford. This research has received ethics clearance by the ethics committees of the University of Oxford and Bocconi University. No deception is involved and the study does not pose any risk to the participants. Participants will be asked to answer a few questions about their preferences, as well as a set of demographic questions. Participation in the study typically takes 10 minutes and is strictly anonymous. During the experiment, you will have the option of donating money to a charity. Your choice will not affect your participation in the experiment.

In order to be paid, it is necessary to finish the survey. If you complete the survey, you will receive a fixed payment of $1.30. On top of that, you might receive a bonus. Each person is only allowed to participate in the experiment once. If you encounter a technical problem, please do not restart the experiment, but contact us at `research.dipmkt@unibocconi.it`. If participants have further questions about this study or their rights, or if they wish to lodge a complaint or concern, they may contact us at `research.dipmkt@unibocconi.it`.

Consent Form

- I have read the information provided on the previous page.
- I have had the opportunity to ask questions about the study.
- I understand that I may withdraw from the study at any time.
- I understand that this project has been reviewed and approved by the ethics committees of the University of Oxford and of Bocconi University.
- I understand how to raise a concern or make a complaint.
- I understand that I can only participate in this experiment once.
- I understand that close attention to the survey is required for my responses to count.

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

Pre-Treatment Characteristics

Personally, how much trust do you have in the official statistics in the U.S., such as the statistics on immigration? [No trust at all, very little trust, some trust, a lot of trust, complete trust]

In the past 12 months, how many petitions have you signed? [number]

We will now ask you for your opinion on some of the issues facing the United States today. Please answer all of the questions carefully. What do you think are the three most important issues facing the United States today? Please select the issues from the list below, and drag them to the appropriate position. [Items: The way income and wealth are distributed in the U.S., The economy, Terrorism, Healthcare policy, Immigration, Foreign affairs, Race relations The way government operates in Washington, The environment]

How worried, if at all, are you about the level of immigration into the U.S.? [Very worried, Fairly worried, Not very worried, Not worried at all]

How concerned are you about the following aspects of immigration? [Not at all concerned, Slightly concerned, Moderately concerned, Very concerned, Extremely concerned]

- Number of legal immigrants currently in the U.S.
- Lack of integration.
- Burden on social services.
- Criminal activity.

- Number of illegal immigrants currently in the U.S.

To what extent do you agree with the following statement? [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]
On balance, immigrants have a negative impact on American society as a whole.

Beliefs about immigrants

On the next pages, we will ask you a series of factual questions on immigration in the United States. Specifically, we will test your knowledge of various statistics on immigration. In case your response is within three percentage points of the true value, you will earn an extra 10 cents. Please note that you will only have 25 seconds to answer each question.

The statistics mentioned in the following questions mainly come from the American Community Survey, which is a nationally representative survey of the population living in the U.S., conducted by the U.S. Census Bureau.

According to the American Community Survey, what percentage of the total U.S. population are immigrants? By immigrants, we refer to people who were not born in the U.S. [slider]

How confident are you that your answer is correct? [very confident, confident, not very confident, not confident at all]

According to the Department of Homeland Security, what percentage of the total U.S. population are illegal immigrants? [slider]

How confident are you that your answer is correct? [very confident, confident, not very confident, not confident at all]

According to the American Community Survey, about 7 percent of all American citizens are unemployed. What percentage of immigrants are unemployed? Please use the slider to select your answer. [slider]

How confident are you that your answer is correct? [very confident, confident, not very confident, not confident at all]

According to the American Community Survey, 3 percent of all American citizens aged between 18 and 39 are incarcerated. Of all the immigrants living in the U.S. who are between 18 and 39 years old, what percentage are incarcerated? Please use the slider to select your answer. [slider]

How confident are you that your answer is correct? [very confident, confident, not very confident, not confident at all]

According to the American Community Survey, what percentage of immigrants can't speak English? Please use the slider to select your answer. [slider]

How confident are you that your answer is correct? [very confident, confident, not very confident, not confident at all]

For the control group only

Thank you very much for taking part in this short quiz!

We will now ask you a series of questions regarding your opinions on immigrants and immigration policies.

For the treatment group only

Thank you very much for taking part in this short quiz!

We will now go over your answers, and we will tell you the correct answer to each of the questions. Please pay close attention to the information provided, as you will have to retake the same quiz at some later point.

