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Biased Beliefs about Immigration and Economic Concerns: Evidence from Representative Experiments

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Biased Beliefs about Immigration and Economic Concerns: Evidence from Representative Experiments

Abstract

We investigate the link between biased beliefs about immigrants, economic concerns and policy preferences. Conducting representative survey experiments with more than 8000 respondents, we first document substantial biases in respondents' beliefs about the immigrant population in various domains. Exposure to different types of signals about immigrants reduces concerns about adverse effects of immigration on the welfare state. On the contrary, different types of signals offset their effects on concerns about increasing labor market competition. Employing a data-driven approach to uncover systematic effect heterogeneity, we find that prior beliefs about immigration explain conditional average treatment effects. While attitudinal change is thus more pronounced among individuals with pre-intervention biases about immigrants, education and attitudes towards cultural diversity are additional drivers of heterogeneity. Treatment effects on welfare state concerns persist in a five to eight week follow-up.

JEL-Codes: C900, D830, F220, H200, J150.

Keywords: immigration attitudes, biased perceptions, belief updating, welfare state, labor market, causal forest.

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www.socialscisceregistry.org/trials/6819 and www.socialscisceregistry.org/trials/8166.

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1 Introduction

The integration of immigrants into host country labor markets is of key relevance for policy agendas across developed economies. Immigration is oftentimes regarded as a major contribution in protecting a country's long-term economic prosperity by alleviating shortages of skilled labor supply and population decline. A key determinant for immigration to foster economic prosperity over the medium and long run are attitudes of the host country's population. Western economies have, however, recently been subject to increasing economic protectionism, public opposition towards immigration and stronger support for right-wing anti-immigration policy agendas (Barone et al. 2016; Colantone and Stanig 2019; Halla et al. 2017).

From an economic perspective, individuals may oppose immigration due to concerns about adverse effects on the welfare state and public goods provision (Dahlberg et al. 2012; Facchini and Mayda 2009), or increased competition on the host country's labor market (Haaland and Roth 2020; Ortega and Polavieja 2012). From a voter's perspective, evaluating whether to support or oppose further immigration thus entails to form beliefs about the size and characteristics of the immigrant population to assess what to expect of immigration for the host country's economy.

The aim of this paper is to examine the link between different types of beliefs about the immigrant population on the one hand and economic concerns about immigration and policy preferences on the other hand. It has been documented that individuals in Western economies tend to exert biases when forming their beliefs about immigrants (Barrera et al. 2020; Citrin and Sides 2008). Ultimately, such biases may translate into an incorrect evaluation of the costs and benefits of immigration. Our paper thus directly addresses the relevance of different types of beliefs about immigrants for concerns about adverse effects on the host country's economy that may further translate into preferences for policy. For instance, if individuals tend to underestimate labor market participation of migrants, they may support anti-immigration policy agendas due to fears of detrimental economic effects. In this scenario, biased perceptions of immigra-

tion may ultimately aggravate the economic integration also of existing immigrants by motivating suboptimal immigration policy.

Using two large-scale representative survey experiments with a total of more than 8000 participants, we first elicit individuals' prior beliefs about immigrants, before exposing random subsets of respondents to different signals about the size and characteristics of the immigrant population. More specifically, we elicit respondents' beliefs about the share of immigrants, the unemployment rate of immigrants, and the share of European immigrants among all immigrants in the host country. We then evaluate whether the correction of biases by means of credible signals about immigrants affects respondents' economic concerns about immigration, and examine potential links between beliefs about immigrants and policy preferences.

We first observe that respondents exert considerable biases when stating their beliefs about immigrants. We then find that providing respondents with statistical facts about the immigrant population decreases their concerns about adverse effects of immigration on the welfare state. Our findings reveal that individuals in host societies link the (perceived) share and unemployment rate of immigrants to their evaluation of immigrants' aggregate fiscal contributions, and that belief updating can subsequently reduce concerns about adverse effects of immigration. We find that this effect persists in a five to eight week follow-up survey. In addition, we observe a similar (albeit not persistent) effect when respondents are provided with information about the aggregate cultural distance to immigrants instead of the unemployment rate of immigrants.

On the contrary, our results show that different types of signals about the immigrant population can offset their effects on concerns about increasing labor market competition. More precisely, a bundled signal containing both the share and unemployment rate of immigrants shows a precisely estimated null effect on labor market concerns. This finding highlights a trade-off in the evaluation of different signals about immigrants faced by individuals, which can ultimately cancel out effects of belief updating.

We again find similar results for the provision of information about immigrants' cultural distance.

Concerning effects on policy preferences, our findings reveal links between beliefs about immigration and preferences for further immigration. This finding suggests that belief updating translates into policy assessments, albeit to a less pronounced extent when compared to effects on the welfare state channel. Interestingly, we find suggestive evidence that information bundles about the immigrant population can reduce preferences for overall redistribution in the host society.

By means of a data-driven approach to uncover systematic heterogeneity in treatment effects (Athey et al. 2019; Nie and Wager 2021), we further find that prior beliefs about immigrants strongly moderate effects of information provision. Specifically, we find that our main effects are more pronounced for individuals who hold larger pre-intervention biases about the size and economic characteristics of the immigrant population. This also holds for pre-treatment attitudes towards cultural diversity. Similarly, respondents' educational background explains differences in effect sizes in the population, especially in relation to the labor market channel.

Furthermore, we observe that respondents receiving singular information on the share of immigrants simultaneously show lower biases for the unemployment rate of immigrants. Similarly, respondents who are exclusively exposed to information about immigrants' unemployment rate also exert lower biases when stating beliefs about the share of immigrants post-intervention. While these between-subject effect sizes are considerably smaller when compared to the main effects of information on posterior beliefs, they reveal the presence of cross-learning. This can be explained by respondents' receiving singular pieces of information realizing that their beliefs are biased, and thus subsequently updating a larger information set about the immigrant population in response to the observed signal.

In sum, the findings highlight that the distribution of prior beliefs about immigration in a society can be decisive for the effectiveness of information interventions

in the context of immigration attitudes. This also emphasizes implications for policy suggesting that interventions providing information about immigration statistics should be targeted based on the specific characteristics of the society of interest. Our findings further reveal that different combinations of signals about the immigrant population translate heterogeneously into attitudes in the host society. This observation may inform potential trade-offs for governmental information campaigns targeting misinformation about the immigrant population.

Our contribution to the literature is threefold. First, we jointly evaluate whether different types and quantities of signals about the immigrant population affect the formation of economic concerns and policy preferences in the host society. Specifically, we examine whether the size of the immigrant population (i.e. the share of immigrants), its economic characteristics (e.g. the unemployment rate of immigrants), and the perceived cultural distance to immigrants (e.g. the share of European immigrants) translate into immigration attitudes.

The literature has so far concentrated on either providing large bundles of information or singular treatments with mixed conclusions concerning treatment effectiveness. Grigorieff et al. (2020) find that providing individuals in the U.S. with a large bundle of several pieces of immigration-related information induces more positive attitudes towards legal immigrants, but do not find evidence for robust effects on preferences over immigration policy. Relatedly, Hopkins et al. (2019) investigate the effect of providing a single piece of information on the size of the U.S. immigrant population on natives' attitudes, but find little evidence in favor of sizeable intervention effects. Concentrating on the potential link between misperceptions about immigration and natives' preferences for redistribution, Alesina et al. (2022) present evidence for null effects in terms of information about the share and origins of immigrants. Our approach allows us to directly examine heterogeneity with respect to different types and quantities of signals about the immigrant population.

Second, we focus on economic concerns about immigration as a primary channel for attitudinal change. Based on the theoretical work by Facchini and Mayda (2009), we consider two channels of economic concerns in the host country: labor market concerns and welfare state concerns. While labor market concerns relate to individuals expecting increasing effects of immigration on labor market competition, welfare state concerns describe individuals fearing that immigration may increase the burden on the host country's fiscal system. Both of these channels have been investigated in a range of empirical studies (see e.g. Mayda 2006; Ortega and Polavieja 2012; Scheve and Slaughter 2001).

Previous studies addressing the relationship between misperceptions and economic attitudes towards immigration have focused on the provision of canonical research evidence rather than current statistical facts about immigrants, or have concentrated on the special case of refugee migration. Haaland and Roth (2020) examine whether evidence on the estimated labor market impact of immigration as highlighted by Card (1990) affects support for immigration in the U.S. In a related study, Lergetporer et al. (2021) focus on evidence about refugees' educational background in Germany. Our study extends this literature by directly assessing the causal link between different types of statistical beliefs about immigrants and economic concerns and policy preferences about general immigration in the host country.

Third, the factorial design of one of the two experiments further informs the literature on information provision experiments (Haaland et al. 2022) by allowing for a direct examination of cross-learning in posterior beliefs between different types of signals. While it has been shown that cross-learning can be present in the context of macroeconomic expectations (Coibion et al. 2022), its relevance has not yet been examined in the context of socioeconomic perceptions, such as beliefs about immigrants. A direct investigation of cross-learning in this context, however, is of high relevance to obtain a better understanding of the attitude formation process and the underlying up-

dating of respondents' information sets. Furthermore, insights on cross-learning help policy makers aiming to address socioeconomic misinformation in the population.

The remainder of this paper is structured as follows: section 2 presents the sample, design and hypotheses of our survey experiments. The distribution of beliefs about immigrants and updating of respondents' beliefs are discussed in section 3, while the empirical strategy and experimental results are presented in section 4. Section 5 concludes.

2 Data and Experimental Design

In this section, we describe the data set employed in our empirical analysis and present the experimental design of our survey experiments.

2.1 Sample collection

Our data set consist of two large-scale online surveys of the German adult population of voting age (i.e. 18 years and above). Both surveys are representative with respect to observable characteristics of age, gender, educational background, and residence in Eastern/Western Germany.¹

Figures 1 and 2 present graphical representations of the two survey waves and our experimental designs. The first survey wave with 2352 respondents is used for experiment I. In the second survey wave, which amounts to 5847 individuals, respondents are further randomly allocated to experiment I (3913 respondents), or to experiment II (1934 respondents). We pool data from the two surveys for experiment I.² In sum, we thus examine a total sample size of 8199 respondents across our two survey experi-

¹The sample composition with respect to our quota variables is presented in table A6. Our sample fulfills the representativity quotas with deviations of less than 1.5 percentage points.

²To account for potential time-based heterogeneity between the two survey samples, we include indicators for survey waves in all specifications.

ments.³ The two experiments differ in terms of the types of information that is provided to respondents, which is further discussed in the following section.

The first survey was fielded from end of November 2020 to mid of december 2020, while the second survey was conducted in September 2021. Both surveys were distributed to respondents by the survey company Respondi via an online panel. Before answering to our survey, respondents had to pass a standard attention screener (Chandler et al. 2019; Haaland et al. 2022). In the second survey wave, a subset of 50 percent of respondents was surveyed again five to eight weeks after our treatment interventions.⁴ The structure of the follow-up survey is very similar to the main survey. In contrast to the main survey, however, respondents are not exposed to any information intervention. This allows us to investigate whether treatment effects persist over a longer period of time.

Our data set contains measures related to the assessment of the general economic situation, beliefs about immigration, economic concerns about immigration, immigration policy preferences, preferences for redistribution, concerns about the COVID-19 crisis, general political and social attitudes, and a wide range of sociodemographic covariates.⁵ This allows us to evaluate experimental balance in socio-economic characteristics and personal attitudes and concerns, and further enables us to systematically assess potential heterogeneity in treatment effects for subgroups of the population.

³We restrict our analysis to respondents for whom we have full information on the variables of interest to ensure consistency of the analysis. The share of respondents dropped due to missing values amounts to only 2 percent across survey waves and experiments.

⁴Note that we survey a subset of 50 percent of the original sample of survey wave two due to logistical constraints and not due to attrition.

⁵An overview of the variables used in our analysis is presented in appendix B.

2.2 Stages of the experiments

We conduct two survey experiments to examine the relationship between biased beliefs about immigrants and attitudes towards immigration. In the following, we introduce their pre-registered experimental design.⁶ Our survey experiments were embedded into our representative population surveys.

Both experiments consist of four stages. While experiment I involves three treatment arms and a passive control group, in experiment II, respondents are allocated either to a single treatment group, or to a passive control group. The second survey wave further extends our designs by (i) eliciting posterior beliefs also for the passive control group to enable the examination of potential cross-learning between treatments, and (ii) the collection of a follow-up sample five to eight weeks after the treatment intervention.

In our experiments, we experimentally vary the quantity and the type of facts about immigration provided to respondents between treatment arms, incorporating information about the size of the immigrant population, its economic characteristics, and its non-economic (or cultural) characteristics. This allows us to investigate (i) the extent to which biases about the immigrant population are prevalent in the host society and (ii) whether the tailored provision of facts to counteract such biased perceptions has the potential to affect individuals' concerns about immigration and policy preferences.

2.2.1 Elicitation of prior beliefs about the immigration population

In the *first stage*, respondents are asked to state their prior beliefs about the statistical facts of interest to our study. As a benchmark for general beliefs about federal statistics, we first elicit their beliefs about the general unemployment rate in Germany. In experiment I, we then proceed to elicit respondents' prior beliefs about two key statistics about immigration to Germany: the share and the unemployment rate of immigrants.

⁶The pre-registration and our corresponding pre-analysis plans are available at: www.socialsciences-registry.org/trials/6819 and www.socialscienceregistry.org/trials/8166.

Respondents allocated to experiment II are instead asked to state their beliefs about the share of immigrants from a European country that came to Germany in 2019, in addition to their beliefs about the overall share of immigrants.⁷

The elicitation of prior beliefs enables us to differentiate between those respondents who exert biases in beliefs about immigration prior to treatment, and those respondents who are already well informed about the immigrant population at the time of our intervention.

2.2.2 Treatment arms: signals about immigrants

In the *second stage*, random subsets of respondents are provided with statistical information about the immigrant population. Specifically, we experimentally vary the amount and type of information provided across treatment arms.

Experiment I involves the following three treatment arms, representing a full-factorial design:

Share: Receives information on the share of immigrants (representing the size of the immigrant population).

Unemployment: Receives information on the unemployment rate of immigrants (representing the economic characteristics of the immigrant population).

Share + Unemployment: Receives information on the share *and* the unemployment rate of immigrants (representing a bundle of both types of information).

⁷The wording of belief elicitation is presented in appendix B (table B.2). In conjunction with the definition which is used by German Federal Office of Statistics, we define immigrants based on their citizenship. All survey respondents are provided with this definition. In experiment II, we follow the definition of European immigrants by the Federal Office for Migration and Refugees.

In experiment II, a random subset of respondents is instead subject to the following treatment:

Share + European Share: Receives information on the (overall) share of immigrants in the immigrant population (representing the size of the immigrant population) *and* the share of European immigrants (representing the aggregate cultural distance between natives and immigrants).

In both experiments, there are passive control groups which do not receive an information intervention and serve as counterfactuals. We employ two separate control groups for our two experiments to ensure that respondents have an identical survey flow (including belief elicitation) prior to treatment to avoid biases due to priming.

