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Information about Inequality in Early Child Care Reduces Polarization in Policy Preferences*

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Abstract

We investigate public preferences for equity-enhancing policies in access to early child care, using a survey experiment with a representative sample of the German population ($n \approx 4,800$). We observe strong misperceptions about migrant-native inequalities in early child care that vary by respondents' age and right-wing voting preferences. Randomly providing information about the actual extent of inequalities has a nuanced impact on the support for equity-enhancing policy reforms: it increases support for respondents who initially underestimated these inequalities, and tends to decrease support for those who initially overestimated them. This asymmetric effect leads to a more consensual policy view, substantially decreasing the polarization in policy support between under- and overestimators. Our results suggest that correcting misperceptions can align public policy preferences, potentially leading to less polarized debates about how to address inequalities and discrimination.

Keywords: child care, policy support, information, inequality, discrimination, survey experiment

JEL: I24, J18, J13, D83, C99

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1. Introduction

Inequality of opportunity is a pressing societal challenge in many countries around the world. The opportunities available to individuals are frequently shaped within the education system (Corak, 2013), where socioeconomic disparities in accessing quality education emerge at an early stage. These inequalities are already evident in early child care, where disadvantaged groups are strongly underrepresented (OECD, 2018; García et al., 2020; Heckman and Landersø, 2021).[1] This situation is particularly concerning given the pivotal role of early child care in fostering equality of opportunity and enabling disadvantaged groups to actively engage and contribute to various societal sectors (e.g., Almond and Currie, 2011; Heckman et al., 2013; Campbell et al., 2014). Addressing these foundational inequalities in early childhood is therefore a crucial policy imperative.

However, the implementation of policies that enhance equity is at risk of being compromised by the increasingly polarized political discourse concerning policies aimed at disadvantaged groups, such as minorities or migrants (Dixit and Weibull, 2007; Karakas and Mitra, 2019; Bonomi et al., 2021; Guriev and Papaioannou, 2022). This polarization could potentially deadlock initiatives designed to tackle societal inequality. Many suggest that a primary cause of political disagreement over policies promoting equity — such as anti-discrimination measures, affirmative action, or transparency requirements — stems from differing perceptions of the extent of these inequalities (Settele, 2022; Bursztyn and Yang, 2022; Haaland and Roth, 2023). However, public perceptions about inequality in access to early child care have not yet been investigated.

This study aims to understand the public's perception of migrant-native inequalities in the early child care market, as well as the level of support for equity-enhancing policy reforms. Studying public policy preferences, as we do, has become increasingly popular in economic research in recent years (Haaland et al., 2023), given their crucial role in determining the political viability of reform proposals. Public policy preferences not only directly influence politicians' policy stances (Hager and Hilbig, 2020) but also predict real-world political behaviors, such as signing petitions (see Section 6 for details).[2] Most

[1] Socioeconomic disparities in access to early child care can emerge for several reasons, including complex admission processes (Hermes et al., 2021) and discriminatory behavior of child care center managers (Hermes et al., 2023a). Across most OECD countries, the resulting socioeconomic enrollment gaps are substantial, even when accounting for parental enrollment preferences (OECD, 2018; Jessen et al., 2020).

[2] In Germany, public political engagement, such as protests and referenda on issues like child care policies (SZ, 2024), school policies (Spiegel, 2010), and university tuition (Lergetporer and Woessmann, 2023), occurs regularly and sometimes directly affects education policy making.

importantly, we employ an information provision experiment (Haaland et al., 2023) to causally examine how people's perceptions of these gaps affect their preferences for policy reforms. We investigate the interplay between prior beliefs and reform support, and the information effect on polarization of policy preferences. We implemented the experiment in a large-scale survey with about 4,800 respondents representative of the German population in terms of gender, age, educational background, and residential state.

In the survey, we first elicit respondents' beliefs regarding migrant-native gaps in the early child care market. Our study reveals two important descriptive findings regarding the general public's beliefs. First, we observe substantial variation in prior beliefs about migrant-native gaps, indicating a general lack of knowledge (or awareness) about this issue. Second, respondents consistently underestimate existing inequalities in child care enrollment rates. At the same time, they overestimate the migrant-native gap in response rates to child-care-related email inquiries by parents.[3] Investigating the correlation of prior beliefs about migrant-native inequalities with observable characteristics, we find that respondents who are younger, more educated, and have non-right-wing voting preferences, respectively, are less likely to underestimate inequalities in the child care market.

We also elicited support for various equity-enhancing policy reforms: the provision of (publicly subsidized) additional slots, a centralized admission process, additional financial incentives for child care centers to admit migrants, and preferential admission of migrants. These policies were chosen as existing literature suggests their effectiveness in addressing various dimensions of unequal access to early child care (see Section 3.2 for details). The provision of additional slots was the most favored, garnering support by 70% of survey respondents in the control group. A centralized admission process followed with 40% support. Financial incentives and preferential treatment of migrants were less popular, receiving only 25% and 7% support, respectively. We further observe a notable correlation between prior beliefs about migrant-native gaps (specifically regarding child care centers' responsiveness to emails) and support for equity-enhancing policy reforms. To determine

[3] We focus on Turkish migrants, as they represent the largest migrant group in Germany (see Section 3 for details). Respondents, on average, believe that 36.5 out of 100 Turkish migrant children are enrolled in early child care, while this is true for only 12 out of 100 (Jessen et al., 2020). The migrant-native gap is 21 percentage points, as 33 out of 100 native children are enrolled. Respondents also believe that, on average, 52.4 out of 100 parents of Turkish migrant children receive a response to an inquiry, while this is true for 63 out of 100 (Hermes et al., 2023a). As 71 out of 100 natives receive a response, the migrant-native gap here is 8 percentage points. Crucially, the questions that elicited respondents' beliefs about migrants' enrollment and email response rates included information on the corresponding figures for natives, an anchoring technique commonly employed to reduce measurement error and clarify the interpretation of possible information treatment effects (see Section 3.1).

if this correlation indicates a causal relationship, we experimentally analyze the impact of exogenous shifts in these beliefs on reform support.

In the experiment, we randomly provide information about the actual extent of migrant-native disparities in early child care. Studying the causal relationship between beliefs and policy preferences with observational data is challenging due to the lack of exogenous variation in beliefs about migrant-native inequalities, and a lack of individual-level data on reform preferences. Our experimental survey addresses these identification challenges. The first treatment informs about the migrant-native *enrollment* gap in early child care (Jessen et al., 2020). The second treatment informs about the migrant-native gap in *response rates* of child care centers to parental email requests (Hermes et al., 2023a), and the third treatment combines both pieces of information. In the treatments, information is conveyed both verbally and through simple visual representations. The control group does not receive any of this information.

Providing information about the extent of migrant-native inequalities has no significant effect on reform support on average. However, the information treatments are successful in updating respondents' beliefs. Scrutinizing respondents' perception of the reasons for unequal chances between migrants and natives, 27% of control-group respondents attribute it to the cultural background of migrants.[4] Providing treatment information significantly increases the perception that disparities are based on cultural background by 3.8 percentage points (13.8%, $p = .017$).

It is reasonable to expect that the extent to which information provision affects reform support crucially depends on respondents' prior beliefs about existing inequalities. Therefore, we investigate how the reaction to treatment information depends on prior beliefs. Respondents who *underestimate* inequalities in the child care market (i.e., respondents who initially underestimated migrant-native gaps) exhibit relatively little support for equity-enhancing policies, while those overestimating these gaps are more in favor of such policies. However, when respondents who underestimate inequalities receive information about the actual gaps, they significantly increase their reform support. Those who initially overestimate the gaps tend to decrease reform support upon receiving information, albeit not statistically significantly so. As a result, information provision leads to more consensual reform preferences: the gap in reform support between underestimators and overestimators decreases by 43% in the treatment group as compared to the control group.

[4]More effort required from child care centers to cater to migrant children (51%) and preferences of other parents (46%) were regarded as even more relevant by respondents. Note that multiple answers could be selected.

We confirm this finding in a Causal Forest analysis, which shows that treatment effect heterogeneities depend strongly on prior beliefs about native-migrant gaps.

Furthermore, our large sample size allows us to explore treatment effect heterogeneities across various subgroups. We observe the pattern of converging reform support between females and males and between parents and non-parents.[5] However, one group shows a strikingly different pattern of belief updating: right-wing voters. Compared to other respondents, they generally view the child care market as less discriminatory against migrants and exhibit significantly lower support for equity-enhancing policy reforms. Interestingly, they tend to counter-intuitively *reduce* their policy support upon receiving accurate information. This reaction is consistent with an amplification of party identification discussed in the political-science literature, a mechanism to avoid the discomfort associated with challenging one's own beliefs (Campbell, 1980; Bartels, 2002).

Our study contributes to the existing literature in two key ways. First, we build upon prior research that examines policy preferences about the education system (e.g., Lergetporer et al., 2018; Cattaneo et al., 2020). Educational inequality early in life has profound consequences, paving the way for disparities in lifetime income and human capital accumulation later on (Heckman et al., 2010; Hermes et al., 2023a). However, our study is the first to investigate the causal determinants of public support for policy reforms aimed specifically at promoting equity in access to early child care. Doing so, we extend the work of Haaland and Roth (2023), who studied the effect of providing information about gaps in response rates to applications by white and black Americans on support for pro-black policies. We also explore the impacts of different types of information provided.

Second, our study contributes to the growing body of literature that investigates the impacts of information on the polarization of policy preferences. Previous survey experiments have predominantly shown that additional information about minorities leads to increased polarization of policy preferences (e.g., Naumann et al., 2018; Lergetporer et al., 2021; Settele, 2022), or has little effect on polarization (e.g., Hopkins et al., 2019; Alesina et al., 2023; Haaland and Roth, 2023). In contrast, our study provides evidence suggesting that information about minorities can actually decrease polarization, thereby facilitating the formation of more consensual policy reform preferences. Notably, in a recent literature survey on information provision experiments, Marino et al. (2023) identify only one study in which information about the share of (undocumented) immigrants reduces polarization

[5]Females, initially with lower reform support, show a greater increase than males when provided with information. Conversely, parents, initially more supportive of reforms, exhibit a more pronounced decrease in support upon receiving information.

in immigration policy preferences (Grigorieff et al., 2020). A potential explanation for our findings is the wide variation in respondents' prior beliefs, suggesting a general lack of awareness or knowledge about migrant-native inequalities in early child care. Consequently, these beliefs may be more readily updated upon exposure to new information, as they are less entangled with respondents' identity concerns.

The remainder of this paper is structured as follows. Section 2 provides information on the institutional background of the early child care market in Germany. Section 3 describes the survey data and the experimental design. Section 4 reports our results regarding respondents' prior beliefs about early child care for migrants and average treatment effects on support for policy reforms. Section 5 analyzes treatment effect heterogeneities. Section 6 provides a discussion on the potential limitations of our study and an outlook on how future research may address them. Section 7 concludes.