Information Treatment

You estimated that X percent of the total U.S. population are immigrants. According to the American Community Survey, 13 percent of the total U.S. population are immigrants.

You estimated that X percent of the total U.S. population are illegal immigrants. According to the Department of Homeland Security, 3 percent of the total U.S. population are illegal immigrants.

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

You estimated that X percent of immigrants aged between 18 and 39 are incarcerated. According to the American Community Survey, 2 percent of immigrants aged between 18 and 39 are incarcerated.

You estimated that X percent of immigrants cannot speak English. According to the American Community Survey, 8 percent of immigrants cannot speak English.

In the two tables below, you will find a summary of the correct answers to the different questions. Please take the time to learn the following statistics, as you will have to retake the same quiz at some later point.

	Percentage of immigrants	Percentage of U.S. citizens
Unemployed	6%	7%
Incarcerated	2%	3%

Percentage of immigrants in the general population	13%
Percentage of illegal immigrants in the general population	3%
Percentage of immigrants who can't speak English	8%

Explicit Questions

To what extent do you agree with the following statements? [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]

There are currently too many immigrants in the U.S.

There are currently too many illegal immigrants in the U.S.

To what extent do you agree with the following statements? [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]

Immigrants are more likely to commit crimes than U.S. citizens.

Immigrants are more likely to be unemployed than U.S. citizens.

To what extent do you agree with the following statement? [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]

Immigrants generally learn English within a reasonable amount of time.

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.]

To what extent do you agree with the following statement? [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]
The government should devote a larger share of its budget to find illegal immigrants, and to deport them.
Congress should pass a bill to give some illegal immigrants living in the U.S. a path to legal status.

Which comes closer to your view about what government policy should be toward illegal immigrants currently residing in the United States? 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.
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.]

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]

To what extent do you agree with the following statement? [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]
Over the last 10 years, immigrants have produced more disadvantages than advantages for the U.S. as a whole.

Donation

Every tenth participant taking part in this survey will receive an extra $10. They will have to choose how much money they want to keep for themselves, and how much money they want to donate to the American Immigration Council.

Here is a short presentation of the American Immigration Council: "The American Immigration Council is a non-profit, non-partisan, organization [which] exists to promote the prosperity and cultural richness of our diverse nation by:

- Standing up for sensible and humane immigration policies that reflect American values.
- Insisting that our immigration laws be enacted and implemented in a way that honors fundamental constitutional and human rights.
- Working tirelessly to achieve justice and fairness for immigrants under the law."

To learn more about the American Immigration Council, please click on the following link: `http://immigrationpolicy.org/`

If you do receive an extra $10, how much money would you donate to the American Immigration Council? [slider]

Petition

You will now have the possibility of signing a petition regarding immigration policy.Consider the following petition, and decide whether you would like to sign it or not.

"Facilitate legal immigration into the US! Immigration is beneficial to the US economy, and it is therefore important to increase the number of green cards available for immigrants. Indeed, not only do immigrants strengthen the US economy, but they are also hard-working and law-abiding. Moreover, most of them adapt to our way of life, and they enrich our culture tremendously. This is why we believe that more green cards should be issued to immigrants, so that more of them can take part in the American Dream." [I want to sign this petition, I do not want to sign this petition.]

If the person wants to sign the petition

You stated that you want to sign this petition. To sign this petition, please click on the link below:

[`https://petitions.whitehouse.gov//petition/facilitate-legal-immigration-us-0` for the control group]

[`https://petitions.whitehouse.gov//petition/facilitate-legal-immigration-us-0` for the treatment group]

Did you sign the petition? [Yes, No]

Reminder: If you signed a petition, you will receive a confirmation email from the White House Petition website. To confirm your signature, please click on the link provided in that email.

Why did you choose to sign the petition? One or two sentences should be enough. [open text entry]

If the person doesn't want to sign the petition

Why did you choose not to sign the petition? One or two sentences should be enough. [open text entry]

Demographics

The main part of the survey is now over. We will now just ask you some general questions about yourself.

Which of these describes you more accurately? [Male, Female, Other]

What year were you born?

In which state do you currently reside?

What is your zipcode?

How many people are there in your household including yourself?