The types of statistical information distinguish between signals about the size of the immigrant population, its economic characteristics, represented by the unemployment rate of immigrants, and its non-economic characteristics as proxied by the share of European immigrants. In addition, the two bundled treatments allow us to investigate the dimension of the quantity of signals respondents are exposed to. In addition to the statistical signals, the information treatments further involve conditional feedback on respondents' prior beliefs for the three different treatment arms, based on the statistic(s) which are provided in each case.⁸

2.2.3 Measuring economic concerns about immigration and policy preferences

In the *third stage*, respondents are asked literature-based survey measures of attitudes towards immigration and preferences for redistribution. Specifically, we ask respondents survey questions related to the welfare state and labor market channels of immigration attitudes, immigration policy preferences, and preferences for redistribution.

With respect to economic attitudes towards immigration, the welfare state and labor market channels are emphasized by theory (Facchini and Mayda 2009) and have also

⁸The wording of our information interventions is presented in appendix C.

been extensively investigated by empirical research (Scheve and Slaughter 2001; Ortega and Polavieja 2012; Dahlberg et al. 2012; Hainmueller and Hiscox 2010; Naumann et al. 2018). We follow the notion put forward by Facchini and Mayda (2009), in which the welfare state channel relates to concerns about adverse effects of immigration on taxation and public good provision, while the labor market channel reflects concerns about increases in labor market competition.

The wording of the survey measures of economics concerns about immigration is based on the European Social Survey (ESS):

Welfare state concerns: “Immigrants pay taxes and receive social benefits from the health care and social insurance systems. On balance, do you think that immigrants in Germany receive more social benefits than they pay taxes, or that they pay more taxes than they receive social benefits?”. Answers range from 0 for “Receive more social benefits” to 10 for “Pay more taxes”.

Labor market concerns: “Do you think that immigrants rather take away jobs from workers in Germany, or that they rather help to create new jobs?”. Answers range from 0 for “Take jobs away” to 10 for “Create new jobs”.

In addition to economic concerns about immigration, we also investigate the effect of information provision on immigration policy preferences. Specifically, we employ the following wording which is often used in the related literature (Card et al. 2012; Grigorieff et al. 2020; Mayda 2006; Scheve and Slaughter 2001).⁹

Immigration policy preferences: “Do you think that the number of immigrants coming to Germany each year should be: decreased a lot / decreased slightly / stay the same / increased slightly / increased a lot?”.

⁹While it is sometimes differentiated between characteristics of the origin country, ethnicity, or legal status of immigrants, our employed survey measure refers to policy preferences about immigration in general.

We expand our analysis by also investigating potential effects of information provision on preferences for redistribution. In a recent study, Alesina et al. (2022) investigate whether information on the share or origin of immigrants affects preferences for redistribution, finding no evidence for information effects. We aim to extend their analyses by including statistical information about economic and non-economic characteristics of the immigrant population, represented by the unemployment rate of immigrants and the share of European immigrants, respectively, in addition to information on its size, i.e. the share of immigrants. For our measures of preferences for redistribution, we employ the following wording based on Alesina et al. (2022):

Preferences for redistribution: “Some people think that the government should not care about income differences between rich and poor people. Others think that the government should do everything in its power to reduce income inequality. What do you think?”. Answers range from 0 for “Government should not care about income inequality” to 10 for “Government should do everything against income inequality”.

We code all of our outcome variables such that a higher value indicates a more positive attitude towards immigration or a more supportive attitude towards redistribution, respectively. Labor market concerns, welfare state concerns, and preferences for redistribution are measured on an 11-point scale, and immigration policy preferences are measured on a 5-point scale, respectively.

2.2.4 Elicitation of posterior beliefs

In the *fourth stage*, we elicit respondents’ posterior beliefs about the immigrant population. This elicitation takes place at the very end of the survey in order to reduce concerns about experimenter demand. The elicitation of posterior beliefs allows us to investigate whether respondents in the treatment arms engage in belief updating after the receipt of facts about immigration. In addition, based on the factorial design in

experiment I, it enables us to examine cross-learning between different types of signals about the immigrant population.

2.3 Hypotheses

To derive our pre-registered hypotheses, we focus on the case in which individuals have “immigrant-averse” biases, i.e. in which they overestimate the share and unemployment rate of immigrants in experiment I, or in which they overestimate the share of immigrants, but underestimate the share of European immigrants in experiment II. This assumption allows us to make predictions about the direction of the effects of our signals ex ante and is supported by findings in the literature on beliefs about immigration (Alesina et al. 2022; Barrera et al. 2020; Grigorieff et al. 2020; Hopkins et al. 2019).

Our hypothesis for experiments I and II are largely analogous and are hence presented jointly in the following.¹⁰ We first present our hypotheses with respect to effects of information on economic concerns about immigration:

Hypothesis I – Welfare state channel: Information provision translates into a more positive assessment of immigrants’ welfare state contribution and hence lower welfare state concerns when respondents learn about a smaller size of the immigrant population and/or higher employedness of immigrants (or a larger proportion of European immigrants) than believed ex ante on average.

In this scenario, we expect respondents to develop a more positive evaluation of the aggregate fiscal contributions of immigration, resulting in lower concerns about adverse effects on the welfare state.

¹⁰Their original formulation for the two experiments is presented in the corresponding pre-analysis plans.

Hypothesis II – Labor market channel: Information provision translates into unchanged concerns of respondents about labor market competition when they learn about both a smaller size of the immigrant population and higher employedness of immigrants (or a larger proportion of European immigrants) than believed ex ante on average.¹¹

In this setting, the better integration of immigrants into the labor market is perceived as larger competition on the job market, as respondents learn that more immigrants are employed than previously expected, or,7 the share of European immigrants is larger than anticipated. Contrarily, the lower overall size of the immigrant population when compared to prior beliefs is perceived as less competition on the job market. Thus, as both types of information offer competing implications, they may potentially offset each other when provided jointly.

We now turn to the expected effects of information provision on policy preferences, considering preferences over immigration policy and redistribution:

Hypothesis III – Immigration policy preferences: Information provision translates into more positive immigration policy preferences of respondents when they learn about a smaller size of the immigrant population and/or higher employedness of immigrants (or a larger proportion of European immigrants) than believed ex ante on average.

The reasoning for this hypothesis is analogous to the welfare state channel, now leading to an increase in preferences for further immigration to the country.

Hypothesis IV – Preferences for redistribution: Information provision translates into more supportive preferences for redistribution of respondents

¹¹In our pre-analysis plan, we further included an alternative hypothesis for the labor market channel in which both types of information offer conforming implications. We abstract from further discussion of this alternative hypothesis as it is mutually exclusive to the hypothesis presented here.

when they learn about a smaller size of the immigrant population and/or higher employedness (or a larger proportion of European immigrants) of immigrants than believed *ex ante* on average.

With respect to preferences for redistribution, we expect that after information provision, respondents develop more supportive preferences for redistribution as they learn that immigrants put less strain on the fiscal system as initially predicted (due to a smaller size of the immigrant population and a (potentially) better integration into the labor market).

2.4 Balance in covariates

We evaluate experimental balance between groups on a large set of socio-economic and sociodemographic covariates as indicated in our pre-analysis plan. To obtain a scale-free assessment of balance across our large set of covariates, we calculate normalized differences between treatment and control groups as suggested by Imbens and Rubin (2015, p. 311). The results are presented in table A1 in the appendix. In sum, we observe normalized differences which are very close to zero, indicating well balanced experimental conditions. To ensure robustness across specifications, we nevertheless control for all covariates considered in the balance tests in all subsequent analyses.

3 Beliefs and Updating

In this section, we discuss the distribution and determinants of prior beliefs about immigrants, and the updating of beliefs by respondents after the receipt of signals.

3.1 Distribution of prior beliefs about immigrants

We begin our analyses by descriptively evaluating the extent to which respondents exert biases when stating their beliefs about the immigrant population. Figure 3 shows

the distributions of respondents' prior beliefs about the share and the unemployment rate of immigrants in Germany. While the true share of immigrants contained amounts to 13 percent, the true unemployment rate of immigrants in Germany is 15 percent.

When asked about the size and economic characteristics of the immigrant population, respondents' beliefs are largely dispersed across individuals. In particular, we observe that the share and unemployment rate of immigrants are jointly overestimated by about 50 percent of respondents. Beliefs are slightly more centered around the true value in case of the share of immigrants, while the distribution for beliefs about the unemployment rate of immigrants is highly skewed to the right.

We further investigate the distribution of prior beliefs about the share of European immigrants in experiment II. While the true share of European immigrants in Germany was 66 percent in 2019, almost all respondents underestimate this statistic. We observe strong deviations between the true value and priors with a median expected share of European immigrants of about 20 percent. This suggests that it is rather difficult for respondents to assess the cultural distance of the immigrant population to natives in the aggregate.¹²

3.2 Determinants of biased beliefs about immigrants

Against the background of the observed biases in respondents' beliefs, we advance by exploring which attitudinal and sociodemographic determinants explain such biases. For that purpose, we estimate the following equation:

$$b_i = \delta_0 + \delta^T X_i + \varepsilon_i, \quad (1)$$

¹²Note that the time frame of our experiment was before the onset of the Russian invasion in Ukraine and the subsequent increase in Ukrainian refugees to Germany.

where b_i represents biases in beliefs¹³ in absolute terms about the share of immigrants, their unemployment rate, and the share of European immigrants, respectively, X_i contains a pre-specified set of socio-demographic and attitudinal controls, and ε_i is the error term.

The results are displayed in figure 4 and table A2 in the appendix. Overall, we are able to explain about 41 percent of the variation in beliefs about the share of immigrants, and about 20 percent of the variation in beliefs about the unemployment rate of immigrants, respectively. In terms of correlations, we find beliefs about the general unemployment rate in Germany to be a strong predictor of biases in beliefs about immigrants. Interestingly, respondents stating to be relatively more confident when asked about their beliefs about the immigrant population show stronger biases, both about the size and the economic characteristics of immigrants. Similarly, pre-treatment concerns about immigration are associated with stronger biases about immigrants. On the contrary, respondents exerting more generalized trust report beliefs that are less biased, on average.

There is also a range of sociodemographic characteristics that are associated with biases in beliefs about immigrants. Strikingly, respondents living in the Eastern part of Germany report beliefs about the share immigrants that are more in line with the true value when compared to respondents from Western Germany. In contrast, these respondents, however, exert more strongly pronounced biases when asked about the unemployment rate of immigrants. In addition, respondents who have more contact to immigrants tend to show stronger biases with respect to the share of immigrants, while exerting less pronounced biases in terms of their unemployment rate.

Concerning beliefs about the share of European immigrants, we are able to explain about 15 percent of the variation in prior beliefs. We further observe that respondents' confidence when stating beliefs is not related to biases in beliefs about the share of European immigrants. Still, we find that older respondents, female respondents, and

¹³Note that biases in beliefs are defined as the difference between stated beliefs and true values.

respondents living in East Germany show larger biases, on average. On the contrary, individuals with higher education tend to have lower biases about the share of European immigrants.

For all three belief specifications, we further show the estimated intercept term. Since all covariates are scaled to have mean zero, we can thus interpret the intercept as the conditional mean of the outcome. Interestingly, the estimated intercept terms are very close to zero and statistically insignificant across the different types of beliefs. This suggests that our large set of covariates is able to explain a substantial amount of variation in biases in beliefs about immigrants, leaving an estimated bias close to zero for the average respondent.

3.3 Belief updating

We now turn to the evaluation of effects of signals about the immigrant population on respondents' beliefs. First, we evaluate whether individuals update their beliefs within-subject. Second, we examine effect sizes of our treatments on biases in posterior beliefs in a between-subject setting.

3.3.1 Within-subject updating of beliefs about immigrants

To investigate whether respondents who are exposed to signals about immigrants update their beliefs upon receipt of information, we conduct within-subject t-tests. Specifically, we compare differences in prior and posterior beliefs about immigrants within treatment arms which receive the respective type of information.

The within-subject comparison is presented in figure 5. As we observe above that the distribution of beliefs about immigrants tends to be skewed to the right, we calculate median values for prior and posterior beliefs to abstract from outliers. We find that respondents who receive information on the share of immigrants update their beliefs downward by about 5 percentage points, in the median. For the unemployment rate

of immigrants, this within-subject median difference amounts to a downward shift by about 15 percentage points. Concerning beliefs about the share of European immigrants in experiment II, we observe very strong updating by about 45 percentage points, in the median. Hence, we observe that respondents substantially develop posterior beliefs more in line with the true values across our three types of beliefs about the immigrant population.¹⁴

3.3.2 Between-subject effects of information on biased beliefs about immigrants

In addition to within-subject analyses, we also evaluate between-subject effects of our information treatments on respondents' biases in posterior beliefs about immigrants. This allows us to directly examine potential cross-learning between different types of information about the immigrant population and enables measurement of belief updating in terms of effect sizes.¹⁵ For that purpose, we estimate the following equation for experiment I (and an analogous equation for experiment II):

$$p_i = \rho_0 + \rho_1 A_i + \rho_2 B_i + \rho_3 C_i + \delta^T X_i + \varepsilon_i, \quad (2)$$

where p_i represents biases in posterior beliefs in absolute terms about the immigrant population, A_i , B_i and C_i are treatment indicators for the different treatment arms, X_i contains the covariates employed in the balance tests, and ε_i is the error term.

The estimation results are presented in figure 6 and table A3 in the appendix. Since we define biases in posterior beliefs in absolute terms, a negative effect size represents updating of respondents towards the true value. We observe large negative and statistically significant effect sizes of our information treatments on biases in respondents' posterior beliefs, as should be expected given the evidence on within-subject updating. Updating of the European share is particularly pronounced. Interestingly, when com-

¹⁴All within-subject differences are statistically significant on the 1 percent level.

¹⁵Note that in the first survey wave, we did not elicit posterior beliefs for untreated respondents. Hence, the analysis in this subsection investigates respondents from the second survey wave only.

paring effect sizes between our bundled treatments in experiments I and II, we find smaller, albeit still statistically significant between-subject updating in the latter case for the share of immigrants.

In addition we further find that respondents who receive information only on the share of immigrants still report lower biases for the unemployment rate of immigrants. Analogously, respondents who receive information only on immigrants' unemployment rate still exert lower biases when stating posterior beliefs about the share of immigrants. While effect sizes in these cases are considerably smaller when compared to direct information effects on beliefs, this evidence suggests that there exists some degree of cross-learning between our types of information of interest. This can potentially be explained by respondents' receiving singular pieces of information realizing that their beliefs are biased, and thus subsequently updating a larger information set about the immigrant population in response to the observed signal.

4 Treatment Effects

We observe that respondents' update their beliefs to a considerable extent after the receipt of information. But does belief updating translate into changes in economic concerns about immigration and policy preferences? We will examine this in the following, concentrating first on average effects of information provision, followed by a discussion of heterogeneity with respect to treatment effects and treatment effect persistence.

4.1 Average treatment effects

We first evaluate average treatment effects (ATE), estimating the following equation for experiment I (and analogously for experiment II) which compares our outcome

variables across treatment arms given exogeneity of the treatments:

$$y_i = \gamma_0 + \gamma_1 A_i + \gamma_2 B_i + \gamma_3 C_i + \delta^T X_i + \varepsilon_i, \quad (3)$$

where y_i represents the outcome variable, A_i , B_i , and C_i are treatment indicators for the different treatment arms, X_i contains the covariates employed in the balance tests, and ε_i is the error term.