2. Institutional Background

In Germany, child care is available to all children up until they begin school at the age of six, with specific provision for two age groups: (i) children under the age of three years ("*Krippe*"), and (ii) children between the ages of three and six years ("*Kindergarten*"). Every child is entitled to a child care slot from the age of one year onward. The government subsidizes early child care, covering approximately three-quarters of the total cost (Spiess, 2013). Parents pay very low child care fees (on average 250 EUR per month), equivalent to 10% of the average income. Lower-income families are eligible for fee reductions or exemptions (Felfe and Lalive, 2018). Compared to other countries, the quality of early child care in Germany is relatively high and homogeneous, as measured by group sizes, staff-to-child ratios, and other indicators (Felfe and Lalive, 2018).

While child care in Germany is often described as "universal," the reality is quite different. For instance, only about 34% of children under three years of age are enrolled in early child care. This figure increases significantly for children between two and three years, with a 55% enrollment rate. Notably, over 90% of children attend Kindergarten, indicating widespread participation in some form of child care prior to school. These statistics, as reported in Education Report (2020), highlight a shift in focus from mere access to child care to the specifics of timing and early enrollment. Supporting the relevance of the timing of child care, previous research has demonstrated that early enrollment in child care can significantly enhance a child's development (Drange and Havnes, 2019).

Part of the reason for the relatively low enrollment rates in early child care is the shortage of available child care slots, leading to widespread rationing. Importantly, this issue disproportionately affects parents with a migration background. Although the wish to enroll children in early child care is similar among both native and migrant parents, there is a notable disparity in actual enrollment rates. For instance, only 21% of children with a migration background are enrolled in early child care, compared to 33% of native children (Jessen et al., 2020). This indicates a significant gap in child care access between migrants and natives.

Child care in Germany falls under the purview of the child and youth welfare system, with the federal government bearing responsibility. Nonetheless, the actual provision of child care is managed at the municipality level. A significant majority of child care centers, approximately 83%, are operated by municipalities, non-profit organizations, and associations. In contrast, private for-profit providers constitute a mere 3% of all child care facilities (see Education Report, 2020). The provision of child care services is primarily carried out by small centers, typically catering to 25-75 children (DJI, 2021). Competition between child care centers is generally low (Spiess, 2013).

The German child care market is characterized by a decentralized structure, with each municipality —- and often each center – having its own distinct enrollment process. As a result, the allocation process of child care slots is often criticized as very complicated, non-transparent, and inefficient. Families often face divergent experiences: while some wait years to secure a slot, despite their legal entitlement, others receive multiple offers, inadvertently blocking access for others and prolonging waiting times. The absence of mandatory, standardized criteria for slot allocation and a lack of a centralized system to monitor enrollment decisions exacerbate the difficulties in navigating the application process. The decentralized nature of child care admissions creates conditions that are potentially conducive to high inequality and discrimination (Hermes et al., 2021, 2023a,b).

3. Data and Experimental Design

3.1. The Survey

We implemented our experiment in the second wave of the *Inequality Barometer* of the University of Konstanz. The online survey was conducted in late November 2022, and aimed to capture public perceptions of inequality. The survey was conducted by the survey company Verian (previously known as Kantar Public), and consisted of seven modules, with our experiment being the fourth. The sample includes 4,822 respondents drawn using quotas to represent the German voting-age population (18 years and older) in terms of gender, age, state of residence, and education background. In addition, the survey company provides survey weights to adjust for minor deviations of the sample from the general population, which we applied in all empirical analyses.

The survey was conducted using Verian's Payback Online Panel, a non-probability sample of approximately 150,000 active members. Recruitment takes place through the Payback loyalty program, one of the largest consumer bonus programs in Germany with around 25 million consumers, representing roughly half of all German households. While initial recruitment into the panel may involve non-probability elements, the sampling process from the panel aligns with principles of probability sampling, within the constraints of an online access panel. Overall, about 40% of invited panel members completed the survey, around 5% dropped out, and another 5% were screened out based on quotas, indicating a take-up rate (clicking on the invitation) of about 50%. Median completion time for the full survey was 20 minutes. As an incentive to participate, respondents received 2 EUR in the form of credit points for a voucher system. Further information about the survey and the sampling procedure, as well as screenshots of the main questions, can be found in Appendix C.

The objective of this study is to evaluate existing beliefs about migrant-native gaps in early child care, and investigate the impact of providing information about these inequalities on public preferences for equity-enhancing reforms. In our experiment, we randomly assign respondents to different experimental groups which receive different pieces of information about inequality in early child care before stating their reform support.

The survey module begins by eliciting respondents' initial beliefs regarding migrants' enrollment rate, and child care centers' response rate to email inquiries from migrant parents. Respondents state these beliefs only for migrant parents, while we provide the correct rates for natives as reference points. We focus on Turkish migrants, who represent the largest and geographically most dispersed migrant group in Germany. In 2019, there were approximately 1.5 million people of Turkish origin in Germany, accounting for approximately 1.3% of the German population and 13% of all migrants in Germany (Bundesamt für Migration, 2019). Furthermore, Turkish migrants are severely underrepresented in early child care, as their enrollment rate is 21 percentage points below the rate of natives, while demand for child care is very similar in both groups (Jessen et al., 2020).

In particular, we ask respondents to answer the following questions: i) "Please give your assessment of Turkish parents. How many out of 100 children of Turkish parents attend a daycare center (for children under the age of 3)? For your orientation, we provide you with the figures for German parents. According to a scientific study, 33 out of 100 children of German parents attend a daycare center." ii) "Please give your assessment of Turkish parents. How many Turkish parents who send an e-mail inquiry to a daycare center receive a reply? According to a scientific study, 71 out of 100 German parents receive a reply to an email inquiry from a child care center. ..." Respondents answer using a slider to indicate a numerical value ranging from zero to 100 (see Appendix C.2 for screenshots).

Note that our belief-elicitation questions anchored respondents' beliefs about natives' enrollment and email response rates when eliciting beliefs about migrants. Such anchoring is commonly employed to reduce measurement errors in elicited beliefs (Roth et al., 2022; Lergetporer and Woessmann, 2023). In our study, the anchors are crucial for clarifying the interpretation of possible information treatment effects; without an anchor, any observed treatment effects could potentially arise from updating beliefs about either natives' or migrants' rates. The anchors allow us to attribute any information effects specifically to updates regarding migrant figures (or the migrant-native gap).

3.2. Experimental Design

After eliciting prior beliefs, respondents are randomly assigned to one of three treatment groups, or the control group. Each treatment group receives a specific piece of information, including a graphical representation, as shown in Appendix C.3. We provide the following treatment information:

(i) "T1: Enrollment rate information": Respondents receive information comparing the enrollment rate of children from German parents (33/100) with that of children from Turkish parents (12/100) in early child care (Jessen et al., 2020).

(ii) "T2: Response rate information": Respondents receive information about the response rate of child care centers to inquiries from German parents (71/100) compared to inquiries from Turkish parents (63/100) (Hermes et al., 2023a).

(iii) "T3: Enrollment & response rate information": Respondents receive both sets of information, i.e., the combination of T1 and T2.

In all treatments we display the source of the information with a citation.[6] We also include a mouse-over text box offering a concise summary of the studies, accessible when respondents hover over an information icon, to assure respondents that the provided information is evidence-based. Note that our treatment design adheres to best practices

[6] We also conducted another treatment which informed respondents about German and Turkish parents' enrollment wish. However, we have excluded this treatment from our main analysis due to its interpretational ambiguity. For details, see Appendix E.

in designing information provision experiments (Haaland et al., 2023), such as using official statistics and enhancing verbal statements with graphical depictions of the information.

We then measure respondents' support for four equity-enhancing policy reforms on a five-point Likert scale ranging from "I fully disagree" to " I fully agree." The four policy reforms are i) introducing centralized child care admission processes at the municipal level, ii) providing additional child care slots, iii) implementing preferential treatment of migrant families in the enrollment process, and iv) offering child care centers additional incentives to admit migrant children (see Appendix Figure C5 for the original presentation and wording). Based on the existing body of literature, we selected policy interventions deemed likely to mitigate the migrant-native disparities in early child care access. The initial two (non-targeted) proposals aim to alleviate slot shortages, either through the direct provision of additional slots or by enhancing efficiency in slot distribution via centralization, which is regarded as a key strategy for fostering equitable access to child care (Jessen et al., 2020).[7] However, emerging evidence indicates that non-targeted policies alone are insufficient to completely bridge the gap for migrant families, who may struggle with the child care application process (Hermes et al., 2021) or face discrimination from child care center administrators (Hermes et al., 2023a). The two policies specifically aimed at migrants directly address these barriers to equitable child care access.

To reduce potential biases from imperfect memory of the treatment information, we present a reminder to treated respondents in a text box on the screen where they indicate their policy reform preferences (see Appendix Figure C6 for an example).[8]

Furthermore, we elicit respondents' perceptions about the reasons behind migrants' disadvantages in the early child care market. To do so, respondents are given the option to select multiple reasons from the following list: Unequal treatment due to i) the migrants' cultural background, ii) the additional effort required from child care centers to cater to migrants, and iii) the preferences of other parents. Additionally, respondents could select the options "other reasons", "don't know", and "not specified" (see Appendix Figure C7). We use these perceptions as an indirect manipulation check to assess whether the treatment influences respondents' perceptions. Although a more direct test would

[7] The (decentralized) process of allocating child care slots frequently faces criticism for inefficiency. Some families endure years on waiting lists before securing a slot (Carlsson and Thomsen, 2015), while others receive multiple offers, thereby blocking access and prolonging wait times for other families (Fugger et al., 2017). A centralized admission system could reduce these inefficiencies.

[8] Like many survey experiments in economics, our main outcomes of interest are survey-based stated policy preferences. These outcomes are sometimes criticized for lacking immediate economic or political consequences. In Section 6, we discuss several pieces of evidence highlighting the relevance of stated policy preferences for real-world political processes.

involve repeating the prior-belief questions post-treatment, we avoided this to prevent confusion among participants, particularly in the control group, which could result in biased response behavior (see, e.g., Grewenig et al., 2020).

3.3. Econometric Model

We estimate the treatment effects using OLS regressions of the specific outcome of interest on randomized treatment indicators. Our main specification is the following:

$$Y_i = \alpha_1 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \mathbf{X_i'}\mu + \epsilon_i \tag{1}$$

We define Y_i as the outcome of interest, e.g., reform support, for survey respondent i. To facilitate the interpretation of treatment effects on the overall support for policy reforms, we construct an index following Kling et al. (2007). In particular, we first z-standardize the support for each policy in the control group. Then, we calculate the mean of the four standardized policy support measures for each respondent, and z-standardize it again. The resulting composite measure captures overall treatment effects on reform support across multiple categories.

$T1_i$, $T2_i$, and $T3_i$ are binary indicators that take a value of one if respondent i received treatment 1 ("T1: Enrollment rate informaton"), treatment 2 ("T2: Response rate information"), or treatment 3 ("T3: Enrollment & response rate information"), and zero otherwise. Additionally, we construct an indicator variable called "Treatments (T1 | T2 | T3)" that takes a value of one if the respondent is assigned to any of the three treatment groups, and zero otherwise. This indicator allows us to examine the overall effect of being exposed to any information on a given outcome.