What was your annual household income (before taxes) in 2015? [Less than $10,000, Between $10,000 and $19,999, Between $20,000 and $29,999, Between $30,000 and $39,999, Between $40,000 and $49,999, Between $50,000 and $59,999, Between $60,000 and $69,999, Between $70,000 and $79,999, Between $80,000 and $99,999, More than $100,000]

What is the highest level of education you have completed? [12th grade or less; Graduated high school or equivalent; Some college, no degree; Associate degree; Bachelor's degree; Post-graduate degree]

What is your religion? [Christianity, Judaism, Islam, Hinduism, None, Other]

What is your ethnicity? [White, Black, Hispanic, Asian, Other]

What category would best describe your political orientation? [Extremely liberal; Liberal; Slightly liberal; Moderate, middle of the road; Slightly conservative; Conservative; Extremely conservative; Other]

What category would best describe your political orientation? [Republican, Democrat, Other]

Which of these describes your current situation most accurately? [Employed full-time, Employed part-time, Unemployed and looking for a job, Unemployed but not looking for a job, Retired, Other]

Were you born in the U.S.? [Yes, No]

Were both of your parents born in the U.S.? [Yes, No]

How much contact do you, personally, have with immigrants? [A great deal, A moderate amount, Not much, None at all]

How closely do you follow news about immigration in the United States?

[Very closely, Closely, Not closely, Not closely at all]
From which TV channel do you get most of your information? [NBC News, CBS News, ABC News, Fox News, Fusion, Newsmax TV, One America News, Other, Does not apply]
Pick the category that describes you best: [Mechanical Turk is my main source of income. I work on Mechanical Turk to supplement my income. I work on Mechanical Turk as a hobby. Other]
How many HITs have you already completed on Amazon Mechanical Turk? [dropdown menu]

Posterior Beliefs (only for the treatment group)

The experiment is now almost over! You will now have the chance to retake the quiz on immigration that you took before. Please note that you will only have 25 seconds to answer each question.

According to the American Community Survey, what percentage of the total U.S. population are immigrants? By immigrants, we refer to people who were not born in the U.S. [slider]

According to the Department of Homeland Security, what percentage of the total U.S. population are illegal immigrants? [slider]

According to the American Community Survey, about 7 percent of all American citizens are unemployed. What percentage of immigrants are unemployed? Please use the slider to select your answer. [slider]

According to the American Community Survey, 3 percent of all American citizens aged between 18 and 39 are incarcerated. Of all the immigrants living in the U.S. who are between 18 and 39 years old, what percentage are incarcerated? Please use the slider to select your answer. [slider]

According to the American Community Survey, what percentage of immigrants can't speak English? Please use the slider to select your answer. [slider]

Thank you very much for taking part in this short quiz! The experiment is now almost over!

Trust in experimenters (only for the treatment group)

During the experiment we provided you with some statistics about immigration. To what extent do you agree with the following statement? [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree] I was very confident that the statistics provided were accurate.

Debriefing

Thank you very much for completing our survey! Once we'll have gathered the data, we will calculate how much money each participant earned during the survey. In any case, you will at least get a payment of $1.30, which corresponds to the show-up fee. On top of that, you might receive a bonus, depending on your answers.

There will be a follow-up study in 3 weeks, where we will ask you to answer a set of questions on your policy preferences. The follow-up study will take only 4 minutes and you will earn $1, and on top of that, you may receive a bonus. If there are any remarks that you would like to make or clarifications that you would like to obtain, please do let us know by writing them into the field below: [open text entry]

Experimental Instructions: Follow-Up Experiment

Description

This study is conducted by researchers from Bocconi University and the University of Oxford. This research has received ethics clearance by the ethics committees of the University of Oxford and Bocconi University. No deception is involved and the study does not pose any risk to the participants. Participants will be asked to answer a few questions about their preferences, as well as a set of demographic questions. Participation in the study typically takes 4 minutes and is strictly anonymous.

In order to be paid, it is necessary to finish the survey. If you complete the survey, you will receive a fixed payment of $1. On top of that, you might receive a bonus. Each person is only allowed to participate in the experiment once. If you encounter a technical problem, please do not restart the experiment, but contact us at `research.dipmkt@unibocconi.it`. If participants have further questions about this study or their rights, or if they wish to lodge a complaint or concern, they may contact us at `research.dipmkt@unibocconi.`

`it.`

Consent Form

- I have read the information provided on the previous page.
- I have had the opportunity to ask questions about the study.
- I understand that I may withdraw from the study at any time.
- I understand that this project has been reviewed and approved by the ethics committees of the University of Oxford and of Bocconi University.
- I understand how to raise a concern or make a complaint.
- I understand that I can only participate in this experiment once.
- I understand that close attention to the survey is required for my responses to count.