4.1.1 Economic concerns about immigration

Focussing first on effects of information on economic concerns about immigration, the estimation results are displayed in the first and second panels of figure 7 and panels A of tables A4 and A5 in the appendix. Recalling that our outcome measures are coded such that a higher value indicates a more positive attitude towards immigration (or lower concerns), providing information about the share of immigrants exerts positive and statistically significant effects on both welfare state and labor market concerns about immigration. The effect size for labor market concerns is, however, considerably smaller when compared to the effect on welfare state concerns. We observe a similar pattern for information on the unemployment rate of immigrants.

Finally, for the bundles of information consisting of two signals about the immigrant population, we observe positive and statistically significant effects on welfare state concerns, while effect sizes are very close to zero and statistically insignificant for labor market concerns. For welfare state concerns, we observe similar magnitudes of positive effects amounting to about 11 to 14 percent of a standard deviation across treatments in the two experiments. This finding is in line with hypothesis *I*, suggesting that information about the immigrant population translates into lower concerns about adverse effects of immigration on the welfare state.

In contrast, the observed pattern for labor market concerns suggests a trade-off scenario for the relevance of signals about immigrants: Effect sizes differ considerably

between the single and bundled information treatments. Strikingly, for the bundles of information consisting of both the share and unemployment rate of immigrants (or the overall share and share of European immigrants), the estimates are very close to zero with small standard errors, suggesting precise null effects of the bundled treatment on labor market concerns. These findings are thus in line with hypothesis *II*.

4.1.2 Policy preferences

We now turn to the effects of our information treatments on immigration policy preferences and preferences for redistribution. The results are presented in the third and fourth panels of figure 7 and in panels *A* of tables A4 and A5 in the appendix. We find that the singular information treatments providing either the share or the unemployment rate of immigrants positively affect respondents' preferences for further immigration to their country. The effect sizes range between about 5 and 8 percent of a standard deviation and are more pronounced for the treatment containing only the share of immigrants.

For the bundled treatments in experiments I and II, we observe slightly positive, albeit statistically insignificant effect sizes. Qualitatively, we hence observe a similar trade-off pattern of treatment effect sizes for immigration policy preferences as for labor market concerns. Hence, we find support for hypothesis III only for the subset of treatments containing only the share or the unemployment rate of immigrants, while the bundled treatments do not show sizeable impacts on preferences for further immigration.

Concerning preferences for redistribution, we estimate precise null effects of our information interventions for all treatments in experiment I. Specifically, the coefficients of the three treatment arms are very close to zero with confidence intervals robustly bounded between ± 10 percent of a standard deviation. Hence, this evidence suggests that, on average, respondents do not perceive a link between their beliefs about

immigrants, our information interventions, and their preferences for governmental redistribution in their country.

Interestingly, in experiment II, we find evidence for a statistically significant negative effect of the bundled treatment consisting of the overall share and the share of European immigrants on redistributive preferences. This finding is at odds with hypothesis IV, predicting a positive impact of information about immigrants on respondents' preferences for redistribution. We continue the interpretation of this finding based on our heterogeneity results in the next section.

4.2 Treatment effect heterogeneity

We find evidence for average effects of our information treatments on economic concerns about immigration and preferences for immigration policy. Besides average treatment effects, effects of our information interventions may, however, differ considerably across different subpopulations in our sample. This is of special relevance as our hypotheses have been formulated for immigration-averse beliefs of respondents.

In the following, we hence first discuss treatment effects for those individuals who initially exert such biases in their beliefs about immigrants. As, in addition, we measure a large set of further covariates that are potentially relevant for effect heterogeneity as well, we then proceed with a systematic data-driven examination of heterogeneity to arrive at reliable conclusions regarding the most relevant sociodemographic and attitudinal factors for treatment effectiveness.

4.2.1 Heterogeneity based on prior beliefs about immigrants

Recalling that our hypotheses are derived based on the assumption that individuals jointly overestimate both the share and the unemployment rate of immigrants (or overestimate the share but underestimate the European share in experiment II), we reestimate equation (3) for respondents who exert such immigration-averse biases in

prior beliefs before our interventions. The results are displayed in figure 8 and panels *B* of tables A4 and A5 in the appendix.

Overall, we observe a similar pattern of effects as for the full sample. In line with the derivation of our hypotheses, effect sizes are considerably larger across most outcomes and treatments when compared to the full-sample results: We now find positive effects on welfare state concerns ranging between about 19 to 21 percent of standard deviation, while for labor market concerns, effect sizes are also more pronounced for the singular treatments. Importantly, however, the bundled treatments show the same pattern of null effects also when accounting for biases in prior beliefs.

It may also be the case that treatment impacts on policy preferences are conditional on prior beliefs. Similar to the results for welfare state concerns about immigration, we observe stronger positive effects for immigration policy preferences when compared to the full sample, now ranging between 10 and 17 percent of a standard deviation in experiment I. For the bundled treatment in experiment I, this positive effect is now also robustly statistically significant. These results suggest that respondents who exert upward biases in their beliefs about the immigrant population develop more supportive preferences for immigration policy following the information intervention. While this is in line with hypothesis *II* for experiment I, we do not find evidence for a similar effect for the bundled treatment in experiment II.

On the contrary, with respect to redistributive preferences, we now observe a statistically significant decreases in preferences for redistribution for both bundled treatments across the two experiments. As for the full sample, this finding is in contrast to hypothesis *IV* which predicts positive effects of information provision on redistributive preferences. A potential explanation for this adverse effect may be related to the positive impact of our treatments on welfare state concerns about immigration, conditional on positive biases in prior beliefs: As respondents learn that the share (their labor market participation) of immigrants is smaller (larger) than expected, they update their beliefs to evaluate the aggregate fiscal contribution of immigrants more positively, while thus

seeing less need for an increase in redistribution based on the current labor market integration of immigrants.

This interpretation, however, rests on the assumption that respondents expect immigrants to have a higher propensity to be recipients of redistribution compared to native individuals, on average. In addition, as results are similar between experiments I and II, the findings also highlight that respondents may interpret information about the share of European immigrants as a proxy for immigrants' labor market participation when evaluating welfare state effects.

4.2.2 Heterogeneity based on causal forest estimation

To systematically assess treatment effect heterogeneity also with respect to other potentially relevant sociodemographic and attitudinal domains within our set of covariates, we employ causal forest estimation, a recently developed data-driven approach which makes use of machine learning algorithms to uncover systematic effect heterogeneity across the sample (Athey et al. 2019; Nie and Wager 2021).

The advantage of this approach in comparison to the evaluation of many subgroups in terms of treatment effects is twofold: First, it enables us to obtain a distribution of estimated effect sizes for each respondent and treatment condition, instead of a simple point estimate as obtained by the ATE. This distribution can then be used to directly evaluate the relevance of each covariate for effect heterogeneity. Second, since the prediction of treatment effect heterogeneity itself is based on out-of-bag prediction using only those specifications for prediction of effect size that have not been trained in the respective observation, estimated effect sizes are not subject to overfitting and multiple hypothesis testing.

In this setting, treatment effect heterogeneity is estimated by means of conditional average treatment effects (CATE). More, specifically, the approach allows us to obtain

a non-linear estimate of CATE for each individual and treatment arm, defined as:

$$\tau(x) = E(y_i(1) - y_i(0) | X_i = x), \quad (4)$$

where $\tau(x)$ is the CATE, $y_i(1, 0)$ denotes potential outcomes of our variable of interest if individual i is allocated to a treatment arm or control group, and X_i is a matrix containing the covariates employed in our balance tests, equal to the realization x for individual i .

As potential outcomes for individual i cannot be observed simultaneously and thus cannot directly serve as a target variable for prediction, a transformed outcome is employed instead, which can be calculated as (Athey and Imbens 2016):

$$y_i^* = \frac{y_i(w_i - p)}{p(1 - p)}, \quad (5)$$

where y_i^* is the transformed outcome, w_i is the treatment status (equal to 1 for the treatment arm), and p denotes the treatment propensity of individual i .

Assuming unconfoundedness given exogeneity of treatment, we can then employ an ensemble of regression trees, weighted in a causal forest to predict the transformed outcome, which, in expectation, is an estimate of CATE:

$$E(y_i^* | X_i = x) = \tau(x). \quad (6)$$

To arrive at an estimated CATE distribution for each treatment arm, we rely on a generalization for multi-arm causal forests as developed by Nie and Wager (2021). The resulting estimated distributions of CATE, evaluated separately for both experiments, are presented in figure 9. The median values are very close to the estimated ATE in the previous section, as should be expected. In addition to these point estimates, we are now able to evaluate a distribution of estimated effect sizes for each individual i in

our sample. This allows us to decompose our estimated distributions of CATE into the most relevant covariates in X .

For that purpose, we evaluate the variance of the differences in means between each quartile and the full sample for a given covariate (Athey and Wager 2019). We then scale this variance by the full-sample variance of the covariate. More precisely, we calculate:

$$R_{x_k} = \frac{Var(\bar{x}_{k,q} - \bar{x}_k)}{Var(x_k)}, \quad (7)$$

where R_{x_k} denotes the scaled relevance for a specific covariate x_k in X , $\bar{x}_{k,q}$ is the sample mean for covariate x_k within a specific quantile q of the CATE distribution, and \bar{x}_k is the full-sample mean for that covariate. As this measure is scaled by the full-sample variance of each covariate, we arrive at a scale-free measure of relevance for treatment effect heterogeneity which is directly comparable across covariates.

The results are displayed in figure 10.¹⁶ This descriptive comparison of covariate relevance shows that prior beliefs about immigrants in terms of the share and unemployment rate are the two most relevant variables for treatment effect heterogeneity across the sample. In addition, beliefs about the general unemployment rate in the population (elicited as a baseline measure at the very beginning of our surveys) are also related to heterogeneity.

This reveals that beliefs about the immigrant population are the main drivers of effect heterogeneity in our setting. Their relative relevance is, however, dispersed across treatment arms and outcomes. While beliefs about the share of immigrants are more relevant for the share treatment, beliefs about the unemployment rate of immigrants show more relevance for the unemployment treatment. Interestingly, for the bundled treatment, relevance shifts from beliefs about the unemployment rate of immigrants for welfare state concerns to beliefs about the share of immigrants for labor market concerns and policy preferences. This is in line with the observation that the

¹⁶Note that we restrict the discussion on the seven most relevant variables as evaluated over the whole set of CATE distributions for each treatment arm and outcome.

share treatment shows the most robust effect sizes for the latter outcomes in terms of ATE.

Besides beliefs about immigrants and the general unemployment rate, we also observe that pre-treatment concerns about immigration and the economic situation, as well as attitudes towards cultural diversity are relevant drivers of CATE. In addition, we find respondents' educational background to be of relevance as well, which is most pronounced for the labor market channel. This finding is in line with the theoretical prediction that natives' skill levels should moderate their concerns about increases in labor market competition in case of inflows of migrants of a specific skill group (Facchini and Mayda 2009) and thereby relates to recent evidence about information provision and attitudes towards refugees (Lergetporer et al. 2021).

Concerning effects on preferences for redistribution, we abstract from further discussion of heterogeneity for this outcome as we did not observe significant average effects in our main specification. Hence, in this case the estimated distributions of CATE are expected to be particularly noisy, as is supported by the relatively low relevance measures found for our singular treatments across covariates.

In experiment II, we observe that prior beliefs about the share of immigrants and the general unemployment rate are among the most relevant covariates for CATE. Interestingly, we do not find evidence that beliefs about the share of European immigrants are of high relevance for treatment effectiveness. Instead, we observe that for the treatment containing both the overall share and the share of European immigrants, self-assessed confidence when stating prior beliefs moderate treatment effects. In line with the results for experiment I, we again observe that baseline concerns about immigration and attitudes towards cultural diversity are relevant drivers of CATE.

We have seen that prior beliefs about the share and unemployment rate of immigrants are the most relevant drivers of CATE in experiment I. This is in line with our results in the previous subsection, showing larger effect sizes for those individuals with immigration-averse biases in beliefs prior to treatment. Still, it remains unclear

whether this relationship between CATE and prior beliefs increases monotonically or whether effect sizes are largest only for a specific range of the belief distribution.

To examine the shape of the relationship between CATE and prior beliefs, figure 11 exemplarily plots estimated CATE on welfare state concerns against prior beliefs about the share and the unemployment rate of immigrants.¹⁷ We apply locally estimated scatterplot smoothing (LOESS) to visualize the shape of the relationship between effect heterogeneity and prior beliefs. In addition, we show LOESS estimates of the point-wise confidence intervals of the estimated CATE for each respondent.¹⁸ We observe that across treatments and types of beliefs, the estimated relationships between CATE and beliefs of respondents follow an inverted U-shape: Effect sizes are noisy and small below the true value cutoffs of the share and unemployment rate of immigrants and increase as the deviation between respondents' beliefs and the true values increases, but only up to a point where effect sizes start to decrease again or remain stagnant.

These findings suggest that our information treatments are most effective to reduce welfare state concerns about immigration when respondents (i) show immigration-averse biases in beliefs prior to treatment, which (ii) do not deviate to a very large extent from the true values. The finding also highlights that it may be difficult to address respondents via information provision who exert very pronounced biases about immigrants *ex ante*. Still, as most respondents in our sample show rather moderate biases in beliefs about immigrants, we find that information provision induces shifts in welfare state concerns for a substantial fraction of the population.

¹⁷For this additional analysis, we focus on effect heterogeneity with respect to the welfare state channel as it shows the largest and most robust effect sizes across treatments in experiment I.

¹⁸Note that as these LOESS estimates represent a non-linear aggregation of the point-wise confidence intervals of CATE that are estimated for each respondent, they are an aggregate measure of significance but do not imply the same upper and lower bounds of CATE for each observation.

4.3 Persistence of treatment effects

The results for the main survey waves reveal sizeable impacts of our information treatments on economic concerns about immigration and immigration policy preferences in the short run. But do these effects prevail over a medium time horizon? To address this question, we now turn to the results of the follow-up sample in survey wave two, examining the persistence of treatment effects.

4.3.1 Composition of the follow-up sample

The follow-up survey took place five to eight weeks after the first survey wave, depending on the timing of survey participation for each respondent. In comparison to the literature, this is a rather long time gap between survey waves that allows us to examine whether treatment effects persist over a medium time horizon.¹⁹ Due to logistical constraints, we surveyed a subset of 50 percent of respondents from survey wave two, participating either in experiment I or II. We evaluate the degree of selection into the follow-up survey by comparing follow-up and non-follow-up respondents in terms of observable characteristics.