Due to the randomized experimental design, the causal effect of information provision on the respective outcomes can be calculated from raw differences between treatment and control groups. However, we include a vector of control variables X_i for precision and to account for potential small imbalances across experimental groups. These controls comprise gender, age (respondent is 18 to 39 years, 40 to 59 years, or at least 60 years old), education (respondent's highest degree is secondary, upper secondary, or post-secondary education), and wealth status (respondent owns real estate or not). ϵ_i is the idiosyncratic error term. We employ survey weights provided by the survey company throughout to align the drawn sample to known population counts.

We investigate potential treatment effect heterogeneities using the following model:

$$
\begin{aligned}
Y_i = \alpha_2 \quad &+ \quad \gamma_1 Treatments\ (T1|T2|T3)_i \\
&+ \quad \gamma_2 Treatments\ (T1|T2|T3)_i \times Subgroup_i \\
&+ \quad \gamma_3 Subgroup_i + \mathbf{X_i'}\delta + \nu_i
\end{aligned} \tag{2}
$$

$Treatments\ (T1|T2|T3)_i$ is an indicator that takes a value of one if respondent i is assigned to any of the three treatment groups, and zero otherwise. $Subgroup_i$ is an indicator that takes a value one if respondent i is part of a specific subgroup, and zero otherwise.

Since we expect the information treatment to operate through updating respondents' prior beliefs about migrant-native gaps in early child care, we are particularly interested in exploring heterogeneous treatment effects based on these prior beliefs. In our preferred specification, we divide individuals into two distinct groups. The first group ("underestimator" of inequalities) consists of individuals who consistently hold higher beliefs about migrants' enrollment and response rates compared to the actual rates (12 out of 100 and 63 out of 100, respectively). The second group ("overestimator") comprises all other individuals. Doing so, we examine whether treatment effects differ between individuals who under- or overestimate migrant-native gaps in early child care relative to the information presented in the treatments. We also explore treatment effect heterogeneities by sociodemographic subgroups.

3.4. Balancing

Table 1 presents the means and standard deviations of various respondent characteristics in the treatment groups compared to the control group. Overall, demographic characteristics are well balanced across the experimental groups. Three out of 39 pairwise comparisons turn out statistically significant at the 10%-level, which we would expect by pure chance. None of the differences is significant at the 5%-level.

In Appendix Table B1, we further assess the random assignment of treatments. We regress the treatment indicators on a set of control variables, including a dummy for item non-response and respondents' prior beliefs. The resulting F-statistics reject the joint significance of the explanatory variables (F = .46, F = 1.27, F = .87, F = .83, and F = .46 for Columns (1) to (5), respectively). This finding provides additional support for the conclusion that random assignment was successful.

Table 1: Descriptive Statistics and Balancing

	(1) Control			(2) T1: Enrollment rate information			(3) T2: Response rate information			(4) T3: Enrollment & response rate information		
Variable	N	Mean	SD	N	Diff.	P-value	N	Diff.	P-value	N	Diff.	P-value
Female	1223	0.513	(0.503)	1174	-0.008	0.683	1223	0.032	0.117	1202	0.001	0.975
Migrant	1211	0.159	(0.366)	1167	0.009	0.572	1206	-0.014	0.329	1192	-0.004	0.779
Parent	1223	0.418	(0.493)	1174	0.016	0.436	1223	0.017	0.391	1202	-0.013	0.526
Property owner	1171	0.454	(0.498)	1136	-0.023	0.267	1178	-0.021	0.297	1166	-0.008	0.685
Right-wing voter	1223	0.119	(0.324)	1174	0.009	0.549	1223	0.017	0.247	1202	-0.006	0.650
Age												
18 - 39 years	1223	0.298	(0.458)	1174	-0.000	0.986	1223	0.006	0.758	1202	0.011	0.555
40 - 59 years	1223	0.343	(0.475)	1174	0.012	0.546	1223	0.018	0.352	1202	-0.005	0.803
At least 60 years	1223	0.358	(0.480)	1174	-0.011	0.557	1223	-0.024	0.218	1202	-0.006	0.749
Education												
Lower education	1223	0.327	(0.469)	1174	-0.022	0.244	1223	-0.021	0.259	1202	-0.003	0.891
Medium education	1223	0.298	(0.457)	1174	0.018	0.329	1223	0.036*	0.056	1202	0.032*	0.091
Higher education	1223	0.375	(0.484)	1174	0.004	0.850	1223	-0.015	0.451	1202	-0.029	0.134
Prior beliefs												
Prior enrollment rate	1157	35.399	(25.900)	1111	1.385	0.208	1155	1.982*	0.070	1147	1.264	0.250
Prior response rate	1143	52.773	(29.104)	1095	-0.941	0.443	1142	0.263	0.828	1140	-0.676	0.579

Notes: Table shows means and standard deviations of variables for the control group. *Diff* reports the difference in means of the respective variable between the control group and each of the three treatment groups. We indicate the results of a two-sided t-tests between the control mean and the mean of each respective treatment group with significance stars. *Female:* Categorical variable taking a value of one if the respondent states to be female, two if the respondent states to be diverse, zero otherwise (the diverse category in the gender variable ($n = 10$) is not shown). *Migrant:* Indicator variable taking a value of one if the respondent has a migration background (she or either of her parents were born outside of Germany), zero otherwise. *Parent:* Indicator variable taking a value of one if the respondent is a parent (has at least one child under the age of 18 in the household), zero otherwise. *Property owner:* Indicator variable taking a value one if the respondent owns a house, zero otherwise. *Right-wing voter:* Indicator variable taking a value of one if the respondent stated to vote for a right-wing party (AfD, NPD, Dritter Weg, or Die Rechte), zero otherwise. *Age* "18-39 years" respondent is between 18 and 39 years old; "40-59 years" respondent is between 40 and 59 years old; "At least 60 years" respondent is 60 years and older. *Education:* "Higher education:" college entrance qualification, "Abitur"; "Medium education:" middle-tier secondary education ("Realschulabschluss"); "Lower education':' drop out, still in school, lowest-tier secondary education ("Hauptschulabschluss"). *Prior beliefs:* Respondents' estimation how likely migrants enroll their child into child care and receive a response to child-care-related email inquiries, respectively (in percent). See Appendix D for detailed variable descriptions. Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$.

4. Results on Prior Beliefs and Average Treatment Effects

4.1. Descriptive Findings

First, we present descriptive results regarding respondents' prior beliefs. On average, respondents believe that 35.4 out of 100 migrant children are enrolled in early child care (see Figure 1a). This value significantly overestimates the actual enrollment rate of just 12 out of 100 migrants. Interestingly, respondents believe that migrants' enrollment rate is slightly higher than the one of natives (33 out of 100; see anchor in Figure 1a), while in reality migrants are strongly underrepresented (see Jessen et al., 2020).

Turning to beliefs about child care centers' responses to parental email inquiries, respondents believe that migrant parents receive responses to 52.4 out of 100 emails inquiries. However, the actual value is higher at 63 out of 100 for migrants, and 71 out of 100 for natives. This indicates that respondents overestimate the extent of discrimination that migrant parents face from child care center managers when it comes to email responses.

Importantly, the cumulative distribution functions in Figure 1b reveal that beliefs vary substantially across respondents. For example, the 10–90 percentile range for beliefs about migrants' child care enrollment rate spans from 10% to 79%. Beliefs about the response rate are even more dispersed, with a 10–90 percentile range of 11% to 98%. Put differently, the documented misperceptions about migrant-native gaps in early child care are relatively large compared to misperceptions in other domains found in other studies, lying in the 60–80th percentile (see literature review by Bursztyn and Yang, 2022). Thus, respondents seem to have relatively imprecise knowledge about the true extent of migrant-native inequalities in the early child care market.

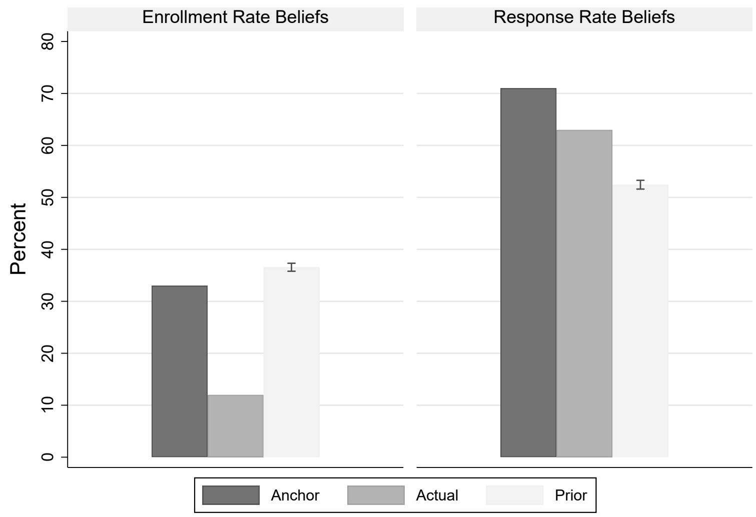
Next, we study how prior beliefs vary across respondents' sociodemographic characteristics. Specifically, we compare (i) females to males; (ii) migrants to natives; (iii) parents to non-parents; (iv) older to younger respondents; (v) those with higher educational degrees to those with lower degrees; (vi) right-wing voters to respondents with other political preferences; and (vii) property owners to non-owners (as a proxy for wealth). Figure 2 depicts the respective subgroup coefficients when regressing enrollment rate or response rate beliefs (or both) on the subgroup indicators; the respective regression results are provided in Table 2. Specifically, the outcome variables are indicators of underestimating migrant-native gaps in the child care market with respect to enrollment rate (left panel), response rate (middle panel), or both enrollment rate and response rate (right panel). Recall that underestimating migrant-native gaps is equivalent to overestimating the values for migrants.

We find that females are 3.4 percentage points more likely to overestimate the enrollment rate of migrant children compared to males ($p = .013$).[9] While males already overestimate the enrollment rate of migrants (33.7% versus the actual value of 12%), females' enrollment beliefs are even more biased. Interestingly, migrants do not hold significantly different priors compared to natives. Parents' beliefs about the enrollment rate do not differ from those of non-parents, but parents are 4 percentage points more likely to overestimate the response rate from child care centers to migrants ($p = .006$).