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

Beliefs about immigrants

On the next pages, we will ask you a series of factual questions on immigration in the United States. Specifically, we will test your knowledge of various statistics on immigration. In case your response is within three percentage points of the true value, you will earn an extra 10 cents. Please note that you will only have 25 seconds to answer each question.

The statistics mentioned in the following questions mainly come from the American Community Survey, which is a nationally representative survey of the population living in the U.S., conducted by the U.S. Census Bureau.

According to the American Community Survey, what percentage of the total U.S. population are immigrants? By immigrants, we refer to people who were not born in the U.S. [slider]

How confident are you that your answer is correct? [very confident, confident, not very confident, not confident at all]

According to the Department of Homeland Security, what percentage of the total U.S. population are illegal immigrants? [slider]

How confident are you that your answer is correct? [very confident, confident, not very confident, not confident at all]

According to the American Community Survey, about 7 percent of all American citizens are unemployed. What percentage of immigrants are unemployed? Please use the slider to select your answer. [slider]

How confident are you that your answer is correct? [very confident, confident, not very confident, not confident at all]

According to the American Community Survey, 3 percent of all American citizens aged between 18 and 39 are incarcerated. Of all the immigrants living in the U.S. who are between 18 and 39 years old, what percentage are incarcerated? Please use the slider to select your answer. [slider]

How confident are you that your answer is correct? [very confident, confident, not very confident, not confident at all]

According to the American Community Survey, what percentage of immigrants can't speak English? Please use the slider to select your answer. [slider]

How confident are you that your answer is correct? [very confident, confident, not very confident, not confident at all]

Thank you very much for taking part in this short quiz!

Explicit Questions

We will now ask you a series of questions regarding your opinions on immigrants and immigration policies.

To what extent do you agree with the following statements? [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]
There are currently too many immigrants in the U.S.
There are currently too many illegal immigrants in the U.S.

To what extent do you agree with the following statements? [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]
Immigrants are more likely to commit crimes than U.S. citizens.
Immigrants are more likely to be unemployed than U.S. citizens.

To what extent do you agree with the following statement? [Strongly disagree,

disagree, neither agree nor disagree, agree, strongly agree]
Immigrants generally learn English within a reasonable amount of time.

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.]

To what extent do you agree with the following statement? [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]
The government should devote a larger share of its budget to find illegal immigrants, and to deport them.
Congress should pass a bill to give some illegal immigrants living in the U.S. a path to legal status.

Which comes closer to your view about what government policy should be toward illegal immigrants currently residing in the United States? 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.
allow illegal immigrants to remain in the United States and become U.S. cit-

izens, but only if they meet certain requirements over a period of time.]

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]

To what extent do you agree with the following statement? [Strongly disagree, disagree, neither agree nor disagree, agree, strongly agree]
Over the last 10 years, immigrants have produced more disadvantages than advantages for the U.S. as a whole.

Demographics

The main part of the survey is now over. We will now just ask you some general questions about your political orientation.

How much of the time do you think you can trust government in Washington to do what is right? [Just about always, Most of the time, Only some of the time, Hardly ever]

In which primary would you like to vote? If you have already voted, please indicate in which primary you have voted. [the Democrat primary, the Republican primary, neither]

In the primary for the Democratic party, who would you vote for now? [Bernie Sanders, Hillary Clinton] (if the participant wants to vote in the Democrat primary)

In the primary for the Republican party, who would you vote for now? [Ted Cruz, Donald Trump, John Kasich] (if the participant wants to vote in the Republican primary)

Debriefing

Thank you very much for completing our survey! Once we'll have gathered the data, we will calculate how much money each participant earned during the survey. In any case, you will at least get a payment of $1, which corresponds to the show-up fee. On top of that, you might receive a bonus, depending on your answers.