As a first step, we check whether the follow-up sample is representative with respect to our quota characteristics. The results are presented in columns 4 and 5 in table A6. Overall, we see that deviations from the quota targets in the follow-up sample are less than 5 percentage points across quota characteristics in absolute terms. The most notable differences concern age and gender composition. Considering that quotas were not reinforced ex ante for the follow-up survey, this gives a first indication that

¹⁹The typical time gap between main and follow-up survey waves in the literature ranges between one week (Haaland and Roth 2020; Lergetporer et al. 2021) and four weeks (Grigorieff et al. 2020), with some studies abstracting from follow-up surveys entirely (Barrera et al. 2020; Hopkins et al. 2019).

the degree of selection into the follow-up is relatively low, albeit more pronounced than in the main survey.²⁰

Still, it may be that follow-up respondents differ from non-follow-up respondents in domains other than our quota targets. Hence, we evaluate normalized differences between the two groups of respondents in table A7 in the appendix. While normalized differences between follow-up and non-follow-up respondents are close to zero for most covariates, there are a few domains in which the two groups of respondents differ. More specifically, respondents in the follow-up sample show more accurate prior beliefs about the general unemployment rate and are more confident when stating their beliefs about the share of European immigrants. They are also less concerned about the economic situation in Germany as well as the COVID-19 crisis in comparison to non-follow-up respondents. In addition, follow-up respondents are more likely to be living in a partnership, and, again, are more likely to be older and male, on average.

These findings suggest that there exists some degree of selection into the follow-up sample, e.g. in relation to the age and gender composition or concerns prior to treatment. Importantly, however, considering all covariates, the degree of selection can be interpreted as comparably low. We are careful, however, to see our estimations in the follow-up sample as providing first tentative results on treatment effect persistence.

4.3.2 Treatment effects over time

To examine whether treatment effects persist over time, we reestimate equation (3) for respondents in the follow-up sample, using economic concerns and policy preferences elicited in the follow-up as the dependent variables. The results are presented in figure 12 and panels A of tables A9 and A10. In sum, we do not find robust evidence that effects persist for the full follow-up sample over a time horizon of five to eight weeks.

²⁰We also reevaluate experimental balance for the follow-up sample. The results are presented in table A8 and suggest good balance. To ensure robustness, we nevertheless control for all covariates from the balance tests also in our follow-up analyses.

More specifically, estimates of treatment effects on follow-up outcomes are mostly statistically insignificant and closer to zero when compared to the results in the main survey.

Recalling the derivation of our hypotheses, these average results may, however, be obfuscated by differences in prior beliefs before the initial intervention in the first survey wave. Hence, we estimate follow-up effects again, now focussing specifically on those respondents with biases in beliefs in line with our hypotheses prior to treatment in the main survey. The results are displayed in figure 13 and panels *B* of tables A9 and A10.

We find that for welfare state concerns, treatment effects persist over a medium time horizon for those respondents with biases in beliefs prior to intervention. Specifically, for the treatment arms containing information on the unemployment rate or the bundled treatment consisting of information on both the share and the unemployment rate of immigrants, we observe positive and statistically significant effect sizes. In contrast, for our other outcome domains of interest, we do not observe evidence for a persistence of treatment effects. Against the background of some selection and reduced statistical power in the follow-up sample, we hence interpret these results as suggestive evidence in favor of a persistence of effects of information for the welfare state channel of attitudes towards immigration.

5 Discussion and Conclusion

We conduct two large-scale survey experiments to examine the relevance of statistical information for economic attitudes towards immigration. We find that providing respondents with signals about the immigrant population decreases their concerns about adverse effects of immigration on the welfare state. This effect is more pronounced for individuals with biases about the size and economic characteristics of the immigrant population prior to intervention. On the contrary, we observe that different types of

signals about immigrants can offset information effects on concerns about increasing labor market competition. We find similar effects in an alternative experimental design in which respondents are exposed to an indicator for cultural distance rather than economic integration of immigrants. The analyses further suggest links between beliefs about immigration and preferences for further immigration in the host population.

Our findings highlight that the distribution of beliefs about immigration in a society prior to potential interventions explains part of treatment heterogeneity and is thus a decisive factor in determining the effectiveness of information campaigns in the context of immigration attitudes. In particular, this suggests that policy interventions providing information about immigration statistics should be targeted based on the specific characteristics of the society of interest. In sum, our results reveal that different combinations of signals about the immigrant population can translate heterogeneously into attitudes in the host society. While we provide evidence for positive effects of signals about immigrants on economic attitudes towards immigration – and address the question of persistence, our findings also inform about potential trade-offs for governmental campaigns targeting misinformation about the immigrant population.

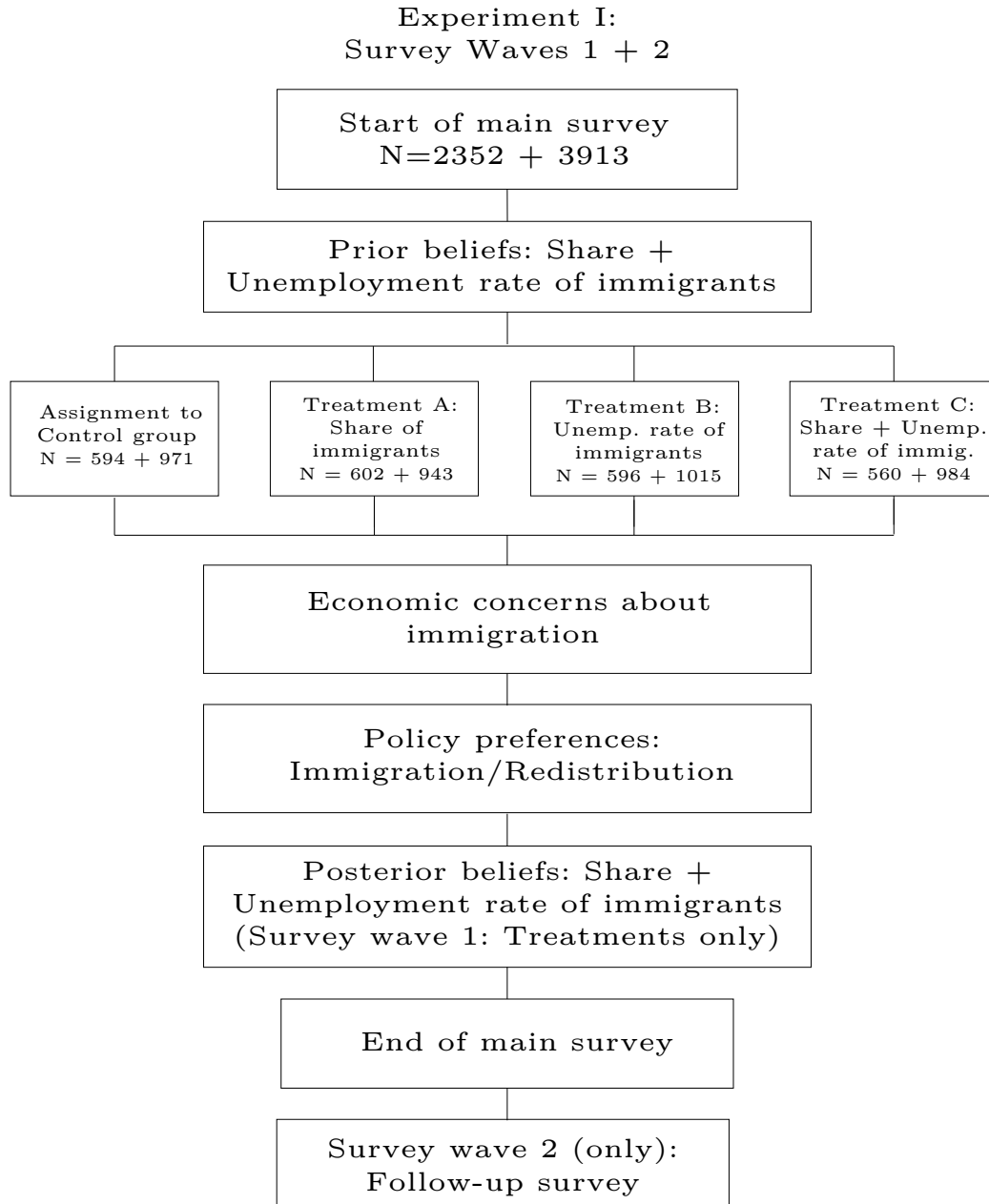
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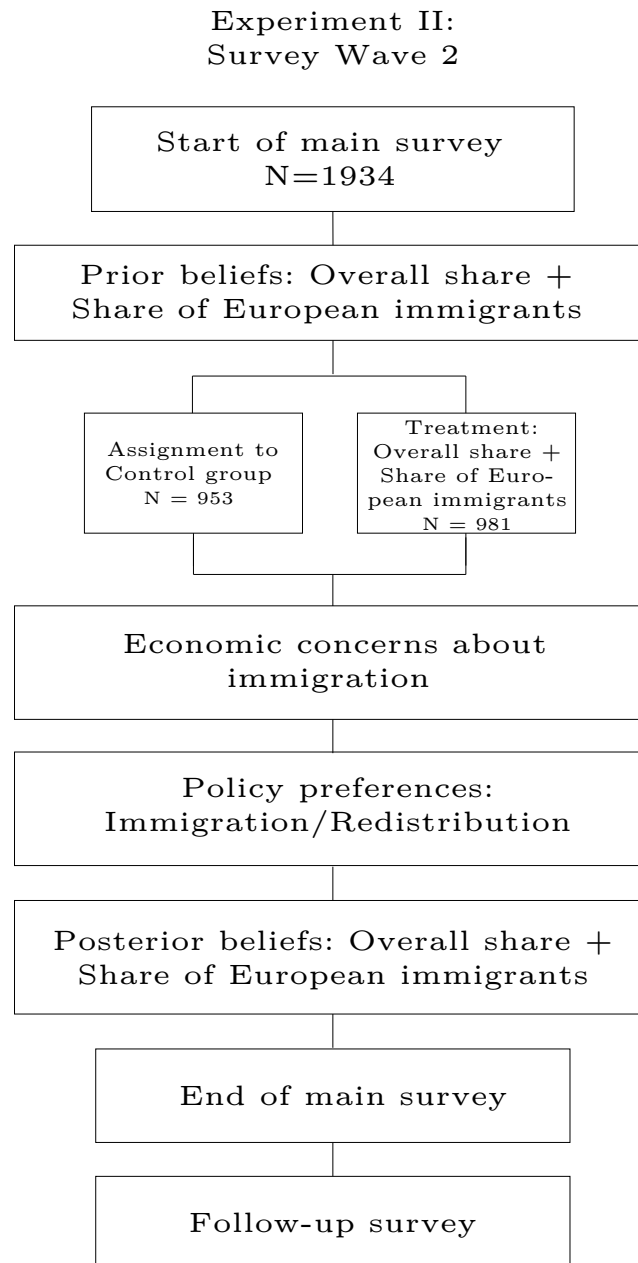
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Figure 1: Experimental design: experiment I.



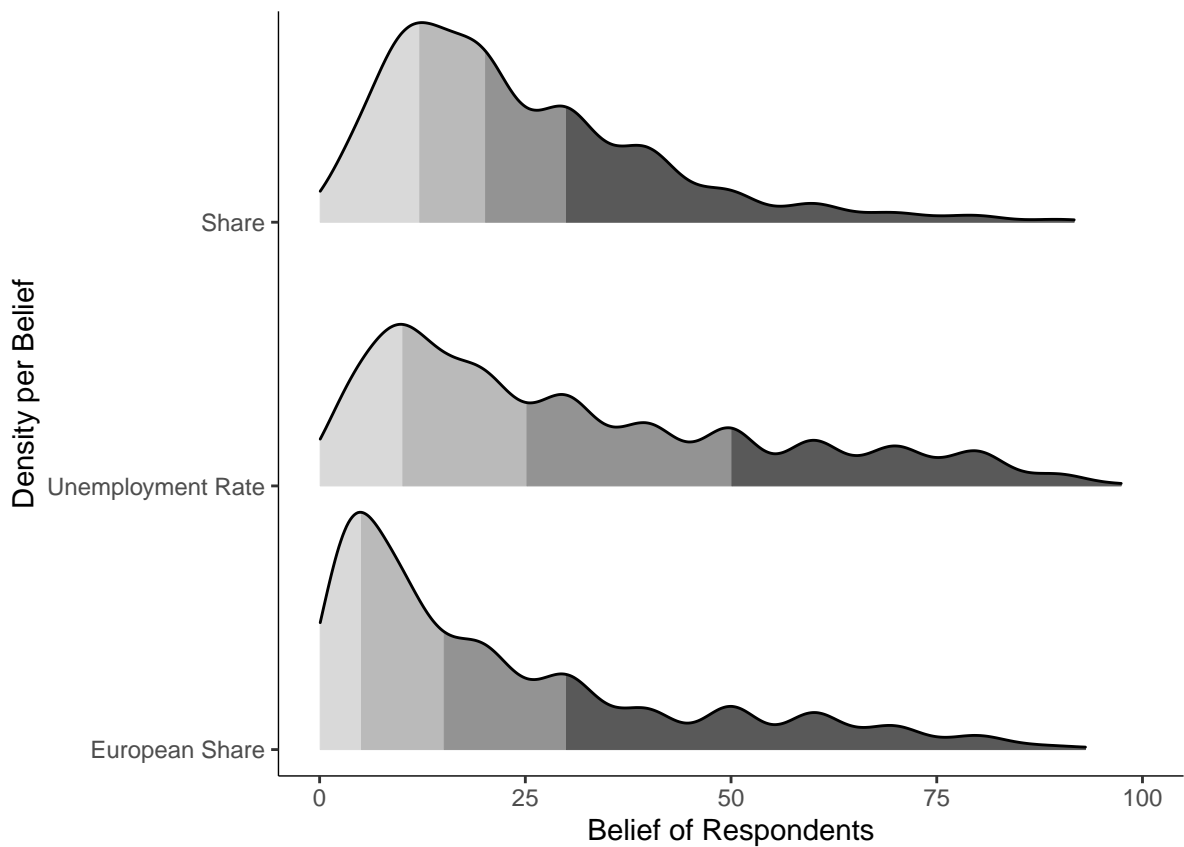
Notes: The figure shows a graphical representation for the experimental design of experiment I. Note that experiment I has been included (with slight adjustments) in both survey waves 1 and 2.

Figure 2: Experimental design: experiment II.