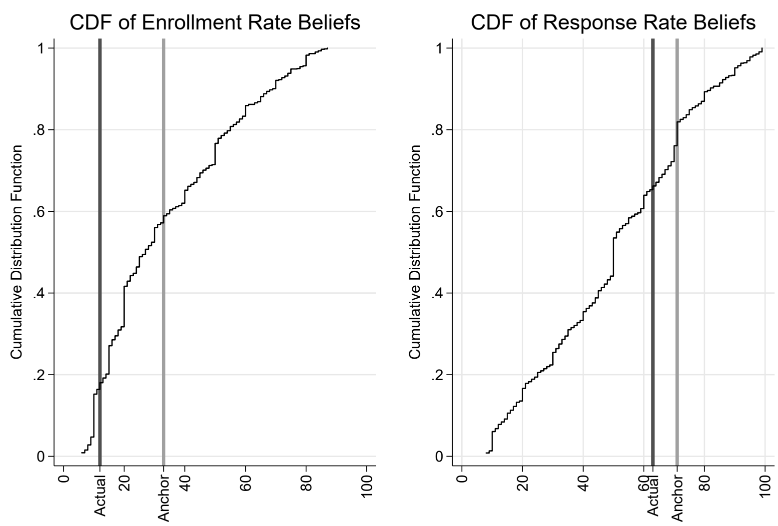
Compared to younger respondents, those aged between 40 and 59 years and those aged 60 years and older are substantially more likely to overestimate the response rate to migrant parents (by 13.2 percentage points and 7.6 percentage points, respectively; $p < .001$ for both age groups). Furthermore, the degree of overestimating migrants' child care enrollment decreases in the education level. Both medium-educated respondents (by

[9]Note that coefficients and p-values refer to the multivariate specifications.

Figure 1: Prior Beliefs about Early Child Care for Migrants



(a) Average Prior Beliefs



(b) Cumulative Distribution Functions of Prior Beliefs

Notes: Subfigure (a) shows the mean answers of respondents to the prior belief elicitation questions for the enrollment rate of migrant children and the response rate to inquiries by migrant parents. Error bars indicate 95% confidence intervals. Subfigure (b) shows the corresponding cumulative distribution functions (CDFs). Both graphs also depict the actual value for migrants (*Actual*) as well as the values for natives that we provided to the respondents (*Anchor*).

4.2 percentage points ($p = .013$)) and higher-educated respondents (by 9.4 percentage points ($p < .001$)) are significantly less likely to overestimate enrollment rates of migrants than those with the lowest education level. Right-wing voters are 13.2 percentage points more likely to overestimate the response rate to inquiries by migrant parents ($p < .001$), and 10.1 percentage points more likely to exhibit overestimation of the combined belief measure

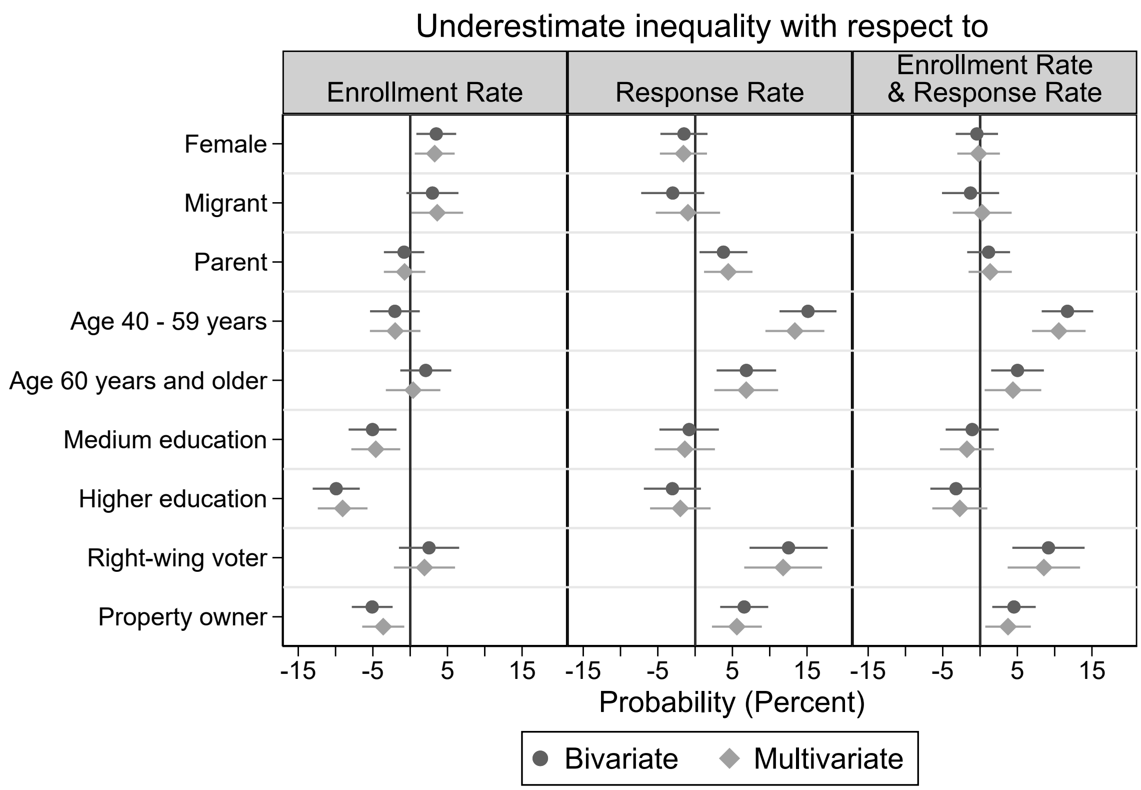
($p. < .001$). Finally, property owners are less likely to overestimate the enrollment rate of migrants ($p = .015$) compared to non-owners, but more likely to overestimate the response rate to migrants ($p = .009$). The combined measure of enrollment rate and response rate beliefs generally yields similar results as the measure of response rate beliefs alone.

In summary, our analysis reveals significant correlations between sociodemographic characteristics and prior beliefs about migrant-native gaps in early child care. Notably, respondents who are younger, more educated, and do not have right-wing voting preferences, respectively, are less likely to underestimate inequalities in the child care market.[10] This correlation pattern is also reflected in respondents' support for policy reforms: younger and more educated respondents show considerably higher support for reforms, whereas support is lower among right-wing voters (see Appendix Figure A1 and Appendix Table B2).[11]

[10] The fact that prior beliefs vary meaningfully with respondents' characteristics (e.g., more educated respondents tend to have more accurate beliefs) suggests that the elicited priors are not mere random guesses.

[11] A concern raised by a reviewer is that some participants might perceive the child care system itself as problematic and would therefore not support reforms — i.e., not because of the reforms themselves but because of a system viewed as flawed. However, our data suggest that this group — if it exists — is relatively small. Specifically, there are only 84 participants (1.7%) who exhibit the lowest level of support for all four proposed policies. When broadening the definition to include participants who consistently express either the lowest or second lowest level of support, or provide no response to the support questions for each of the four policies, we still identify only 257 participants (5.3%) in our dataset. Thus, our data do not support the idea that this phenomenon is of significant importance.

Figure 2: Correlation of Demographics with Prior Beliefs about Inequalities



Notes: Figure shows marginal effects from probit estimations indicating the change in the likelihood to underestimate inequality in relation to the omitted baseline category from regression models with or without control variables. *Female:* Categorical variable taking a value of one if the respondent states to be female, two if the respondent states to be diverse, and zero if the respondent states to be male (omitted); the diverse category ($n = 10$) in the gender variable is not shown. *Migrant:* Indicator variable taking a value of one if the respondent has a migration background (she or either of her parents were born outside of Germany), zero otherwise (omitted). *Parent:* Indicator variable taking a value of one if the respondent is a parent (has at least one child under the age of 18 in the household), zero otherwise (omitted). *Age:* Categorical variable taking a value of two if the respondent is 60 years and older, a value of one if the respondent is between 40 and 59 years old, and a value of zero if the respondent is between 18 and 39 years old (omitted). *Education:* Categorical variable taking a value of two if the respondent has completed "Higher education" (college entrance qualification, "Abitur"), a value of one if the respondent has completed "Medium education" (middle-tier secondary education ("Realschulabschluss")), and a value of zero if the respondent has completed lower education (drop out, still in school, or lower-tier secondary education ("Hauptschulabschluss")) (omitted). *Right-wing voter:* Indicator variable taking a value of one if the respondent stated to vote for a right-wing party (AfD, NPD, Dritter Weg, or Die Rechte), zero otherwise (omitted). *Property owner:* Indicator variable taking a value one if the respondent owns a house, zero otherwise (omitted). Error bars indicate 95% confidence intervals. See Table 2 for coefficients in the multivariate regression model.

Table 2: Correlation between Demographics and Underestimation of Inequality

	Underestimate Inequality with respect to ...		
	Enrollment rate beliefs (1)	Response rate beliefs (2)	Enrollment & response rate beliefs (3)
Female	0.034**	-0.018	-0.004
	(0.014)	(0.016)	(0.014)
Migrant	0.033*	-0.013	0.000
	(0.018)	(0.022)	(0.020)
Parent	-0.008	0.045***	0.015
	(0.014)	(0.016)	(0.015)
40 - 59 years	-0.024	0.132***	0.104***
	(0.017)	(0.020)	(0.018)
At least 60 years	0.004	0.076***	0.050***
	(0.018)	(0.022)	(0.019)
Medium education	-0.042**	0.002	-0.003
	(0.017)	(0.021)	(0.019)
Higher education	-0.094***	-0.013	-0.021
	(0.017)	(0.021)	(0.019)
Right-wing voter	0.024	0.132***	0.101***
	(0.021)	(0.027)	(0.025)
Property owner	-0.035**	0.045***	0.030*
	(0.014)	(0.017)	(0.016)
Pre-specified Controls	Yes	Yes	Yes
N	4,570	4,520	4,822

Notes: Table shows probit estimation parameters on the margin for regressions of individual characteristics on a binary indicator that takes a value of one if the respondent underestimates inequality. Results are based on multivariate probit regressions and calculated on the margin. Outcome variables are defined as follows: Column (1): Respondent underestimates inequality with regard to enrollment rates; Column (2): Respondent underestimates inequality with regard to response rates; Column (3): Respondent underestimates inequality with regard to both, zero otherwise. *Female:* Categorical variable taking a value of one if the respondent states to be female, two if the respondent states to be diverse, and zero if the respondent states to be male (omitted); the diverse category ($n = 10$) in the gender variable is not shown. *Migrant:* Indicator variable taking a value of one if the respondent has a migration background (she or either of her parents were born outside of Germany), zero otherwise (omitted). *Parent:* Indicator variable taking a value of one if the respondent is a parent (has at least one child under the age of 18 in the household), zero otherwise (omitted). *Age:* Categorical variable taking a value of two if the respondent is 60 years and older, a value of one if the respondent is between 40 and 59 years old, and a value of zero if the respondent is between 18 and 39 years old (omitted). *Education:* Categorical variable taking a value of two if the respondent has completed "Higher education" (college entrance qualification, "Abitur"), a value of one if the respondent has completed "Medium education" (middle-tier secondary education ("Realschulabschluss")), and a value of zero if the respondent has completed lower education (drop out, still in school, or lower-tier secondary education ("Hauptschulabschluss")) (omitted). *Right-wing voter:* Indicator variable taking a value of one if the respondent stated to vote for a right-wing party (AfD, NPD, Dritter Weg, or Die Rechte), zero otherwise (omitted). *Property owner:* Indicator variable taking a value one if the respondent owns a house, zero otherwise (omitted). We use survey weights to affirm national representativeness. See Appendix D for detailed variable descriptions. Robust standard errors in parentheses. Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$.