If there are any remarks that you would like to make or clarifications that you would like to obtain, please do let us know by writing them into the field below: [open text entry]

Additional tables

Table A1: 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**
MTurk					
Panel A					
Prior Belief	21.727	13.853	22.042	12.606	32.464
Panel B					
Posterior Belief	12.098	4.949	6.882	3.946	9.246
Panel C					
Belief Updating	9.629	8.904	15.159	8.660	23.219
	(0.760)	(0.694)	(1.036)	(0.690)	(1.079)
N	407	408	408	406	407
TNS Sample					
Panel D					
Prior Belief	33.619	24.470	23.592	17.211	36.032
Panel E					
Posterior Belief	14.958	8.823	9.734	7.181	11.311
Panel F					
Belief Updating	18.661	15.647	13.858	10.030	24.721
	(0.894)	(0.772)	(0.818)	(0.660)	(1.059)
N	598	598	598	598	598
True values	13	3	6	2	8

Note: The five outcome variables are people's beliefs about: the share of immigrants in the U.S., the share of illegal immigrants, the share of unemployed immigrants, the share of incarcerated immigrants, and the share of immigrants who cannot speak English. For the treatment group from the MTurk experiment, 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. For the treatment group from the experiment with TNS Global, participants' beliefs prior to the treatment are displayed in Panel D, while their beliefs after the treatment are displayed in Panel E. In Panel F, we show the difference between people's beliefs before and after the treatment. Standard errors are displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: 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)	21.039	13.042	21.185	11.842	31.911
Panel B					
Posterior (Baseline)	11.565	4.682	6.631	3.676	8.923
Panel C					
Follow-up	15.042	7.134	9.333	6.696	16.089
Panel D					
Difference A - C	5.997	5.908	11.851	5.146	15.821
	(0.787)	(0.832)	(1.144)	(0.769)	(1.315)
Panel E					
Difference B - C	-3.476	-2.452	-2.702	-3.021	-7.167
	(0.615)	(0.541)	(0.600)	(0.496)	(0.907)
N	336	336	336	336	336

Note: In these regressions, we only look at individuals from the treatment group who also completed the follow up. The five outcome variables are people's beliefs about: the share of immigrants in the US, the share of illegal immigrants, the share of unemployed immigrants, the share of incarcerated immigrants, and 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 A3: Policy Preferences: Heterogeneity by Republicans

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	There are not too many		Increase the number of		Decrease	Facilitate	Not Deport	Index 1	Index 2
	Legal Imm	Illegal Imm	Incoming Legal Imm	Green cards	Budget to deport	Legalization	Illegal Immigrants	Policy Preference	
MTurk Sample									
Treatment	0.190***	0.250***	0.076	0.038	0.014	-0.025	-0.044	0.069	0.012
	(0.063)	(0.059)	(0.068)	(0.067)	(0.068)	(0.068)	(0.066)	(0.043)	(0.047)
Treatment × Republican	0.237*	0.056	0.316**	0.268*	0.153	0.171	0.248*	0.207**	0.231**
	(0.128)	(0.121)	(0.139)	(0.138)	(0.139)	(0.139)	(0.135)	(0.088)	(0.097)
Republican	-0.354**	-0.481***	-0.345**	-0.361**	-0.547***	-0.716***	-0.904***	-0.535***	-0.574***
	(0.138)	(0.130)	(0.149)	(0.148)	(0.149)	(0.149)	(0.144)	(0.094)	(0.104)
P-value (Tr + Tr×Rep)	0.000	0.004	0.001	0.011	0.173	0.233	0.085	0.000	0.004
N	800	800	800	800	800	800	800	800	800
TNS Sample									
Treatment	0.075	0.223***	0.041	-0.022	0.010	-0.033	0.074	0.049	0.014
	(0.056)	(0.059)	(0.061)	(0.062)	(0.057)	(0.062)	(0.062)	(0.038)	(0.041)
Treatment × Republican	0.080	0.074	0.284***	0.236**	0.161	0.137	0.043	0.146**	0.172**
	(0.100)	(0.105)	(0.108)	(0.111)	(0.102)	(0.111)	(0.109)	(0.067)	(0.073)
Republican	-0.011	-0.195**	-0.259***	-0.291***	-0.278***	-0.271***	-0.221***	-0.208***	-0.264***
	(0.074)	(0.078)	(0.080)	(0.082)	(0.076)	(0.082)	(0.081)	(0.050)	(0.054)
P-value (Tr + Tr×Rep)	0.059	0.000	0.000	0.018	0.042	0.252	0.193	0.000	0.002
N	1193	1193	1193	1193	1193	1193	1193	1193	1193
Pooled Sample									
Treatment	0.127***	0.242***	0.038	-0.011	0.019	-0.035	0.028	0.055*	0.007
	(0.042)	(0.042)	(0.046)	(0.046)	(0.044)	(0.046)	(0.045)	(0.028)	(0.031)
Treatment × Republican	0.115	0.056	0.307***	0.258***	0.136*	0.144*	0.121	0.162***	0.193***
	(0.078)	(0.079)	(0.086)	(0.087)	(0.082)	(0.087)	(0.085)	(0.053)	(0.059)
Republican	-0.113*	-0.231***	-0.294***	-0.345***	-0.291***	-0.348***	-0.368***	-0.276***	-0.329***
	(0.059)	(0.060)	(0.065)	(0.065)	(0.062)	(0.066)	(0.064)	(0.040)	(0.044)
P-value (Tr + Tr×Rep)	0.000	0.000	0.000	0.001	0.027	0.139	0.038	0.000	0.000
N	1993	1993	1993	1993	1993	1993	1993	1993	1993