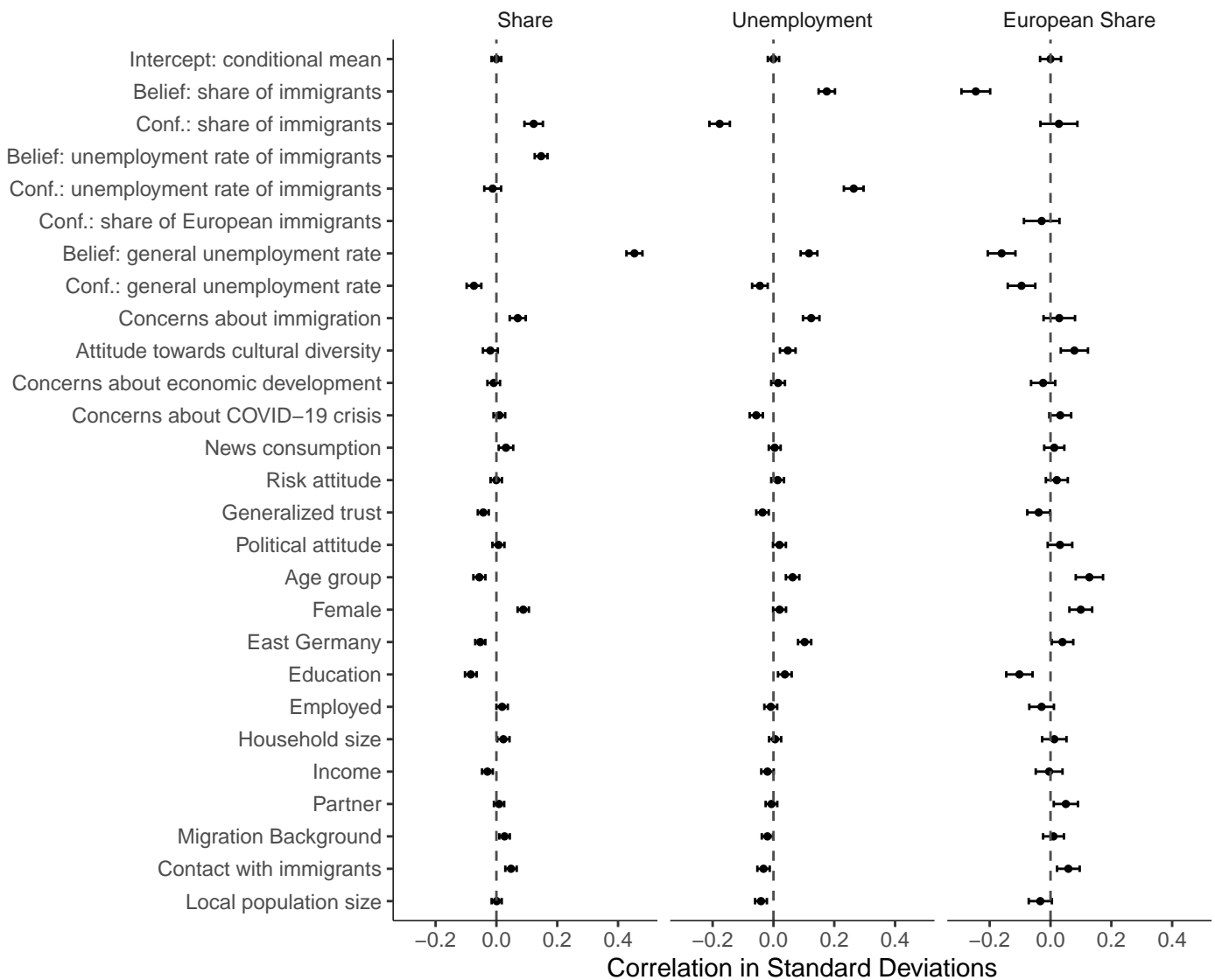
Notes: The figure shows a graphical representation for the experimental design of experiment II. Note that experiment II has been included in survey wave 2 only.

Figure 3: Distribution of prior beliefs about immigrants.



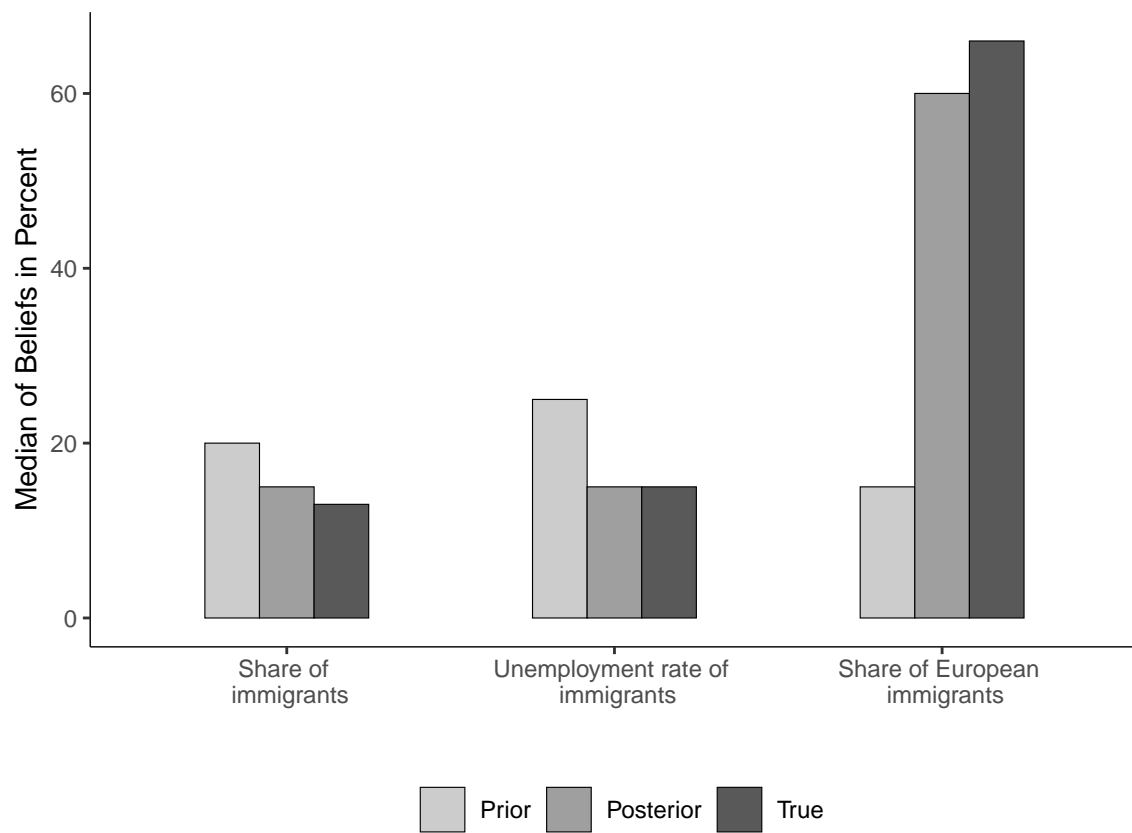
Notes: The figure shows descriptive distributions of respondents' prior beliefs about the share and the unemployment rate of immigrants as well as the European share of immigrants. The shaded areas denote quartile ranges for each distribution. True values for the beliefs correspond to 13 percent for the share of immigrants, 15 percent for the unemployment rate of immigrants, and 66 percent for the share of European immigrants, respectively.

Figure 4: Determinants of biased beliefs about immigrants.



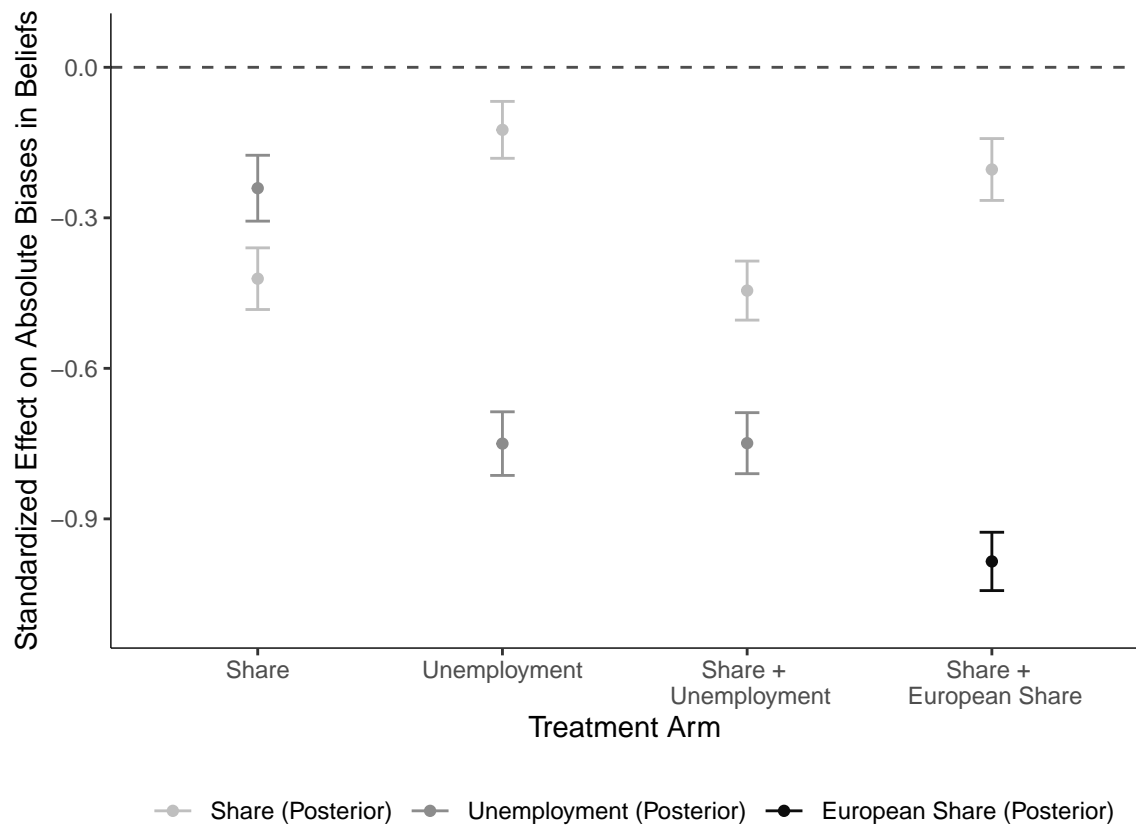
Notes: The figure shows correlations between our range of sociodemographic control variables and biases in prior beliefs about immigrants. The dependent variables and covariates have been standardized in terms of their mean and standard deviation and biases in beliefs are defined in absolute terms. Robust standard errors are employed and 90% confidence intervals are displayed. Specifications for the share and unemployment rate of immigrants further include an indicator for survey waves. As all covariates are standardized to have mean zero, the intercept represents the conditional mean in biases about immigrants.

Figure 5: Belief updating: prior and posterior median values.



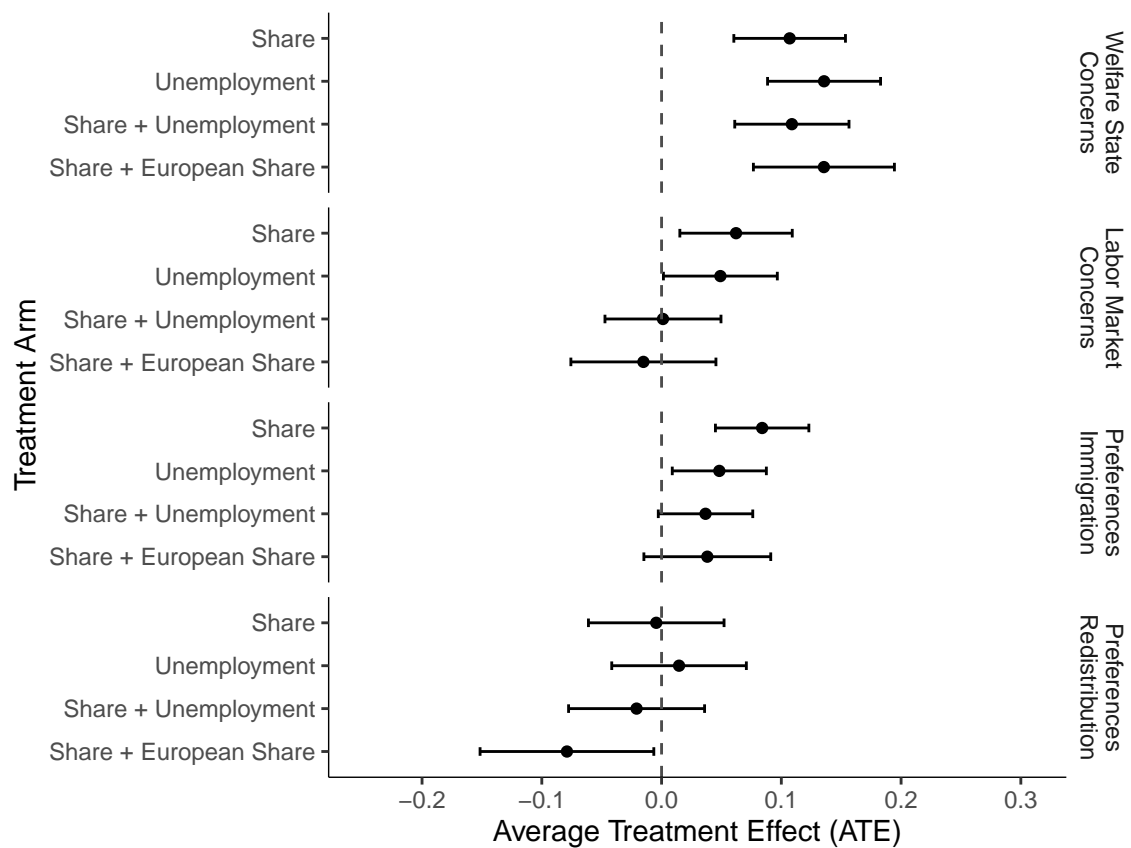
Notes: The figure shows median values of prior and posterior beliefs of treated respondents. In addition, we display true values for each of the three statistics on immigration contained in both experiments.

Figure 6: Between-subject effects of information on biased beliefs about immigrants.



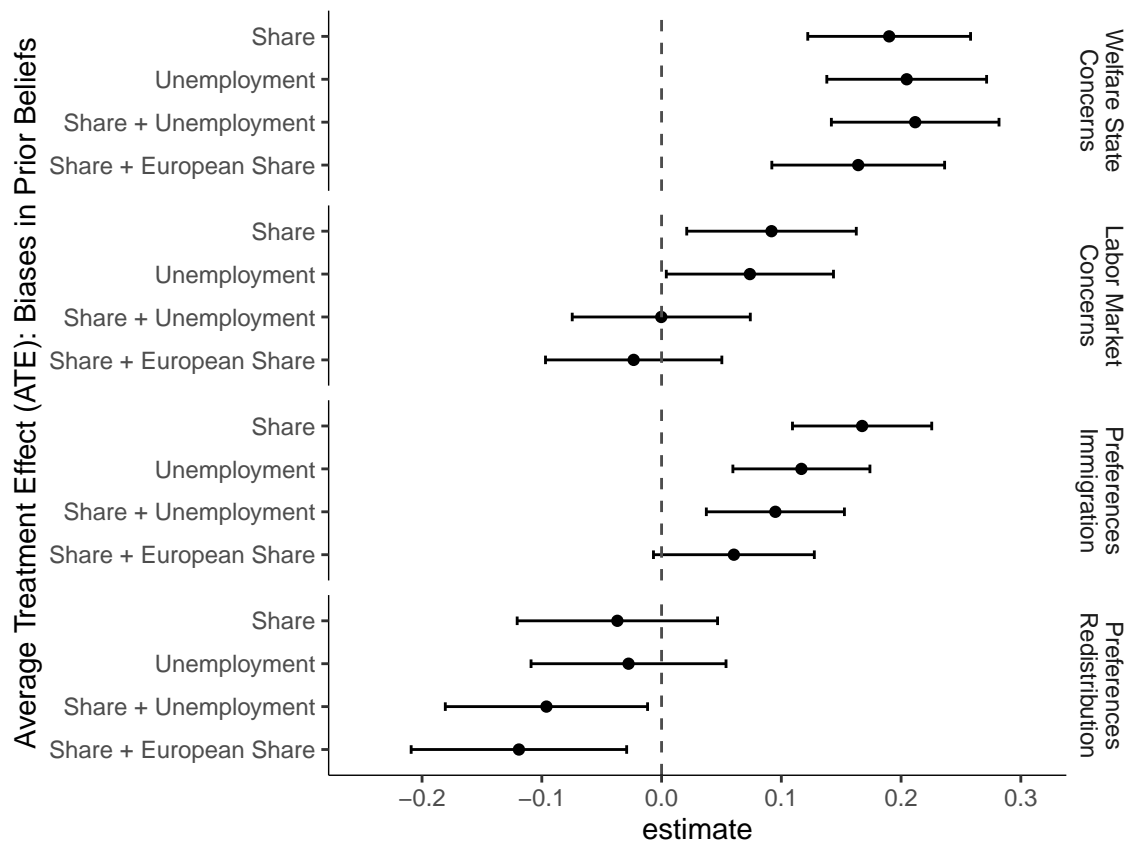
Notes: The figure shows between-subject effects of information on posterior beliefs about immigration, i.e. after randomized receipt of information signals. The dependent variables have been standardized in terms of their mean and standard deviation and biases in beliefs are defined in absolute terms. Robust standard errors are employed and 90% confidence intervals are displayed. The controls comprise of all covariates employed in the balance tests. All specifications further include an indicator for survey waves.