4.2. Average Treatment Effects on Reform Support

To set the stage for analyzing information treatment effects on reform support, we first document reform support in the control group (see Table 3). Increasing the number of slots for early child care is the most popular policy reform, receiving an average support rating of 3.94 out of 5 (70% of control-group respondents "fully" or "somewhat" support this policy reform). Implementing a centralized admission system is the second most popular reform (support rating: 3.09; 40% support), followed by providing additional financial incentives for child care centers to admit migrant children (support rating: 2.57; 25% support). The least popular policy reform is granting preferential treatment of migrant children during the admission process (support rating: 1.86; 7% support).

Turning to the causal effect of providing information about migrant-native gaps on policy support, we find precisely estimated zero effects when combining all treatments (see Panel A of Table 3). Panel B confirms this finding when considering the different information treatments separately.

Although our treatments do not alter average reform support, they still significantly influence respondents' perceptions, as detailed in Appendix Table B3. Specifically, respondents who received one of the treatments are 3.8 percentage points more likely to attribute migrant-native disparities in early child care to migrants' cultural background (Column (1) of Panel A; $p = .017$), an increase of 13.8% relative to the control group mean. Thus, the absence of treatment effects on reform support does not imply that respondents disregard the information provided.

A likely reason for the lack of average treatment effects on reform support, despite treatment-induced shifts in perceptions about migrant-native gaps, is heterogeneity of treatment effects based on respondents' prior beliefs. Indeed, the direction in which the information treatments update beliefs should determine treatment effects on policy support (see Haaland et al., 2023). Put differently, if respondents believe that inequality is not an issue but then learn that large inequalities exist, they should increase their support for equity-enhancing policies (and vice versa). In the following section, we explore whether the overall null effect on policy support masks counterbalancing effects based on respondents' prior beliefs.

Table 3: Treatment Effects on Reform Support

	Centralized Admission	Additional Slots	Preferential Treatment	Financial Incentives	Reform Index
	(1)	(2)	(3)	(4)	(5)
Panel A: Treatments combined					
Treatments (T1 | T2 | T3)	0.003	-0.022	0.044	-0.020	0.007
	(0.049)	(0.039)	(0.038)	(0.048)	(0.036)
Scaled treatment effect	0.11	-0.56	2.34	-0.76	-
Control Mean	3.09	3.94	1.86	2.57	-0.02
Panel B: Treatments separately					
T1: Enrollment rate information	0.048	-0.003	0.081*	0.026	0.053
	(0.060)	(0.049)	(0.047)	(0.059)	(0.044)
T2: Response rate information	-0.032	-0.043	0.005	-0.082	-0.043
	(0.059)	(0.049)	(0.048)	(0.060)	(0.046)
T3: Enrollment & response rate information	-0.006	-0.020	0.046	-0.001	0.012
	(0.059)	(0.050)	(0.046)	(0.058)	(0.044)
Pre-specified Controls	Yes	Yes	Yes	Yes	Yes
N	4,634	4,713	4,739	4,714	4,767

Notes: Table shows treatment effects on an indicator for how much the respondent agreed with a given policy reform on a five-point Likert scale. Results are based on multivariate OLS regressions. Outcome variables are defined as follows: Column (1): Answer on a five-point Likert scale to the statement "Instead of individual child care center managers, a central office at the community level should decide which child gets a child care slot."; Column (2): Answer on a five-point Likert scale to the statement "The number of child care slots should be further expanded using taxpayers' money."; Column (3): Answer on a five-point Likert scale to the statement "Families with a migration background should be given preference in the allocation of child care slots."; Columns (4): Answer on a five-point Likert scale to the statement "Child care centers should receive more support from taxpayers to accommodate children with an immigrant background."; Column (5): An index combining support for all reforms. *T1: Enrollment information*, *T2: Response rate information*, and *T3: Enrollment & response rate information* are indicator variables taking a value of one if the respondent is in the respective treatment group, zero otherwise. *Treatments (T1 | T2 | T3)* is an indicator variable taking a value of one if the respondent is in any of the three treatment groups, zero otherwise. Panel A shows treatment effects for all treatment groups pooled. Panel B shows treatment effects separately for each treatment group. *Reform Index* is defined as follows: Applying the procedure of Kling et al. (2007), we first z-standardize the four stated policy reform support variables (*Centralized admission*, *Increase slots*, *Preferential treatment*, and *Financial incentives*) by subtracting the control-group mean and dividing by the control-group standard deviation. Then, we calculate an equally weighted average of the standardized variables and, finally, z-standardize this average. An index is computed for all respondents who have a valid response to at least one reform variable. *Pre-specified Controls* include gender in three categories (female, male, diverse), age in three categories (between 18 and 39 years old, between 40 and 59 years old, at least 60 years old), education in three categories (completed lower, medium, or higher education), and wealth in two categories (respondent owns property or not). We use survey weights to ensure national representativeness. See Appendix D for detailed variable descriptions. Robust standard errors in parentheses. Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$.

5. Heterogeneous Treatment Effects

In this section, we study heterogeneities of treatment effects on policy support in three sets of analyses. First, and most importantly, we investigate heterogeneities by prior beliefs, which will reveal whether respondents systematically differ in how they react to new information given what they already know (or believe to be true) about the child care market. Second, we present an exploratory analysis of heterogeneities along different socioeconomic dimensions. Third, we present a data-driven Causal Forest analysis that identifies the primary drivers of treatment effect heterogeneities in our experimental data.

5.1. Heterogeneity by Prior Beliefs

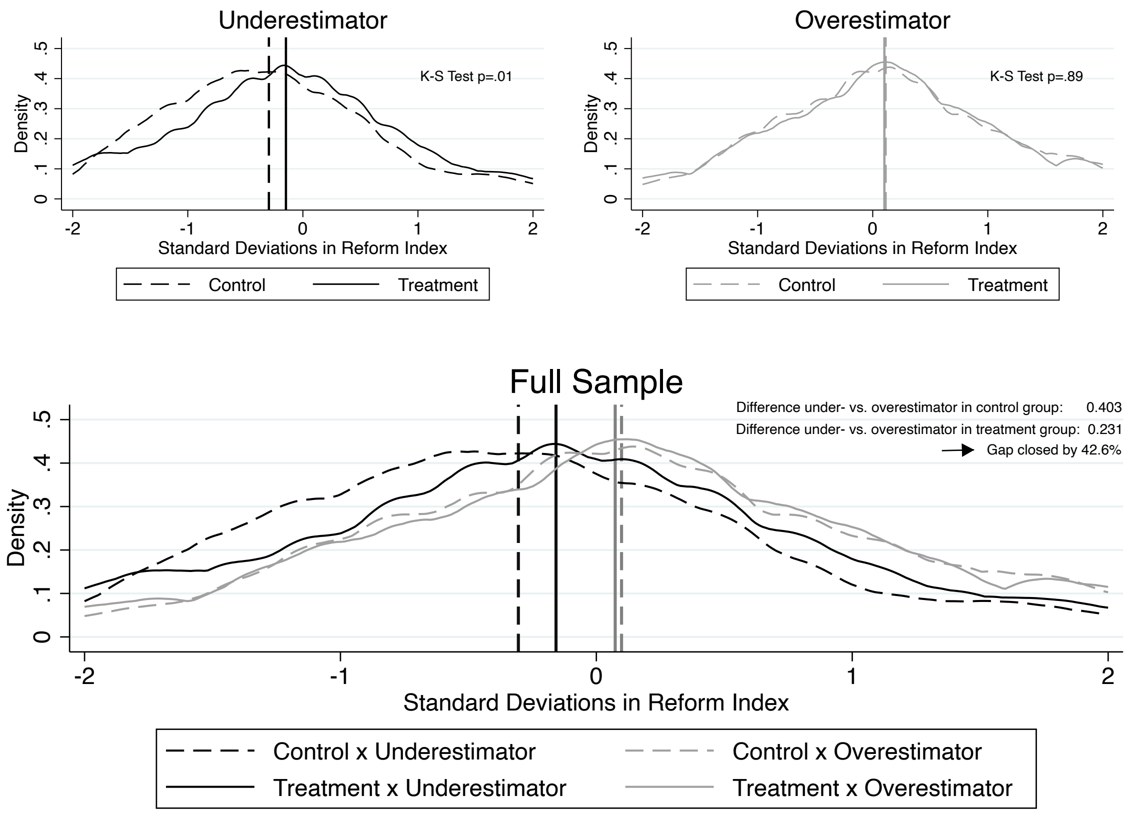
Figure 3 compares treatment effects on the policy support index for respondents who initially underestimated or overestimated actual migrant-native inequalities in child care. We perform the analysis on raw data (i.e., without any controls). In total, 29.0% of our sample are classified as underestimators, with the shares very similar between the control group (28.3%) and the treatment groups (29.2%). In line with expected belief updating, the treatment significantly shifts the distribution of the policy support index upward among underestimators. This can be seen by comparing the solid and dashed red lines in the upper left panel (Kolmogorov-Smirnov (K-S) test: $p = .010$). In the upper right panel, we find the opposite qualitative pattern for overestimators, though this effect is not statistically significant (comparing the solid and dashed blue lines; K-S test: $p = .887$). By combining under- and overestimators, the lower panel shows that information provision substantially reduces the polarization in reform support between both subgroups by 42.6% relative to the control group.[12] This treatment effect heterogeneity by prior beliefs explains the average null effect in the overall sample.

We confirm these graphical results in our regressions analysis accounting for the pre-specified control variables (see Table 4).[13] As shown in Column (1), respondents who underestimate inequalities in the child care markets are significantly less likely to support the equity-enhancing reform proposals. However, when receiving the treatment, under-

[12] To compute the reduction in polarization, we first compare the average policy support for underestimators and overestimators in both the control and treatment groups. In the control group, the average policy support was -0.3048 for underestimators and 0.0984 for overestimators, resulting in a difference of 0.4032. In the treatment group, the average policy support was -0.1577 for underestimators and 0.0737 for overestimators, resulting in a difference of 0.2314. We then calculate the reduction in the gap as $(0.4032 - 0.2314) = 0.1718$, which translates to a percentage gap closure of $0.1718/0.4032 * 100 = 42.6\%$.

[13] Appendix Table B4 provides the results for each individual policy, both for the treatments combined and broken down separately by each treatment.