Note: 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. Index 1 uses variables from columns (1) to (7). Index 2 uses variables from columns (3) to (7). We include the same list of controls as in Table 3.3. Robust standard errors are displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Petition: Heterogeneity by Republicans

	Intention to sign	**Self-report: Sign**	**Index: Petition**
MTurk Sample			
Treatment	-0.022	-0.100	-0.061
	(0.072)	(0.061)	(0.062)
Treatment × Republican	0.336**	0.104	0.220*
	(0.147)	(0.126)	(0.127)
Republican	-0.778***	-0.582***	-0.680***
	(0.158)	(0.135)	(0.136)
P-value (Tr + Tr×Rep)	0.015	0.976	0.154
N	800	800	800
TNS Sample			
Treatment	-0.105*	-0.046	-0.076
	(0.064)	(0.066)	(0.061)
Treatment × Republican	0.270**	0.245**	0.258**
	(0.113)	(0.117)	(0.107)
Republican	-0.437***	-0.400***	-0.419***
	(0.084)	(0.087)	(0.080)
P-value (Tr + Tr×Rep)	0.075	0.039	0.039
N	1193	1193	1193
Pooled Sample			
Treatment	-0.074	-0.074	-0.074*
	(0.048)	(0.047)	(0.044)
Treatment × Republican	0.283***	0.208**	0.245***
	(0.090)	(0.087)	(0.082)
Republican	-0.420***	-0.365***	-0.392***
	(0.068)	(0.066)	(0.062)
P-value (Tr + Tr×Rep)	0.006	0.070	0.014
N	1993	1993	1993

Note: 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. We include the same list of controls as in Table 3.3. Robust standard errors are displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Additional Results: Opinion on Immigrants (TNS)

	No positive effect of Removing all illegal Immigrants	Immigrants produce	Legal Immigrants produce more advantages than disadvantages	Illegal Immigrants produce	Index Opinions	Donation
Treatment	0.090*	0.139***	0.065	0.164***	0.115***	0.070
	(0.049)	(0.048)	(0.049)	(0.047)	(0.040)	(0.056)
N	1193	1193	1193	1193	1193	1193

Note: 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. We include the same list of controls as in Table 3.3. 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 A6: Additional Results: Policy Preferences (TNS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	There are not too many		Increase the number of		Decrease	Facilitate	Not Deport	Access to Social	Imm: high Contrib
	Legal Imm	Illegal Imm	Incoming Legal Imm	Green cards	Budget to deport	Legalization	Illegal Immigrants	Services for Imm	to Public Goods
Panel A									
Treatment	0.102**	0.240***	0.127**	0.047	0.053	0.003	0.080	-0.016	0.183***
	(0.046)	(0.049)	(0.050)	(0.052)	(0.048)	(0.051)	(0.051)	(0.058)	(0.049)
N	1193	1193	1193	1193	1193	1193	1193	1193	1193

	(1) Main Index	(2) Index 2	(3) Index 3	(4) Index 4
Panel B				
Treatment	0.090***	0.062*	0.091***	0.068**
	(0.031)	(0.034)	(0.030)	(0.032)
N	1193	1193	1193	1193