Figure 7: Effects of information on economic concerns and policy preferences: full samples.



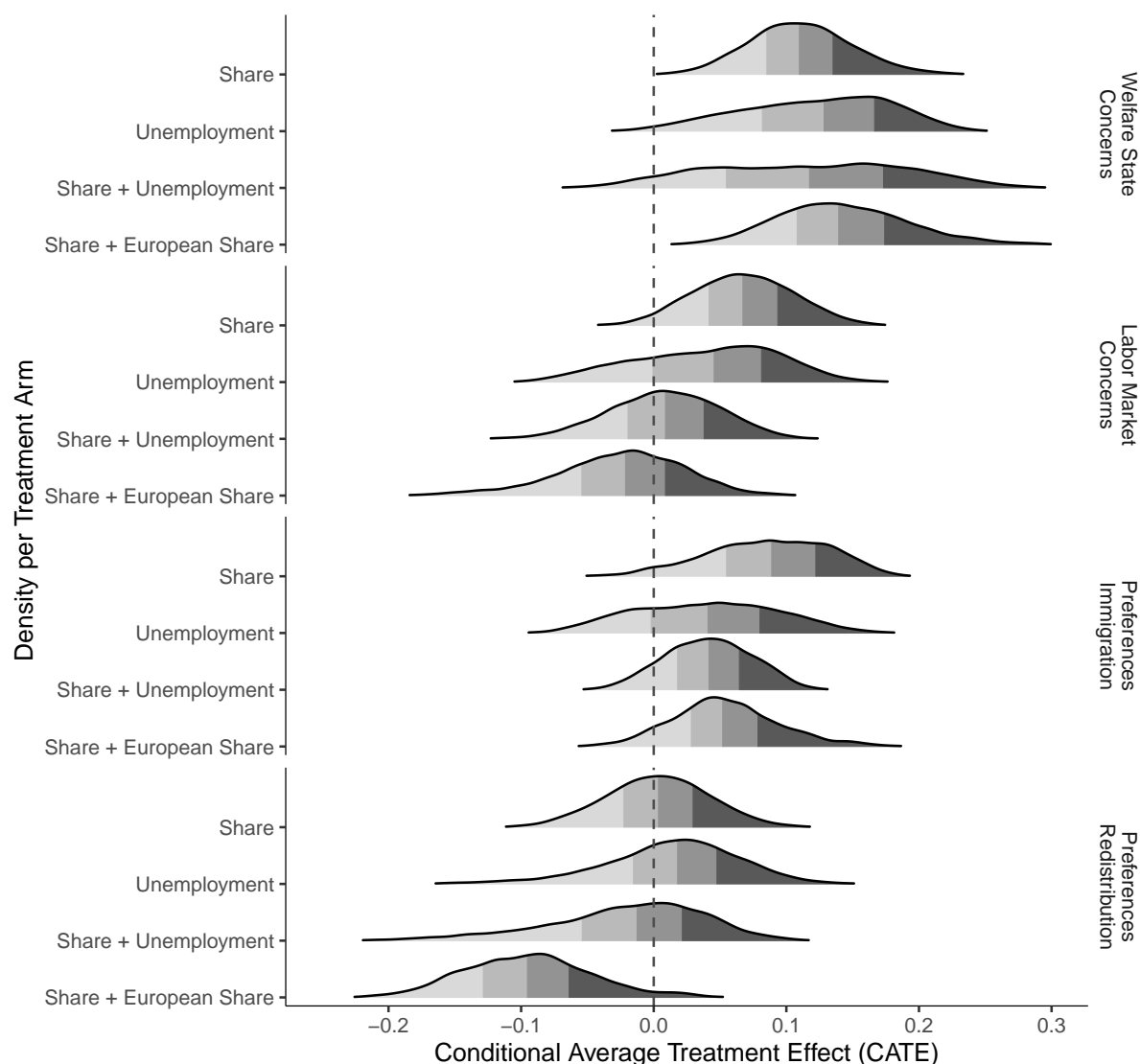
Notes: The figure shows average treatment effects (ATE) of our information interventions. Outcome variables are coded such that a higher value indicates more positive attitudes towards immigration and redistribution and have been standardized in terms of their mean and standard deviation. Robust standard errors are employed and 90% confidence intervals are displayed. The controls comprise of all covariates employed in the balance tests. All specifications further include an indicator for survey waves.

Figure 8: Effects of information on economic concerns and policy preferences: biases in prior beliefs.



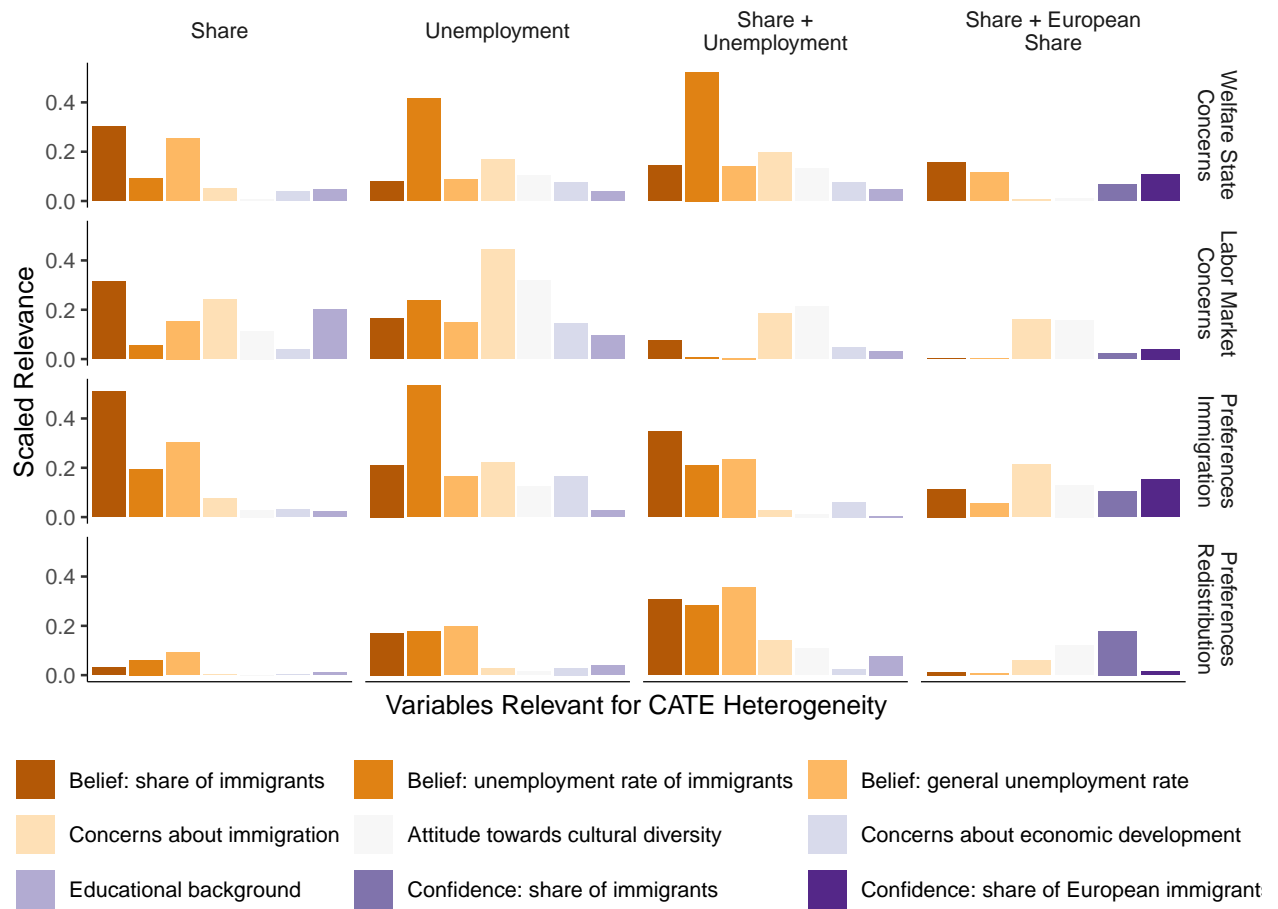
Notes: The figure shows average treatment effects (ATE) of our information interventions for respondents with biases in prior beliefs. Outcome variables are coded such that a higher value indicates more positive attitudes towards immigration and redistribution. The dependent variables have been standardized in terms of their mean and standard deviation. Robust standard errors are employed and 90% confidence intervals are displayed. The controls comprise of all covariates employed in the balance tests. All specifications further include an indicator for survey waves.

Figure 9: Causal forest: estimated distributions of conditional average treatment effects (CATE).



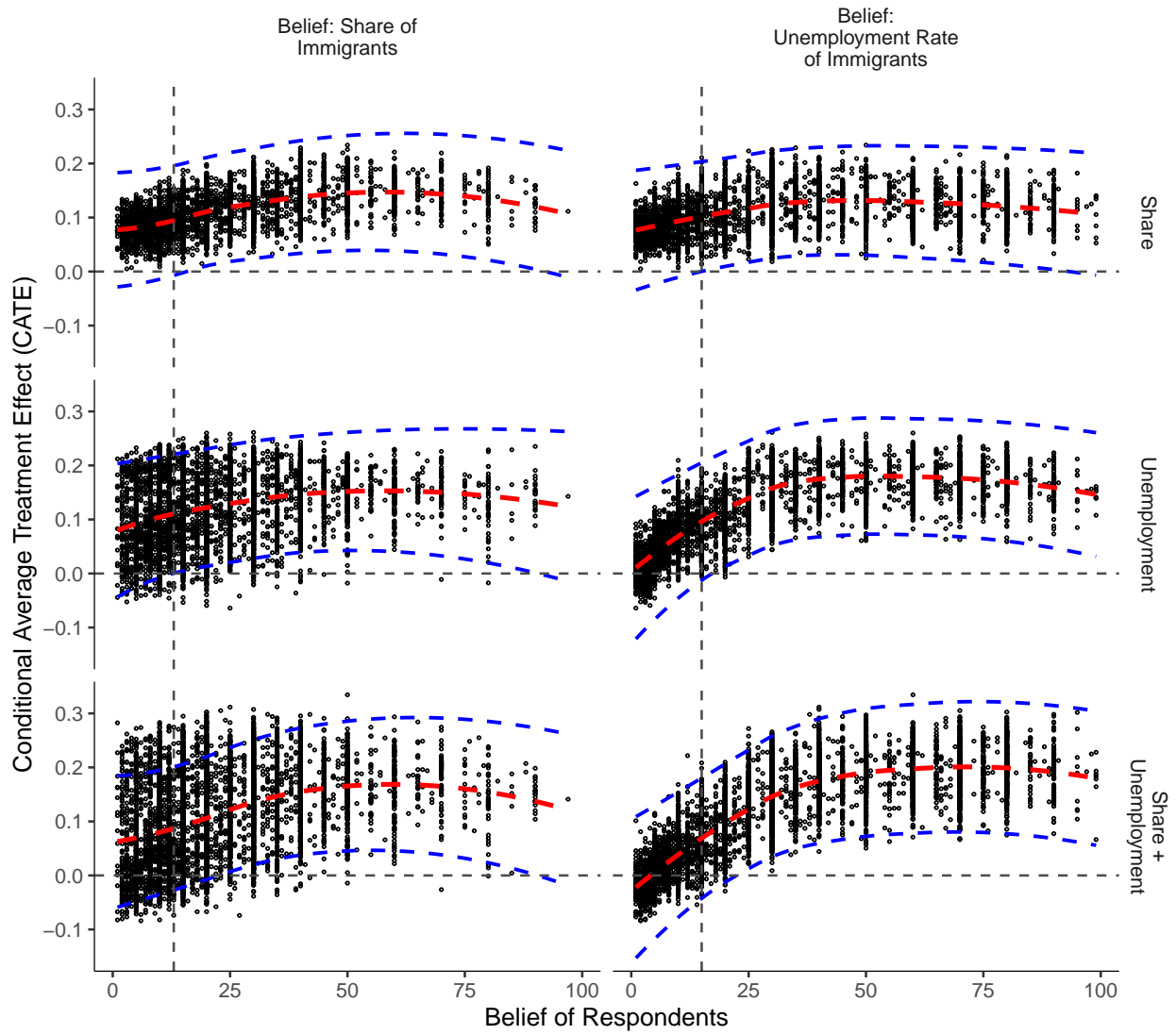
Notes: The figure shows the estimated distributions of conditional average treatment effects (CATE) of our information interventions. The shaded areas denote quartile ranges for each distribution. Outcome variables are coded such that a higher value indicates more positive attitudes towards immigration, or more supportive policy preferences, respectively. The dependent variables have been standardized in terms of their mean and standard deviation. The covariates included in the causal forest estimation comprise of all covariates employed in the balance tests and an indicator for survey waves.

Figure 10: Treatment effect heterogeneity: relevance of covariates.