Figure 3: Distribution of Reform Index for Treatment and Control Groups by Prior Beliefs



Notes: Figure shows the unconditional distribution of the reform index for treatment and control groups by prior beliefs about inequalities in the child care market. Vertical lines report the means for treatment (solid) and control group (dashed). *Underestimator* is an indicator variable taking a value of one if the respondent answered both of the belief questions for migrants (*Prior enrollment rate*, *Prior response rate*) with values higher than the actual values (12 out of 100 and 63 out of 100, respectively), otherwise respondents are classified as *Overestimators.* In total, there are 1,223 individuals in the control group, with 877 overestimators and 346 underestimators. Across the three treatment groups, there are 3,599 individuals, with 2,549 overestimators and 1,050 underestimators. *Reform Index* is defined as follows: Applying the procedure of Kling et al. (2007), we first z-standardize the four stated policy reform support variables (*Centralized admission*, *Increase slots*, *Preferential treatment*, and *Financial incentives*) by subtracting the control-group mean and dividing by the control-group standard deviation. Then, we calculate an equally weighted average of the standardized variables and, finally, z-standardize this average. An index is computed for all respondents who have a valid response to at least one reform variable. We report p-values of Kolmogorov-Smirnov tests for the difference of distributions of the reform index between treatment and control groups separately for under- and overestimators in the upper two panels. In the lower panel, we report by how much the mean difference between under- and overestimators decreases in the treatment group as compared to the control group.

estimators exhibit a stronger increase in reform support than overestimators. Specifically, the treatment effect on reform support is 16.7% of a standard deviation ($p = .032$) higher for underestimators compared to overestimators. This pattern is also evident in Columns (2) and (4), which present treatment effects in the samples of underestimators and overestimators, respectively. While treatment effects are positive and statistically significant ($p = .041$) for underestimators (Column 2), they are negative, albeit insignificant ($p = .392$), for overestimators (Column 4).

Moreover, going beyond the combined treatment effect, Columns (3) and (5) of Table 4 present separate effects for the three information treatments. For underestimators, all treatment effects are positive, with the effect of information about the migrant-native enrollment gap (T1) being by far the largest (Column (3)). For overestimators, all three information treatments have negative effects, while none of them captures statistical significance (Columns (5)).[14]

Overall, these findings highlight the importance of considering respondents' prior beliefs when investigating the effects of information provision on reform support. We detect strong heterogeneous reactions to the information treatments based on respondents' priors such that information reduces polarization in respondents' preferences for equity-enhancing reforms in early child care.[15]

5.2. Heterogeneity by Demographics and Regional Characteristics

Next, we provide an exploratory analysis of heterogeneous treatment effects among sociodemographic subgroups and regional characteristics related to child care.

Demographics. First, we investigate whether treatment effects are different for females (compared to males) and parents (compared to non-parents), as these two subgroups are especially impacted by child care policies owing to their active involvement in child care activities. While females and males show no significant differences in policy reform support in the control group, females react significantly more positively to the information treatment (see Column (1) of Appendix Table B6). This finding aligns with the observation that females are more likely to underestimate inequalities for enrollment rates (see Figure 2). Parents generally show greater support for equity-enhancing policies than non-

[14] Results are qualitatively similar when we construct the reform index based on a principal component analysis (PCA) allowing for unequal weighting of the four policy support variables (see Appendix Table B5). If anything, results for the PCA-based reform index get slightly stronger, as the effect of the combined treatment (T3) in the subsample of underestimators becomes significant at the 10-percent-level.

[15] The reported heterogeneities by prior beliefs also indicate that the treatment effects on policy preferences operate through "information-based updating", as opposed to other possible channels like "salience-based updating" (Bleemer and Zafar, 2018). While the former means that effects are due to correcting biased beliefs, the latter simply means that the information treatment makes certain aspects—in our case, inequality in early child care—more salient. Heterogeneities by prior beliefs, which we find, are often interpreted as evidence for information-based updating (Lergetporer and Woessmann, 2023). This is also consistent with our experimental setup, as eliciting prior beliefs about migrant-native gaps among all respondents (including control group respondents) should mute potential salience-based effects.

Table 4: Treatment Effect Heterogeneity by Prior Beliefs

	Reform Index				
	Full Sample	Underestimator		Overestimator	
	(1)	(2)	(3)	(4)	(5)
Treatments (T1 | T2 | T3)	-0.039 (0.042)	0.136** (0.066)		-0.036 (0.042)	
× Underestimator	0.167** (0.078)				
Underestimator	-0.322*** (0.066)				
T1: Enrollment rate information			0.242*** (0.082)		-0.014 (0.052)
T2: Response rate information			0.053 (0.089)		-0.070 (0.053)
T3: Enrollment & response rate information			0.123 (0.080)		-0.025 (0.051)
Pre-specified Controls	Yes	Yes	Yes	Yes	Yes
N	4,767	1,392	1,392	3,375	3,375

Notes: Table shows treatment effects on the *Reform Index* by prior beliefs. Results are based on multivariate OLS regressions. *Underestimator* is an indicator variable taking a value of one if the respondent answered both of the belief questions for migrants (*Prior enrollment rate*, *Prior response rate*) with values higher than the actual values (12 out of 100 and 63 out of 100, respectively), otherwise respondents are classified as *Overestimators*. In Column (1) we run OLS regressions on the full sample. In Columns (2)–(5), we estimate treatment effects for the subsamples of *Underestimators* (Columns (2) and (3)) and *Overestimators* (Columns (4) and (5)) separately. *T1: Enrollment information*, *T2: Response rate information*, and *T3: Enrollment & response rate information* are indicator variables taking a value of one if the respondent is in the respective treatment group, zero otherwise. *Treatments (T1 | T2 | T3)* is an indicator variable taking a value of one if the respondent is in any of the three treatment groups, zero otherwise. *Reform Index* is defined as follows: Applying the procedure of Kling et al. (2007), we first z-standardize the four stated policy reform support variables (*Centralized admission*, *Increase slots*, *Preferential treatment*, and *Financial incentives*) by subtracting the control-group mean and dividing by the control-group standard deviation. Then, we calculate an equally weighted average of the standardized variables and, finally, z-standardize this average. An index is computed for all respondents who have a valid response to at least one reform variable. *Pre-specified Controls* include gender in three categories (female, male, diverse), age in three categories (between 18 and 39 years old, between 40 and 59 years old, at least 60 years old), education in three categories (completed lower, medium, or higher education), and wealth in two categories (respondent owns property or not). We use survey weights to ensure national representativeness. See Appendix D for detailed variable descriptions. Robust standard errors in parentheses. Significance levels: * $p <$ .10, ** $p <$ .05, *** $p <$ .01.

parents (see Column (2)). Notably, the information treatment leads to a significantly stronger reduction of parents' policy support.[16]

[16]Results hold for different subgroups of parents. For instance, parents of children under the age of ten also exhibit greater policy support compared to non-parents (coef. = .192, p = .032), which diminishes significantly after exposure to the treatment information (coef. = -.229, p = .032).

Second, we investigate heterogeneous treatment effects for respondents that report voting for right-wing parties. As one might expect, they exhibit less support for equity-enhancing policy reforms compared to other respondents (see Column (3) of Appendix Table B6, $p < .001$). Furthermore, in contrast to the predicted updating behavior that we observe in the general population, right-wing voters even *decrease* their policy support when given information about the actual migrant-native gaps in early child care ($p = .001$). Thus, information provision seems to reinforce, rather than mitigate, anti-migrant sentiments of these respondents. This phenomenon, wherein information exacerbates pre-existing biases, is common in situations where new information conflicts with personal beliefs (see Marino et al., 2023). In the political science literature, this is explained as an amplification of party identification based on the fact that challenging beliefs closely linked to individuals' political identity is psychologically taxing. Consequently, a typical response is to avoid this internal conflict by further aligning with the party's perspective (Campbell, 1980; Bartels, 2002).

Finally, we explored whether some respondents did not respond to the treatments because they did not believe the provided information. Although we did not directly ask respondents about their trust in the provided information, we utilized data on their trust levels towards institutions (i.e., Bundestag (German Parliament), judiciary, police, politicians, political parties, and the European Parliament), which the *Inequality Barometer* measured on a scale from 0 to 10. Regressing our reform index on the treatment indicator, respondents' trust in institutions (indexed using the standardization approach by Kling et al. (2007)), and their interaction, we find a positive and significant interaction term (see Column (4) of Appendix Table B6). At face value, this result suggests that distrust among some respondents may indeed attenuate treatment effects. However, we caution against over-interpreting this result, as our trust measure is only an indirect proxy for trust in the provided information.

Regional Characteristics. It also seems crucial to understand regional heterogeneities in both reform support and treatment effects, as both the availability and the institutional setting of child care varies substantially across regions. Each federal state in Germany has the authority to establish and manage its own overarching child care regulations and frameworks. Administrative responsibility, however, including the responsibility to set (income-based) fees prior to the age when child care becomes free of charge, lies with each municipality. We investigated whether general support for child care reforms and the observed treatment effects vary by regional characteristics, including (i) financial incentives for child care centers to enroll migrant children, (ii) whether child care is free for certain age ranges, (iii) urbanity level, and (iv) degree of child care slot rationing. The first two variables are measured at the federal state level ($k = 16$), while the latter two are measured at the county level ($k = 398$). While the state-level variables capture the "right" variation (as financial incentives and fee exemptions vary at the state level), we would have preferred to use information on urbanity and the degree of slot rationing at the municipality level. However, such finely grained data do not exist in Germany.

Results are presented in Appendix Table B7. First, as shown in the lower part of the table, support for child-care-related policy reforms in the control group shows no systematic variation with most regional characteristics. However, support is significantly higher in the 11 states where child care is free for certain age ranges compared to the five states that charge fees until age six. This correlation could be due to, among many other things, positive experiences with existing policies, economic relief, or a generally more supportive cultural and political environment that may enhance support for further reforms. Nonetheless, these comparisons should be interpreted cautiously due to the limited number of federal states and potential unobserved state-specific factors.

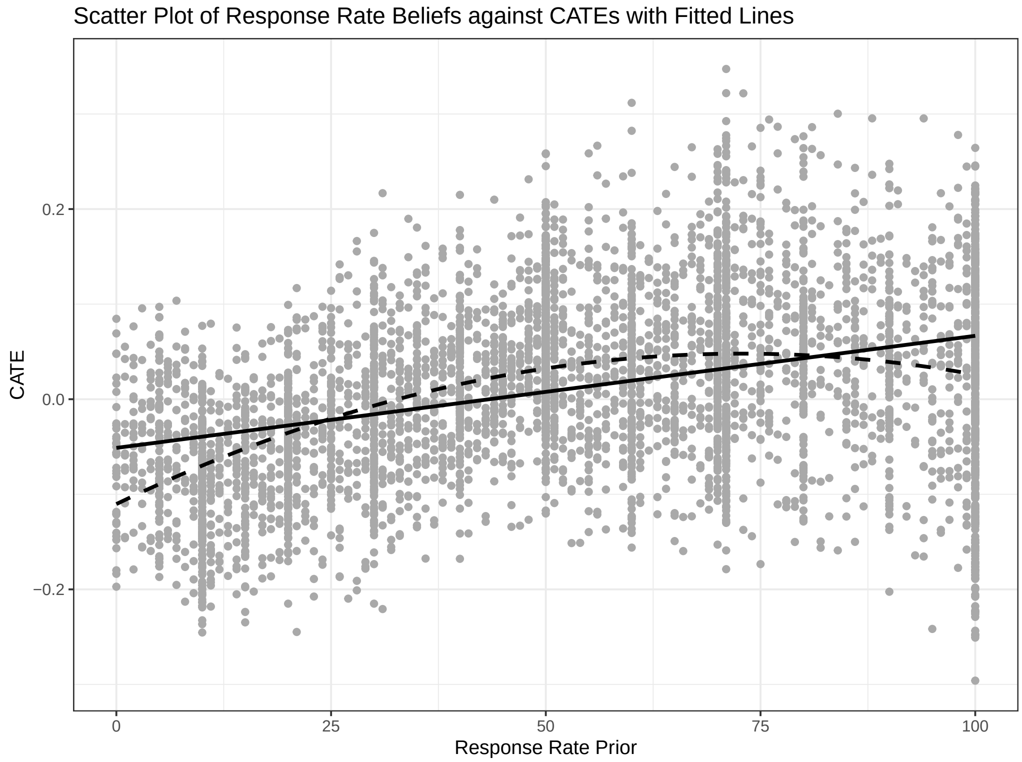
Second, there is no evidence for heterogeneity of our treatment effects by regional characteristics (see upper part of Appendix Table B7). Interactions of the treatment indicator with the regional variables are not only statistically insignificant, but also economically small. This consistency in the results suggests that the absence of significant treatment effects for Germany as a whole does not mask important heterogeneities at the subnational level.