Note: 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 et al., 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 3.3. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets. Index 1 uses variables from columns (1) to (7) from Panel A. Index 2 uses variables from columns (3) to (7). Index 3 uses variables from columns (1) to (9). Index 4 uses variables from columns (3) to (9). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Transatlantic Trends Survey: Worries about Immigration

	I am not worried about immigration					
	Legal	Illegal	Legal (US)	Illegal (US)	Within the EU	Outside the EU
Treatment	0.0025	0.0389	0.1721	0.0298	0.0709**	0.0241
	(0.0333)	(0.0328)	(0.1101)	(0.1247)	(0.0294)	(0.0295)
	[.894]	[.548]	[.419]	[.894]	[.107]	[.704]
N	7457	7437	930	923	9367	9360
R^2	0.000	0.000	0.008	0.000	0.001	0.000

Note: 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 et al., 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, 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 A8: Transatlantic Trends Survey: Perception of Immigrants

	Legal Immigrants		Illegal Immigrants		Immigrants	
	No burden for social serv	not increase crime	no burden for social serv	not increase crime	do not take jobs	create jobs
Treatment	0.0375	0.1051**	0.0736	0.1427***	0.0515	0.0323
	(0.0468)	(0.0461)	(0.0497)	(0.0493)	(0.0345)	(0.0345)
	[.264]	[.062]*	[.162]	[.025]**	[.162]	[.264]
N	3699	3669	3622	3569	7364	7145
R^2	0.000	0.003	0.001	0.005	0.001	0.000

Note: 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 et al., 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, 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 A9: Transatlantic Trends Survey: Policy Preferences

	Immigrants can stay permanently	Immigrants can be legalized	More Refugees	Immigrants can be legalized: US
Treatment	0.0259	-0.0314	-0.0236	-0.1173
	(0.0332)	(0.0331)	(0.0285)	(0.1358)
	[.774]	[.774]	[.774]	[.774]
N	7416	7420	11234	690
R^2	0.000	0.000	0.000	0.003

Note: 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 et al., 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, 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 A10: 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

Note: Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Determinants of the Biases in Beliefs: MTurk

	(1)	(2)	(3)	(4)	(5)	(6)
	Size of Biases in Beliefs about immigrants					
	Crime	No English	Unemployed	Share Immigrants	Share Illegal Immigrants	Index
Share of immigrants at respondent's zip code	-0.019	0.018	-0.083	0.123**	0.049	0.001
	(0.047)	(0.074)	(0.067)	(0.053)	(0.049)	(0.003)
State: share of Immigrants who do not speak English	-0.095	-0.248	-0.148	-0.159	0.198	-0.005
	(0.179)	(0.282)	(0.256)	(0.206)	(0.187)	(0.010)
State: share of Immigrants who are unemployed	2.219**	4.422***	4.181***	1.848*	0.873	0.166***
	(0.950)	(1.492)	(1.355)	(1.078)	(0.994)	(0.053)
Fox News	3.683**	2.773	1.934	5.346***	4.954***	0.274***
	(1.539)	(2.409)	(2.192)	(1.742)	(1.603)	(0.086)
Republican	1.155	3.496*	5.584***	2.068	2.319*	0.174**
	(1.330)	(2.088)	(1.897)	(1.507)	(1.391)	(0.075)
Christian	3.215***	3.853**	3.189**	4.662***	4.526***	0.265***
	(1.079)	(1.697)	(1.541)	(1.222)	(1.125)	(0.060)
At least bachelor	-1.845*	-1.256	-1.586	-3.822***	-3.654***	-0.171***
	(1.025)	(1.610)	(1.461)	(1.158)	(1.068)	(0.057)
Log income	-1.247*	0.201	-0.681	-1.663**	-2.669***	-0.100***
	(0.649)	(1.019)	(0.932)	(0.740)	(0.680)	(0.036)
Age	-0.669**	-0.308	-0.301	-0.527*	-0.468	-0.030*
	(0.277)	(0.436)	(0.397)	(0.314)	(0.289)	(0.016)
Age Squared	0.005	0.003	0.002	0.005	0.003	0.000
	(0.003)	(0.005)	(0.005)	(0.004)	(0.003)	(0.000)
Male	-0.372	-1.459	0.006	-2.711**	-2.860***	-0.104*
	(1.000)	(1.578)	(1.428)	(1.137)	(1.047)	(0.056)
Hispanic	-6.651**	-7.033	-1.937	-3.121	3.658	-0.216
	(3.027)	(4.780)	(4.387)	(3.475)	(3.167)	(0.170)
asian	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)
Black	-0.019	-1.412	0.591	-2.303	2.861	0.002
	(2.424)	(3.829)	(3.453)	(2.754)	(2.527)	(0.136)
White	-3.480*	-2.651	-3.184	-4.249**	-0.645	-0.207**
	(1.813)	(2.872)	(2.594)	(2.067)	(1.897)	(0.102)
Unemployed	-1.619	-2.333	-0.308	0.888	0.758	-0.032
	(1.603)	(2.537)	(2.291)	(1.831)	(1.678)	(0.090)
Born US	-4.441*	0.613	-0.378	-7.857***	-9.935***	-0.383***
	(2.425)	(3.823)	(3.489)	(2.788)	(2.537)	(0.136)
N	765	765	767	761	768	771
R^2	0.100	0.040	0.047	0.127	0.160	0.150