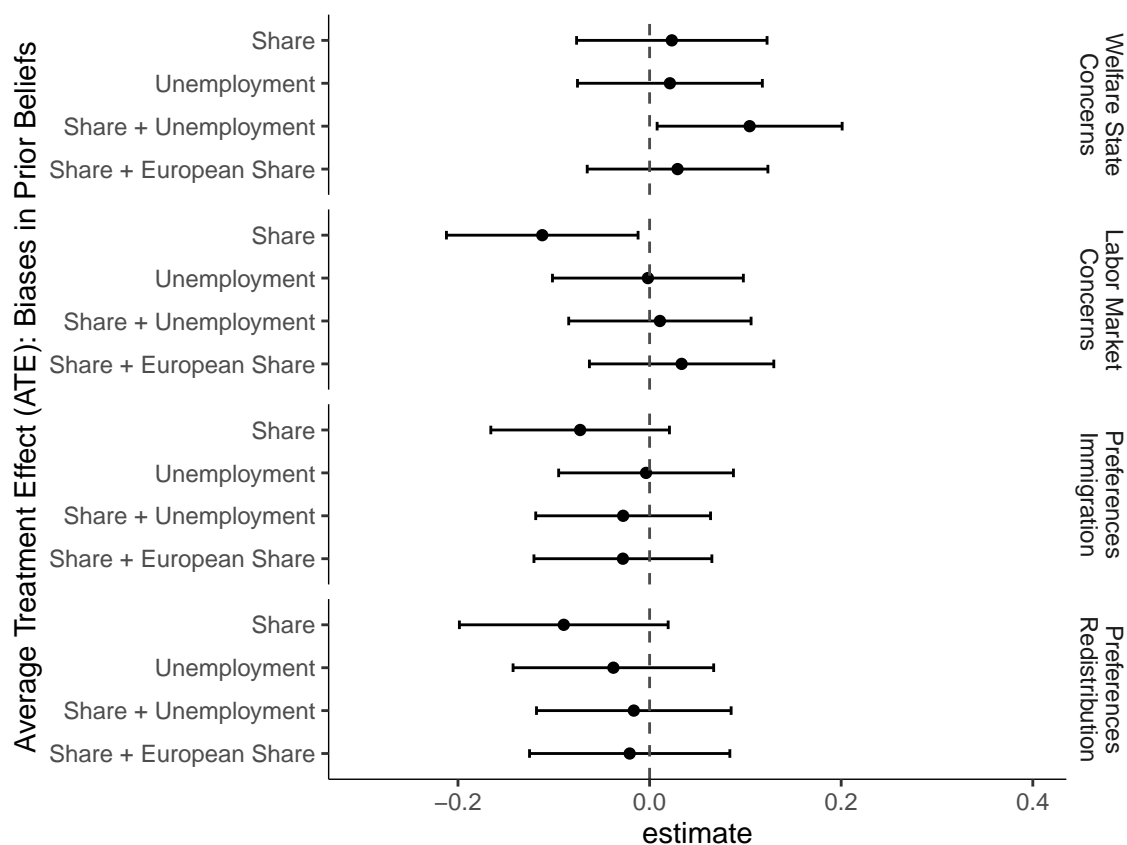
Notes: The figure shows the most relevant covariates in terms of CATE. The relevance is calculated as the variance of the differences in means between each quartile and the full sample for a given covariate, scaled by the full-sample variance of the respective covariate. Please note that measures of relevance should thus be interpreted relatively and not in absolute terms.

Figure 11: Treatment effect heterogeneity: relationship between CATE on welfare state concerns and prior beliefs about immigrants.



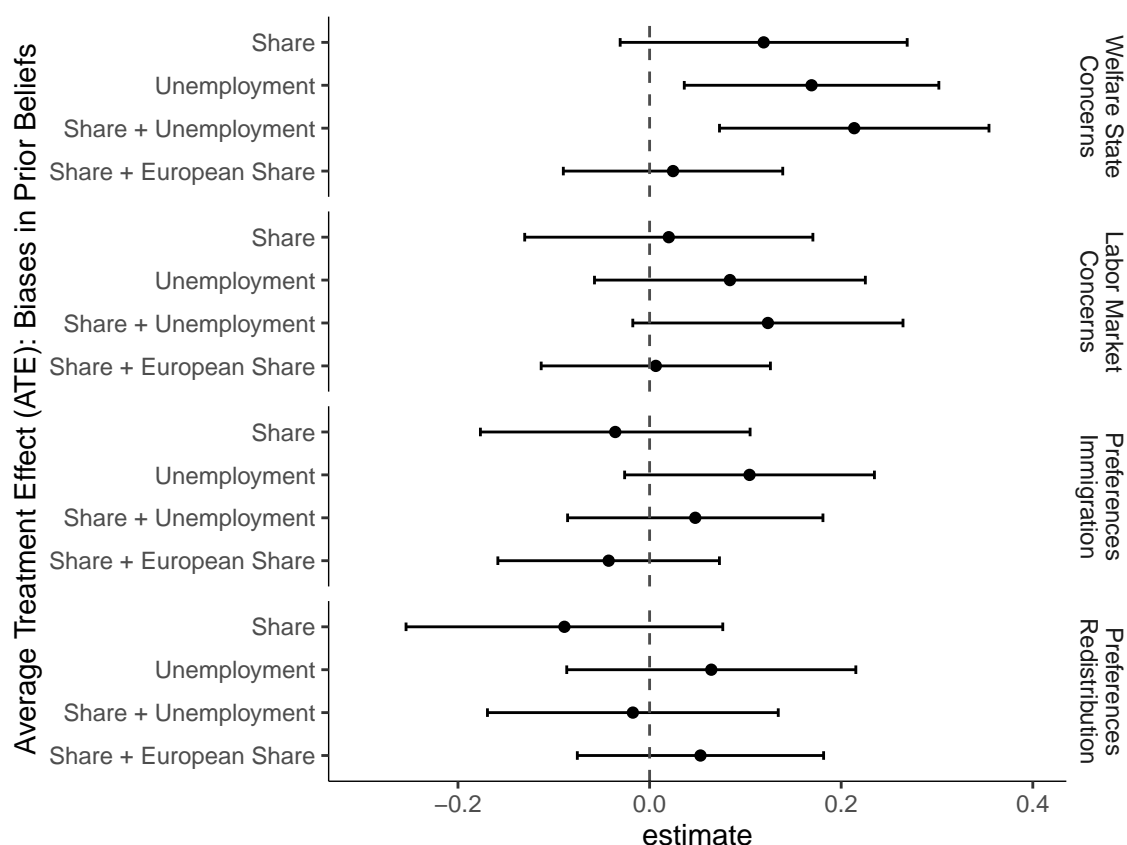
Notes: The figure shows the relationship between the estimated CATE for each treatment arm on welfare state concerns and respondents' prior beliefs about immigrants. The red lines represent locally estimated scatterplot smoothing (LOESS) estimates of the respective CATE. The blue lines represent LOESS estimates of 90% point-wise confidence intervals of the estimated CATE for each respondent. Dashed lines represent true values for the share and unemployment rate of immigrants.

Figure 12: Effects of information on economic concerns and policy preferences: full follow-up sample.



Notes: The figure shows average treatment effects (ATE) of our information interventions. Outcome variables are coded such that a higher value indicates more positive attitudes towards immigration and redistribution. The dependent variables have been standardized in terms of their mean and standard deviation. Robust standard errors are employed and 90% confidence intervals are displayed. The controls comprise of all covariates employed in the balance tests. All specifications further include an indicator for survey waves.

Figure 13: Effects of information on economic concerns and policy preferences: respondents with initially biased beliefs in follow-up.



Notes: The figure shows average treatment effects (ATE) of our information interventions. Outcome variables are coded such that a higher value indicates more positive attitudes towards immigration and redistribution. The dependent variables have been standardized in terms of their mean and standard deviation. Robust standard errors are employed and 90% confidence intervals are displayed. The controls comprise of all covariates employed in the balance tests. All specifications further include an indicator for survey waves.

Appendix A: Additional Tables

Table A1: Experimental balance in covariates: normalized differences.

	Control vs. Share	Control vs. Unemp.	Control vs. Share + Unemp.	Control vs. Share + Europ. Share
Belief: share of immigrants	−0.026	0.011	−0.001	0.016
Conf.: share of immigrants	−0.036	0.012	0.028	0.030
Belief: unemployment rate of immigrants	−0.042	0.039	−0.043	
Conf.: unemployment rate of immigrants	−0.008	0.028	0.036	
Belief: share of European immigrants				−0.005
Conf.: share of European immigrants				0.065
Belief: general unemployment rate	0.018	0.042	0.040	−0.040
Conf.: general unemployment rate	0.038	0.008	0.039	−0.040
Concerns about immigration	0.095	0.131	0.074	−0.075
Attitude towards cultural diversity	0.064	0.045	0.054	−0.010
Concerns about economic development	0.063	0.052	0.078	0.021
Concerns about COVID-19 crisis	0.025	−0.076	0.035	0.051
News consumption	0.062	0.004	0.003	−0.008
Risk attitude	0.011	−0.028	0.041	−0.003
Generalized trust	−0.009	−0.047	−0.011	−0.011
Political attitude	0.020	0.021	−0.018	0.001
Age group	−0.023	−0.060	−0.031	−0.031
Female	0.004	0.025	0.039	−0.028
East Germany	0.073	0.044	0.018	0.051
Education	−0.026	−0.038	−0.050	0.012
Employed	0.056	0.087	0.019	0.003
Household size	−0.012	0.038	0.017	0.000
Income	0.017	0.019	−0.010	−0.018
Partner	0.000	0.023	0.020	−0.110
Migration Background	0.010	−0.002	0.039	−0.050
Contact with immigrants	−0.028	−0.038	−0.015	−0.001
Local population size	0.036	−0.001	0.003	−0.010
Second survey wave	−0.021	0.020	0.035	

Notes: Comparison of treatments and control groups in terms of normalized differences (Imbens and Rubin 2015). As a rule of thumb, normalized differences smaller than 0.25 in absolute terms indicate sufficient balance in a standard regression framework (Imbens and Wooldridge 2009).

Table A2: Determinants of biased beliefs about immigrants.

	Share of immigrants	Unemployment rate of immigrants	Share of Euro- pean immigrants
Intercept: conditional mean	0.000 (0.010)	0.000 (0.011)	0.000 (0.021)
Belief: share of immigrants		0.175*** (0.016)	−0.246*** (0.029)
Conf.: share of immigrants	0.123*** (0.018)	−0.177*** (0.020)	0.028 (0.037)
Belief.: Unemployment rate of immigrants	0.147*** (0.013)		
Conf.: Unemployment rate of immigrants	−0.012 (0.017)	0.264*** (0.020)	
Conf.: share of European immigrants			−0.029 (0.036)
Belief: general unemployment rate	0.454*** (0.016)	0.117*** (0.017)	−0.161*** (0.028)
Conf.: general unemployment rate	−0.074*** (0.015)	−0.045*** (0.016)	−0.095*** (0.028)
Concerns about immigration	0.070*** (0.016)	0.124*** (0.016)	0.029 (0.031)
Attitude towards cultural diversity	−0.020 (0.015)	0.047*** (0.016)	0.078*** (0.027)
Concerns about economic development	−0.009 (0.013)	0.015 (0.014)	−0.024 (0.024)
Concerns about COVID-19 crisis	0.010 (0.012)	−0.057*** (0.013)	0.032 (0.022)
News consumption	0.031** (0.015)	0.004 (0.012)	0.012 (0.020)
Risk attitude	−0.001 (0.011)	0.014 (0.013)	0.021 (0.022)
Generalized trust	−0.043*** (0.011)	−0.036*** (0.012)	−0.039* (0.023)
Political attitude	0.007 (0.012)	0.020 (0.013)	0.031 (0.025)
Age group	−0.056*** (0.012)	0.063*** (0.013)	0.128*** (0.027)
Female	0.088*** (0.011)	0.020 (0.013)	0.100*** (0.023)
East Germany	−0.053*** (0.010)	0.102*** (0.013)	0.039* (0.022)
Education	−0.084*** (0.012)	0.037*** (0.014)	−0.102*** (0.026)
Employed	0.019 (0.011)	−0.009 (0.013)	−0.029 (0.025)

Table A2: Determinants of biased beliefs about immigrants (cont.).

	Share of immigrants	Unemployment rate of immigrants	Share of Euro- pean immigrants
Household size	0.023* (0.012)	0.005 (0.012)	0.013 (0.024)
Income	-0.030*** (0.011)	-0.020 (0.012)	-0.005 (0.027)
Partner	0.009 (0.010)	-0.007 (0.012)	0.050** (0.024)
Migration background	0.027** (0.011)	-0.020* (0.011)	0.010 (0.021)
Contact with immigrants	0.048*** (0.011)	-0.033*** (0.012)	0.059*** (0.023)
Local population size	0.001 (0.010)	-0.041*** (0.012)	-0.034 (0.023)
Adj. R^2	0.406	0.200	0.154
Observations	6265	6265	1934

Notes: The dependent variables have been standardized in terms of their mean and standard deviation and biases in beliefs are defined in absolute terms. Predictors have been standardized in terms of their mean and standard deviation. Robust standard errors are displayed in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Specifications for the share and unemployment rate of immigrants further include an indicator for survey waves.

Table A3: Between-subject effects of information on biased beliefs about immigrants.

	Share of immigrants	Unemployment rate of immigrants	Share of Euro- pean immigrants
Panel A: experiment I:			
Share	−0.421*** (0.037)	−0.241*** (0.040)	
Unemployment	−0.125*** (0.034)	−0.750*** (0.039)	
Share + Unemployment	−0.445*** (0.036)	−0.749*** (0.037)	
Controls	Yes	Yes	
Observations	3913	3913	
Panel B: experiment II:			
Share + European Share	−0.204*** (0.037)		−0.985*** (0.035)
Controls	Yes		Yes
Observations	1934		1934

Notes: Outcome variables represent biases in posterior beliefs in absolute terms and have been standardized in terms of their mean and standard deviation. Robust standard errors are displayed in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The controls comprise of all covariates employed in the balance tests. Specifications for the share and unemployment rate of immigrants further include an indicator for survey waves.

Table A4: Effects of information on economic concerns and policy preferences: experiment I.

	Welfare State Concerns	Labor Market Concerns	Preferences Immigration	Preferences Redistribution
Panel A: full sample:				
Share	0.107*** (0.028)	0.062** (0.029)	0.084*** (0.024)	−0.004 (0.034)
Unemployment	0.136*** (0.029)	0.049* (0.029)	0.048** (0.024)	0.015 (0.034)
Share + Unemployment	0.109*** (0.029)	0.001 (0.029)	0.037 (0.024)	−0.021 (0.035)
Controls	Yes	Yes	Yes	Yes
Observations	6265	6265	6265	6265
Panel B: immigration-adverse biases:				
Share	0.190*** (0.041)	0.092** (0.043)	0.167*** (0.035)	−0.037 (0.051)
Unemployment	0.205*** (0.041)	0.074* (0.042)	0.117*** (0.035)	−0.028 (0.049)
Share + Unemployment	0.212*** (0.043)	−0.000 (0.045)	0.095*** (0.035)	−0.096* (0.051)
Controls	Yes	Yes	Yes	Yes
Observations	3027	3027	3027	3027

Notes: Outcome variables are coded such that a higher value indicates more positive attitudes towards immigration and have been standardized in terms of their mean and standard deviation. Robust standard errors are displayed in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The controls comprise of all covariates employed in the balance tests. All specifications further include an indicator for survey waves.

Table A5: Effects of information on economic concerns and policy preferences: experiment II.

	Welfare State Concerns	Labor Market Concerns	Preferences Immigration	Preferences Redistribution
Panel A: full sample:				
Share + European Share	0.136*** (0.036)	-0.015 (0.037)	0.038 (0.032)	-0.079* (0.044)
Controls	Yes	Yes	Yes	Yes
Observations	1934	1934	1934	1934
Panel B: immigration-adverse biases:				
Share + European Share	0.164*** (0.044)	-0.023 (0.045)	0.060 (0.041)	-0.119** (0.055)
Controls	Yes	Yes	Yes	Yes
Observations	1290	1290	1290	1290

Notes: Outcome variables are coded such that a higher value indicates more positive attitudes towards immigration and have been standardized in terms of their mean and standard deviation. Robust standard errors are displayed in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The controls comprise of all covariates employed in the balance tests.

Table A6: Sample composition and representativity.