5.3. Causal Forest Estimation

Finally, we conduct a Causal Forest analysis (Wager and Athey, 2018; Athey and Wager, 2019) for three main reasons. First and most importantly, the Causal Forest analysis is a data-driven approach that identifies the primary drivers of treatment effect heterogeneities in our sample. Second, the analysis provides valuable insights into the functional form of heterogeneity, revealing complex interactions and non-linearities. Third, the analysis addresses concerns about external validity by identifying subgroups with differing treatment effects, thereby supporting the generalizability of our findings.

Conceptually, the Causal Forest analysis divides the data into subsets along different covariates, and subsequently evaluates the treatment effect within each subset. The method calculates the Conditional Average Treatment Effects (CATEs) for each respondent by averaging these effects across numerous trees, accounting for confounding variables. It also determines the importance of different variables for driving treatment effect

Figure 4: Scatter Plot of CATEs and Response Rate Beliefs



Notes: Figure shows individual CATEs (y-axis) plotted against respondents' response rate belief percentiles (x-axis). CATEs are the result of a Causal Forest with 25,000 trees as described in Appendix F. The blue line is a fitted line minimizing mean squared errors. The red line is a quadratic fitted line minimizing mean squared errors. Vertical lines indicate the actual response rates to migrants and natives, taken from Hermes et al. (2023a).

heterogeneities by gauging their contribution to predictive accuracy (see Appendix F for details).

In our application, using all covariates collected in the survey, the Causal Forest highlights prior beliefs about migrants' enrollment and response rates as major drivers of treatment effect heterogeneity. In Figure 4, we illustrate the relationship between each respondent's belief about the response rate to migrant inquiries and their individual CATEs (see Appendix Figure F1 for the analogous plot for enrollment rate beliefs). The graph shows significant variation in treatment effects based on these prior beliefs. Respondents who perceive no migrant-native gap in the response rate or who believe migrants receive more responses than natives typically increase their reform support upon receiving treatment information. Conversely, those who perceive little discrimination (with response rate beliefs ranging from about 30 to 60) show minimal to no change in reform support. However, respondents who greatly underestimate the response rate (thus overestimating inequalities) tend to reduce their support for policy reforms after receiving the information. These trends are further supported by additional analyses, including quadratic OLS and quartile regressions, as detailed in Appendix Table B8.

In sum, the Causal Forest analysis highlights prior beliefs about migrant-native inequalities as main drivers of treatment effect heterogeneity. This finding is reassuring, as it echoes our conceptual considerations which led us to focus our main heterogeneity analysis on these beliefs (see Section 5.1).

6. Discussion of Possible Limitations

Having presented our empirical results, we now discuss some potential limitations of our study and suggest directions for future research.

First, the fact that our main outcomes are survey-based stated preferences, without direct economic incentives, merits further discussion. While frequently used in contemporary economic research to assess people's (policy) preferences (Falk et al., 2018; Haaland et al., 2023), these measures are occasionally met with skepticism due to their possible susceptibility to various types of reporting bias. For instance, survey-based preferences, lacking immediate political or economic consequences, may be susceptible to *hypothetical bias*. This occurs when respondents' answers, based on hypothetical scenarios, do not reflect their actual behaviors in real-life situations. This bias is particularly noted in surveys valuing non-market goods, such as pristine natural environments (Carson, 2012; Kling et al., 2012). Furthermore, survey responses on sensitive issues can be affected by *social desirability bias*. For example, Coffman et al. (2017) demonstrate that the reported prevalence of antigay sentiments in the U.S. population is understated in standard surveys. Finally, Falk and Zimmermann (2013) identify *preferences for consistency* as another potential source of bias in survey-reported preferences. Specifically, they find that including certain questions in surveys influences respondents' answers to subsequent related questions. For example, asking respondents if everyone deserves a second chance can reduce their agreement to lifetime imprisonment for murderers in subsequent questions.

Despite potential biases in survey-based stated preference measures, it is reassuring that multiple pieces of evidence indicate the relevance of survey responses in actual political processes. For instance, Hainmueller et al. (2015) highlight the external validity of survey experiments, showing that the outcomes of hypothetical survey experiments match with the results from similar real-world referendums on immigration policies (see also Alesina et al., 2023; Haaland and Roth, 2023; Lergetporer and Woessmann, 2023, for further evidence). Similarly, survey-based preference measures often correlate strongly with incentivized outcomes such as donations or signing a petition (Haaland et al., 2023).

More generally, Blinder and Krueger (2004) suggest that public-opinion surveys are politically significant, as evidenced by the substantial resources politicians allocate to polling to inform their policymaking. Several empirical studies support this view. For instance, Hager and Hilbig (2020) provide quasi-experimental evidence that German politicians' policy stances are indeed shaped by public opinions as reflected in surveys. In a similar vein, Butler and Nickerson (2011) randomly sent letters with district-specific survey results on policy proposals to legislators and find that this information increased the likelihood of voting in alignment with constituent opinions. Similarly, Liaqat (2020) demonstrates that informing politicians about citizens' preferences on various policy issues increases the likelihood that they will recommend policies aligning with those preferences to their party leadership. Additionally, Banerjee et al. (2024) experimentally show that Delhi municipal councillors who received a pre-election report card two years prior to elections increased pro-poor spending in high-slum areas and influenced electoral outcomes. In summary, although we recognize the potential limitations of survey-based preference measures, the evidence discussed gives us confidence that our outcomes accurately reflect respondents' policy preferences. For future research, exploring the direct effects of our information treatments on political actions, such as signing petitions or voting behavior, would be insightful.

Second, the reasons for why effects on reform support differ across treatments require additional explanation. In particular, one intriguing result in Table 4 is that effects for T1 are considerably larger than for T3, which contains some of the same information (i.e., information about the migrant-native enrollment gap). One potential explanation for the difference in the magnitude of the coefficients is information overload (e.g. Bawden and Robinson, 2020). T1 offers a single, clear piece of information, making it easier for respondents to comprehend and use as a reference point. In contrast, T3 presents two pieces of information, which may overwhelm respondents and thus inhibit effectiveness. Additionally, the first piece of information in T3 might act as a benchmark for the second, potentially causing cognitive dissonance or confusion (for supporting evidence, see Mierisch, 2024). Furthermore, the difference might be due to the specific nature of the information; enrollment rates (T1) are more tangible and easily understood, whereas response rates (part of T3) are more abstract. While we can only speculate on these explanations, it would be interesting for future work to investigate the underlying reasons for this difference in more detail.

7. Conclusions

In this paper, we present results from a representative survey experiment investigating how information about migrant-native gaps in access to early child care affects public preferences for equity-enhancing policy reforms. Respondents have strong misperceptions about inequalities in early child care, overestimating the enrollment rate of migrants and underestimating the response rate of child care centers to inquiries from migrant parents. While providing factual information about the extent of migrant-native gaps successfully updates respondents' beliefs, it has no average effect on reform support.

Importantly, the overall null effect of information provision on reform support masks two countervailing effects for respondents with different prior beliefs about migrant-native gaps: respondents who initially underestimated inequalities increase their reform support upon receiving the information. On the other hand, those who initially overestimated inequalities tend to decrease reform support, albeit not statistically significantly so. Put together, correcting misperceptions through providing factual information narrows the gap in reform support between these two groups by as much as 43%, suggesting that information provision can reduce polarization in policy preferences.

Finally, we would like to highlight some policy implications of our results. First, we elucidate the degree of public support of different equity-enhancing policy reforms in early child care. We find that providing additional child care slots is particularly well received by the public, even enjoying majority appeal. Second, our results show a lack of prior knowledge (or awareness) about the early child care market, as indicated by the considerable variation in prior beliefs about enrollment and response rates. This lack of knowledge could explain why we find that providing factual information decreases polarization in reform support. In contrast, in settings with a better informed population, studies tend to find that information provision either increases polarization in reform support or does not affect polarization at all (see literature review by Marino et al., 2023). Our findings suggest that informational campaigns could aid policymakers in achieving greater consensus on policies targeting societal inequality in less-debated topics.