Note: Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A12: Main Effect: Controlling for Changes in Perception

	Donation	**Petition**	**Policy Preferences**	**Opinions**
Panel A: Main				
Treatment (A)	0.135***	-0.007	0.101***	0.120***
	(0.047)	(0.037)	(0.024)	(0.030)
N	1993	1993	1993	1993
Panel B: Controlling for perceptions				
Treatment (B)	0.052	-0.089**	-0.036	-0.023
	(0.048)	(0.038)	(0.023)	(0.029)
N	1993	1993	1993	1993
P-value(Tr(A) = Tr(B))	0.001	0.001	0.001	0.001

Note: Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A13: Heterogeneous Effect: Controlling for Changes in Perception

	Donation	**Petition**	**Policy Preferences**	**Opinions**
Panel A: Main				
Treatment	0.098*	-0.075*	0.057**	0.090**
	(0.055)	(0.044)	(0.029)	(0.035)
Treatment × Republican (A)	0.137	0.245***	0.162***	0.108
	(0.104)	(0.082)	(0.054)	(0.067)
Panel B: Controlling for perceptions				
Treatment	0.031	-0.140***	-0.057**	-0.029
	(0.056)	(0.044)	(0.026)	(0.034)
Treatment × Republican (B)	0.095	0.204**	0.091*	0.034
	(0.103)	(0.081)	(0.048)	(0.062)
N	1993	1993	1993	1993
P-value(Tr(A) = Tr(B))	0.001	0.001	0.001	0.001
P-value(Tr× Rep(A) = Tr× Rep(B))	0.001	0.001	0.001	0.001

Note: All of the outcomes are indices. 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. We include the same list of controls as in Table 3.3. Our measure of perception of immigrants is instrumented by whether our respondents were assigned to the treatment or the control group. 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 A14: Response of Outcomes w.r.t. Changes in Perceptions: IV

	Perception	**Donation**	**Petition**	**Policy Preferences**	**Opinions**
Panel A: IV Republicans					
Perception (A)		0.582***	0.403**	0.444***	0.317**
		(0.211)	(0.164)	(0.114)	(0.148)
N		568	568	568	568
Panel B: ITT Republicans					
Treatment	0.405***	0.236***	0.163**	0.180***	0.128**
	(0.055)	(0.084)	(0.064)	(0.053)	(0.065)
N	568	568	568	568	568
Panel C: IV Non-Republican					
Perception (B)		0.310	-0.299*	0.205*	0.328**
		(0.196)	(0.174)	(0.112)	(0.135)
N		1425	1425	1425	1425
Panel D: ITT Non-Republican					
Treatment	0.293***	0.091	-0.087*	0.060	0.096**
	(0.036)	(0.058)	(0.048)	(0.037)	(0.045)
N	1425	1425	1425	1425	1425

Note: All of the outcomes are indices. 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. We include the same list of controls as in Table 3.3. Our measure of perception of immigrants is instrumented by whether our respondents were assigned to the treatment or the control group. Robust standard errors are displayed in parentheses, while the p-values adjusted for a false discovery rate of five percent are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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.[53]

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]

[53]We recode the variables such that high values correspond to positive 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 of the control group and divide them by the standard deviation of the control group for 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).

- **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.]
  - 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 pol-

icy 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.