	Main Survey		Follow-up		Target Share
	Absolute	Share	Absolute	Share	
Age: 18–29 years	1318	0.161	385	0.131	0.163
Age: 30–39 years	1297	0.158	427	0.145	0.155
Age: 40–64 years	1213	0.148	409	0.139	0.147
Age: 50–64 years	2278	0.278	823	0.280	0.275
Age: 65 years and above	2093	0.255	898	0.305	0.260
Gender: female	4151	0.506	1369	0.465	0.507
Gender*: male	4038	0.493	1568	0.533	0.493
Residence: East Germany	1209	0.147	442	0.150	0.151
Residence: West Germany	6990	0.853	2500	0.850	0.849
Education: low	2957	0.361	1028	0.349	0.373
Education: middle	2543	0.310	936	0.318	0.300
Education: high	2699	0.329	978	0.332	0.327

Notes: The sources for target shares are provided by the German Federal Statistical Office. * In addition, there are 10 respondents who do neither identify as female nor male.

Table A7: Differences between follow-up and non-follow-up respondents.

	Mean: Follow-up	Mean: Non-follow-up	Normalized Difference
Belief: share of immigrants	23.105	24.370	−0.079
Conf.: share of immigrants	4.097	3.977	0.048
Belief: unemployment rate of immigrants	30.962	30.950	0.001
Conf.: unemployment rate of immigrants	3.803	3.740	0.025
Belief: share of European immigrants	21.774	22.995	−0.057
Conf.: share of European immigrants	3.821	3.574	0.100
Belief: general unemployment rate	15.124	17.261	−0.132
Conf.: general unemployment rate	4.767	4.634	0.052
Concerns about immigration	6.050	5.922	0.040
Attitude towards cultural diversity	5.310	5.267	0.015
Concerns about economic development	5.842	6.207	−0.142
Concerns about COVID-19 crisis	5.121	5.612	−0.169
News consumption	63.609	65.313	−0.025
Risk attitude	3.809	3.883	−0.030
Generalized trust	4.069	4.089	−0.008
Political attitude	4.779	4.729	0.026
Age group	3.483	3.211	0.194
Female	0.465	0.529	−0.128
East Germany	0.150	0.146	0.012
Education	1.983	1.960	0.027
Employed	0.499	0.539	−0.079
Household size	2.118	2.247	−0.059
Income	2.500	2.523	−0.019
Partner	0.631	0.526	0.213
Migration Background	0.224	0.231	−0.016
Contact with immigrants	2.691	2.735	−0.036
Local population size	3.241	3.218	0.016

Notes: Comparison of respondents in the follow-up and non-follow-up samples in terms of mean values and normalized differences (Imbens and Rubin 2015).

Table A8: Experimental balance in covariates in follow-up sample: normalized differences.

	Control vs. Share	Control vs. Unemp.	Control vs. Share + Unemp.	Control vs. Share + Europ. Share
Belief: share of immigrants	0.020	0.019	0.014	0.017
Conf.: share of immigrants	0.017	0.036	0.130	0.021
Belief: unemployment rate of immigrants	-0.011	0.113	-0.028	
Conf.: unemployment rate of immigrants	-0.003	-0.019	0.062	
Belief: share of European immigrants				-0.053
Conf.: share of European immigrants				0.090
Belief: general unemployment rate	0.035	-0.034	-0.007	-0.066
Conf.: general unemployment rate	0.029	0.006	0.063	-0.064
Concerns about immigration	0.016	0.022	-0.030	-0.047
Attitude towards cultural diversity	0.024	-0.015	0.021	0.009
Concerns about economic development	0.058	-0.003	0.002	0.008
Concerns about COVID-19 crisis	0.015	-0.112	-0.030	0.060
News consumption	0.062	-0.053	-0.030	-0.053
Risk attitude	0.050	-0.115	-0.029	-0.027
Generalized trust	-0.027	-0.085	-0.060	0.010
Political attitude	-0.018	-0.065	-0.056	-0.034
Age group	-0.166	-0.201	-0.164	0.026
Female	0.056	0.074	0.052	-0.018
East Germany	0.068	0.034	0.014	0.052
Education	0.037	-0.020	-0.058	-0.064
Employed	0.080	0.114	0.081	-0.072
Household size	-0.083	-0.033	-0.053	-0.040
Income	-0.048	-0.046	-0.090	-0.091
Partner	0.065	0.073	0.036	-0.160
Migration Background	0.020	-0.047	0.040	-0.037
Contact with immigrants	0.022	-0.098	-0.055	0.033
Local population size	0.142	-0.042	-0.011	-0.017

Notes: Comparison of treatments and control groups in terms of normalized differences (Imbens and Rubin 2015). As a rule of thumb, normalized differences smaller than 0.25 in absolute terms indicate sufficient balance in a standard regression framework (Imbens and Wooldridge 2009).

Table A9: Effects of information on economic concerns and policy preferences: follow-up sample for experiment I.

	Welfare State Concerns	Labor Market Concerns	Preferences Immigration	Preferences Redistribution
Panel A: full sample:				
Share	0.023 (0.060)	-0.112* (0.061)	-0.072 (0.057)	-0.090 (0.066)
Unemployment	0.021 (0.059)	-0.002 (0.061)	-0.004 (0.055)	-0.038 (0.064)
Share + Unemployment	0.104* (0.059)	0.011 (0.058)	-0.028 (0.055)	-0.016 (0.062)
Controls	Yes	Yes	Yes	Yes
Observations	1963	1963	1963	1963
Panel B: immigration-adverse biases:				
Share	0.119 (0.091)	0.020 (0.091)	-0.036 (0.085)	-0.089 (0.100)
Unemployment	0.169** (0.081)	0.084 (0.086)	0.104 (0.079)	0.064 (0.092)
Share + Unemployment	0.214** (0.085)	0.123 (0.086)	0.048 (0.081)	-0.017 (0.092)
Controls	Yes	Yes	Yes	Yes
Observations	932	932	932	932

Notes: Outcome variables represent follow-up variables and are coded such that a higher value indicates more positive attitudes towards immigration and have been standardized in terms of their mean and standard deviation. Robust standard errors are displayed in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The controls comprise of all covariates employed in the balance tests. All specifications further include an indicator for survey waves.

Table A10: Effects of information on economic concerns and policy preferences: follow-up sample for experiment II.

	Welfare State Concerns	Labor Market Concerns	Preferences Immigration	Preferences Redistribution
Panel A: full sample:				
Share + European Share	0.029 (0.057)	0.033 (0.058)	-0.028 (0.056)	-0.021 (0.063)
Controls	Yes	Yes	Yes	Yes
Observations	979	979	979	979
Panel B: immigration-adverse biases:				
Share + European Share	0.024 (0.070)	0.006 (0.073)	-0.043 (0.070)	0.053 (0.078)
Controls	Yes	Yes	Yes	Yes
Observations	638	638	638	638

Notes: Outcome variables represent follow-up variables and are coded such that a higher value indicates more positive attitudes towards immigration and have been standardized in terms of their mean and standard deviation. Robust standard errors are displayed in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The controls comprise of all covariates employed in the balance tests.

Appendix B: Overview of Variables

B.1 Outcome variables:

Variable name	Type	Description
Welfare state concerns	Numerical (0–10)	Respondent's welfare state concerns as measured by the following question: <i>"Immigrants pay taxes and receive social benefits from the health care and social insurance systems. On balance, do you think that immigrants in Thuringia receive more social benefits than they pay taxes, or that they pay more taxes than they receive social benefits?"</i> . Answers range from 0 for "Receive more social benefits" to 10 for "Pay more taxes".
Labor market concerns	Numerical (0–10)	Respondent's labor state concerns as measured by the following question: <i>"Do you think that immigrants rather take away jobs from workers in Thuringia, or that they rather help to create new jobs?"</i> . Answers range from 0 for "Take jobs away" to 10 for "Create new jobs".
Preferences Immigration	Numerical (1–5)	Respondent's immigration policy preferences as measured by the following survey question: <i>"Do you think that the number of immigrants coming to Thuringia each year should be: decreased a lot / decreased slightly / stay the same / increased slightly / increased a lot?"</i> .
Preferences Redistribution	Numerical (0–10)	Respondent's preferences for redistribution as measured by the following survey question: <i>"Some people think that the government should not care about income differences between rich and poor people. Others think that the government should do everything in its power to reduce income inequality. What do you think?"</i> . Answers range from 0 for "Government should not care about income inequality" to 10 for "Government should do everything against income inequality".

B.2 Belief Elicitation:

Variable name	Type	Description
Belief: share of immigrants	Numerical	Respondent's prior beliefs about the share of immigrants in Germany (true value 13%) based on the question: "Now it is about the share of immigrants in Germany. What do you estimate, please answer spontaneously: What percentage of people living in Germany do not have German citizenship?". Hint text (clickable via question mark icon): "The percentage is understood here as the number of immigrants per 100 inhabitants in Germany."
Confidence: share of immigrants	Numerical (0–10)	Measures a respondent's confidence when stating prior beliefs about the share of immigrants on an 11-point scale from 0 for "Very unconfident" to 10 for "Very confident".
Belief: unemployment rate of immigrants	Numerical	Respondent's prior beliefs about the unemployment rate of immigrants in Germany (true value 15%) based on the question: "Now it is about the unemployment rate of working-age immigrants in Germany. What do you estimate, please answer spontaneously: What percentage of these people are unemployed?". Hint text (clickable via question mark icon): "The percentage is understood here as the number of unemployed persons per 100 immigrants of working age in Germany. Immigrants are considered unemployed if they are registered as unemployed with the Federal Employment Agency. Asylum seekers and tolerated persons are included in the unemployment rate if they have a work permit but no job and are registered as unemployed."
Confidence: unemployment rate of immigrants	Numerical (0–10)	Measures a respondent's confidence when stating prior beliefs about the unemployment rate of immigrants on an 11-point scale from 0 for "Very unconfident" to 10 for "Very confident".

Variable name	Type	Description
Belief: share of European immigrants	Numerical	Respondent's prior beliefs about the share of European immigrants in Germany (true value 66%) based on the question: "Now it is about all immigrants who have come to Germany in 2019. What do you estimate, please answer spontaneously: What percentage of these immigrants come from a European country?". Hint text 1 (always visible): "European countries include the countries of the European Union and European third countries including Turkey and the Russian Federation." Hint text 2 (clickable via question mark icon): "The percentage is understood here as the number of European persons per 100 immigrants to Germany."
Confidence: share of European immigrants	Numerical (0–10)	Measures a respondent's confidence when stating prior beliefs about the share of European immigrants on an 11-point scale from 0 for "Very unconfident" to 10 for "Very confident".
Belief: general unemployment rate	Numerical	Respondent's prior beliefs about the general unemployment rate in Germany (true value 5%, not provided during treatments) based on the following question: "Now it is about the unemployment rate in Germany. What do you estimate, please answer spontaneously: What percentage of people of working age in Germany are unemployed?".
Confidence: general unemployment rate	Numerical (0–10)	Measures a respondent's confidence when stating prior beliefs about the general unemployment rate on an 11-point scale from 0 for "Very unconfident" to 10 for "Very confident".

B.3 Further covariates:

Variable name	Type	Description
Concerns about immigration	Numerical (0–10)	Measures a respondent's concerns about immigration pre-treatment on an 11-point scale from 0 for "Not concerned" to 10 for "Very concerned".
Attitude towards cultural diversity	Numerical (0–10)	Measures a respondent's attitude towards cultural diversity according to his/her agreement to the following statement pre-treatment: "It is better for a country when everyone shares the same customs and traditions." on an 11-point scale from 0 for "Disagree strongly" to 10 for "Agree strongly".
Concerns about economic development	Numerical (0–10)	Measures a respondent's concerns about the development of the economy in Germany pre-treatment on an 11-point scale from 0 for "Not concerned" to 10 for "Very concerned".
Concerns about COVID-19 crisis	Numerical (0–10)	Measures a respondent's concerns about the COVID-19 crisis on an 11-point scale from 0 for "Not concerned" to 10 for "Very concerned".
News consumption	Numerical	Measures news consumption by respondent in minutes on a typical day.
Risk attitude	Numerical (0–10)	Measures a respondent's attitude towards risk on an 11-point scale from 0 for "Not at all willing to take risks" to 10 for "Very willing to take risks".
Generalized trust	Numerical (0–10)	Measures a respondent's generalized trust on an 11-point scale from 0 for "You cannot be too careful" to 10 for "Most people can be trusted".
Political attitude	Numerical (0–10)	Measures a respondent's generalized political attitude on an 11-point scale from 0 for "Left" to 10 for "Right".
Age group	Numerical (1–5)	Respondent's age group according to the ranges: <i>16 to 29, 30 to 39, 40 to 49, 50 to 64, 65 and older.</i>
Female	Binary	Indicates a respondent's gender.
East Germany	Binary	Indicates whether a respondent lives in East Germany (excluding Berlin).

Variable name	Type	Description
Education	Numerical (1–3)	Respondent’s education based on highest school-leaving certificate according to the ranges: <i>low, medium, high</i> .
Employed	Binary	Indicates whether a respondent is in employment.
Household size	Numerical	Number of persons living in respondent’s household.
Income	Numerical (1–5)	Respondent’s household net income in Euro according to the ranges: <i>Below 1500, 1500–2500, 2500–3500, 3500–4500, 4500 and above</i> .
Migration background	Binary	Indicates whether respondent or one of his/her parents were born outside of Germany.
Partnership	Binary	Indicates whether respondent lives in a partnership.
Contact with immigrants	Numerical (1–5)	Measures self-assessed amount of contact with immigrants in a respondent’s neighborhood.
Local population size	Numerical (1–5)	Population size in respondent’s location according to the ranges: <i>Below 5000, 5000–10000, 10000–50000, 50000–200000, 200000 and above</i> .

Appendix C: Wording of Information Treatments

The precise wording of our information interventions is presented below:²¹

Share: “We will take a brief look at your estimate of the share of immigrants in Germany:

The share of immigrants in Germany is around 13 percent. Your estimate of [show estimate] was therefore [too low / quite accurate / too high]”.

Unemployment: “We will take a brief look at your estimate of the unemployment rate of immigrants in Germany:

The unemployment rate of immigrants in Germany is around 15 percent. Your estimate of [show estimate] was therefore [too low / quite accurate / too high]”.

Share + Unemployment: “We will take a brief look at your two estimates:

The share of immigrants in Germany is around 13 percent. Your estimate of [show estimate] was therefore [too low / quite accurate / too high].

The unemployment rate of immigrants in Germany is around 15 percent. Your estimate of [show estimate] was therefore [too low / quite accurate / too high]”.

Share + European Share: “We will take a brief look at your two estimates:

The share of immigrants in Germany is around 13 percent. Your estimate of [show estimate] was therefore [too low / quite accurate / too high].

The share of European immigrants in Germany is around 66 percent. Your estimate of [show estimate] was therefore [too low / quite accurate / too high]”.

²¹For conditional feedback, we allow for a margin of error of ± 1 percentage points for respondents to receive feedback for “quite accurate” beliefs. In addition to the information treatments, we disclose the sources of the information provided to ensure its credibility. Specifically, the information on the share of immigrants stems from the German Federal Statistical Office, the information on the unemployment rate of immigrants stems from the German Federal Employment Agency, and the information on the share of European immigrants stems from the German Federal Office for Migration and Refugees.