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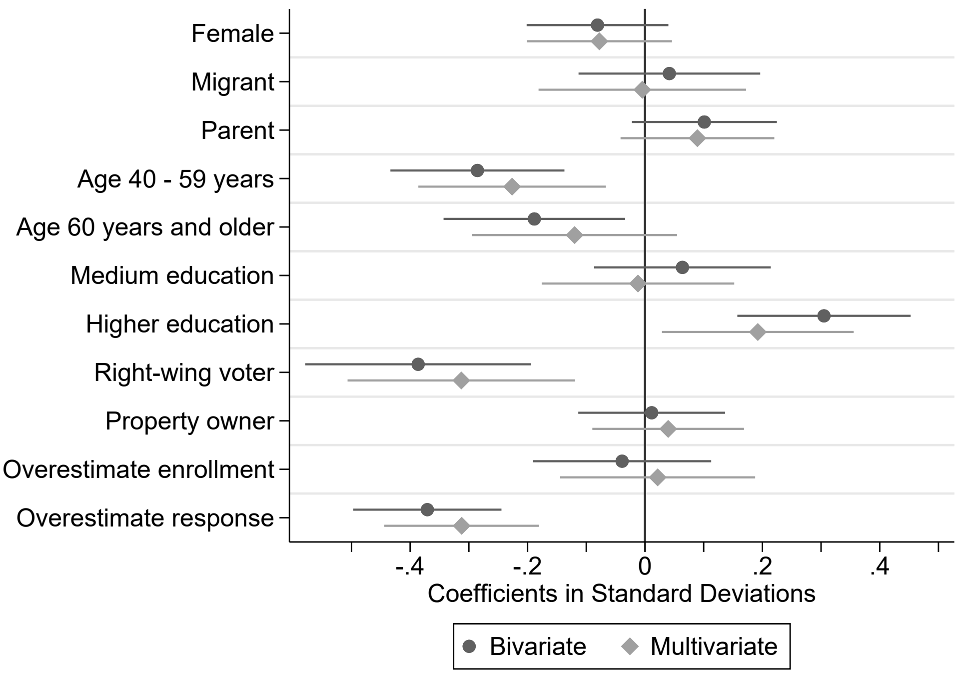
Appendix A. Figures

Figure A1: Correlation of Demographics with Reform Index in the Control Group



Notes: Figure shows OLS estimation coefficients of demographics on the reform index in the control group, in bivariate and multivariate regression models. Error bars indicate 95% confidence intervals. See Table B2 for estimation coefficients. *Reform Index* is defined as follows: Applying the procedure of Kling et al. (2007), we first z-standardize the four stated policy reform support variables (*Centralized admission*, *Increase slots*, *Preferential treatment*, and *Financial incentives*) by subtracting the control-group mean and dividing by the control-group standard deviation. Then, we calculate an equally weighted average of the standardized variables and, finally, z-standardize this average. An index is computed for all respondents who have a valid response to at least one reform variable. *Female:* Categorical variable taking a value of one if the respondent states to be female, two if the respondent states to be diverse, and zero if the respondent states to be male (omitted); the diverse category ($n = 10$) in the gender variable is not shown. *Migrant:* Indicator variable taking a value of one if the respondent has a migration background (she or either of her parents were born outside of Germany), zero otherwise (omitted). *Parent:* Indicator variable taking a value of one if the respondent is a parent (has at least one child under the age of 18 in the household), zero otherwise (omitted). *Age:* Categorical variable taking a value of two if the respondent is 60 years and older, a value of one if the respondent is between 40 and 59 years old, and a value of zero if the respondent is between 18 and 39 years old (omitted). *Education:* Categorical variable taking a value of two if the respondent has completed "Higher education" (college entrance qualification, "Abitur"), a value of one if the respondent has completed "Medium education" (middle-tier secondary education ("Realschulabschluss")), and a value of zero if the respondent has completed lower education (drop out, still in school, or lower-tier secondary education ("Hauptschulabschluss")) (omitted). *Right-wing voter:* Indicator variable taking a value of one if the respondent stated to vote for a right-wing party (AfD, NPD, Dritter Weg, or Die Rechte), zero otherwise (omitted). *Property owner:* Indicator variable taking a value one if the respondent owns a house, zero otherwise (omitted). *Overestimate enrollment:* Indicator variable taking a value one if a respondent overestimated the enrollment rate of migrants, zero otherwise (omitted). *Overestimate response:* Indicator variable taking a value one if a respondent overestimated the response rate to migrants, zero otherwise (omitted).

Appendix B. Tables

Table B1: Balance Tests

	Control	T1: Enrollment rate information	T2: Response rate information	T3: Enrollment & response rate information	Treatments (T1 | T2 | T3)
	(1)	(2)	(3)	(4)	(5)
Female	0.006	-0.004	0.019	-0.021	-0.006
	(0.015)	(0.015)	(0.015)	(0.014)	(0.015)
Migrant	-0.020	0.045**	-0.031	0.006	0.020
	(0.020)	(0.021)	(0.019)	(0.020)	(0.020)
Parent	0.006	0.016	-0.004	-0.018	-0.006
	(0.016)	(0.015)	(0.015)	(0.015)	(0.016)
40 - 59 years	-0.015	0.012	0.013	-0.010	0.015
	(0.019)	(0.019)	(0.019)	(0.018)	(0.019)
At least 60 years	0.008	0.008	-0.011	-0.005	-0.008
	(0.021)	(0.021)	(0.020)	(0.020)	(0.021)
Medium education	-0.020	0.017	0.003	0.001	0.020
	(0.020)	(0.018)	(0.019)	(0.019)	(0.020)
Higher education	-0.011	0.027	0.004	-0.021	0.011
	(0.020)	(0.019)	(0.019)	(0.019)	(0.020)
Right-wing voter	-0.005	0.004	0.014	-0.013	0.005
	(0.025)	(0.024)	(0.024)	(0.023)	(0.025)
Property owner	0.012	-0.020	-0.008	0.016	-0.012
	(0.016)	(0.016)	(0.016)	(0.015)	(0.016)
Prior enrollment rate	-0.000	0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior response rate	0.000	-0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	4,453	4,453	4,453	4,453	4,453

Notes: Table shows regression coefficients of the experimental conditions on the preregistered control variables. Results are based on multivariate OLS regressions. *Female:* Categorical variable taking a value of one if the respondent states to be female, two if the respondent states to be diverse, and zero if the respondent states to be male (omitted); the diverse category (n = 10) in the gender variable is not shown. *Migrant:* Indicator variable taking a value of one if the respondent has a migration background (she or either of her parents were born outside of Germany), zero otherwise (omitted). *Parent:* Indicator variable taking a value of one if the respondent is a parent (has at least one child under the age of 18 in the household), zero otherwise (omitted). *Age:* Categorical variable taking a value of two if the respondent is 60 years and older, a value of one if the respondent is between 40 and 59 years old, and a value of zero if the respondent is between 18 and 39 years old (omitted). *Education:* Categorical variable taking a value of two if the respondent has completed "Higher education" (college entrance qualification, "Abitur"), a value of one if the respondent has completed "Medium education" (middle-tier secondary education ("Realschulabschluss")), and a value of zero if the respondent has completed lower education (drop out, still in school, or lower-tier secondary education ("Hauptschulabschluss")) (omitted). *Right-wing voter:* Indicator variable taking a value of one if the respondent stated to vote for a right-wing party (AfD, NPD, Dritter Weg, or Die Rechte), zero otherwise (omitted). *Property owner:* Indicator variable taking a value one if the respondent owns a house, zero otherwise (omitted). *Prior enrollment rate* is the answer to the question "How many out of 100 children of Turkish parents attend a child care center (for children under 3)?" on a slider in integers from 0 to 100. *Prior response rate* is the answer to the question "How many Turkish parents who send an e-mail request to a child care center get a response?" on a slider in integers from 0 to 100. See Appendix D for detailed variable descriptions. Missing values are due to non response in the "Enrollment rate belief" and the "Response rate belief". Robust standard errors in parentheses. Significance levels: * $p <$ .10, ** $p <$ .05, *** $p <$ .01.

Table B2: Correlation between Demographics and Reform Index in the Control Group

	Reform Index
	(1)
Female	-0.077
	(0.062)
Migrant	-0.004
	(0.089)
Parent	0.085
	(0.066)
40 - 59 years	-0.224***
	(0.080)
At least 60 years	-0.110
	(0.085)
Medium education	-0.015
	(0.081)
Higher education	0.196**
	(0.080)
Right-wing voter	-0.312***
	(0.098)
Property owner	0.037
	(0.065)
Overestimate enrollment	0.015
	(0.079)
Overestimate response	-0.320***
	(0.065)
N	1,120

Notes: Table shows estimation parameters for regressions of individual characteristics on the *Reform index*. Results are based on multivariate OLS regressions in the control group. *Female:* Categorical variable taking a value of one if the respondent states to be female, two if the respondent states to be diverse, and zero if the respondent states to be male (omitted); the diverse category ($n = 10$) in the gender variable is not shown. *Migrant:* Indicator variable taking a value of one if the respondent has a migration background (she or either of her parents were born outside of Germany), zero otherwise (omitted). *Parent:* Indicator variable taking a value of one if the respondent is a parent (has at least one child under the age of 18 in the household), zero otherwise (omitted). *Age:* Categorical variable taking a value of two if the respondent is 60 years and older, a value of one if the respondent is between 40 and 59 years old, and a value of zero if the respondent is between 18 and 39 years old (omitted). *Education:* Categorical variable taking a value of two if the respondent has completed "Higher education" (college entrance qualification, "Abitur"), a value of one if the respondent has completed "Medium education" (middle-tier secondary education ("Realschulabschluss")), and a value of zero if the respondent has completed lower education (drop out, still in school, or lower-tier secondary education ("Hauptschulabschluss")) (omitted). *Right-wing voter:* Indicator variable taking a value of one if the respondent stated to vote for a right-wing party (AfD, NPD, Dritter Weg, or Die Rechte), zero otherwise (omitted). *Property owner:* Indicator variable taking a value one if the respondent owns a house, zero otherwise (omitted). *Overestimate enrollment:* Indicator variable taking a value one if a respondent overestimated the enrollment rate of migrants, zero otherwise (omitted). *Overestimate response:* Indicator variable taking a value one if a respondent overestimated the response rate to migrants, zero otherwise (omitted). *Reform Index* is defined as follows: Applying the procedure of Kling et al. (2007), we first z-standardize the four stated policy reform support variables (*Centralized admission*, *Increase slots*, *Preferential treatment*, and *Financial incentives*) by subtracting the control-group mean and dividing by the control-group standard deviation. Then, we calculate an equally weighted average of the standardized variables and, finally, z-standardize this average. An index is computed for all respondents who have a valid response to at least one reform variable. We use survey weights to affirm national representativeness. See Appendix D for detailed variable descriptions. Robust standard errors in parentheses. Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table B3: Treatment Effects on the Perception of Reasons for Unequal Chances

	Cultural Background	**More Effort**	**Parental Preferences**
	(1)	(2)	(3)
Panel A: Treatments combined			
Treatments (T1 | T2 | T3)	0.038**	0.020	0.009
	(0.016)	(0.019)	(0.018)
Scaled treatment effect	13.76	3.87	2.07
Control Mean	0.27	0.51	0.46
Panel B: Treatments separately			
T1: Enrollment rate information	0.042**	0.030	-0.009
	(0.020)	(0.023)	(0.023)
T2: Response rate information	0.040**	0.019	0.045**
	(0.020)	(0.022)	(0.023)
T3: Enrollment & response rate information	0.030	0.009	-0.009
	(0.020)	(0.023)	(0.022)
Pre-specified Controls	Yes	Yes	Yes
N	4,822	4,822	4,822

Notes: Table shows treatment effects on an indicator for whether or not a respondent agreed with a given reason for inequality. Results are based on multivariate OLS regressions. Outcome variables are defined as follows: Column (1): *Cultural background* is an indicator variable taking a value of one if the respondent agreed to/checked the statement "Turkish parents are disadvantaged because of their migration background.", zero otherwise; Column (2): *More effort* is an indicator variable taking a value of one if the respondent agreed to/checked the statement "Child care centers suspect a greater workload among Turkish parents, e.g., because of language barriers.", zero otherwise; Column (3): *Parental preferences* is an indicator variable taking a value of one if the respondent agreed to/checked the statement "Child care centers make sure that the proportion of Turkish children in the groups is not too large, because many parents want it that way.", zero otherwise. If the respondent stated "Don't know", answers are coded as "don't agree". *T1: Enrollment information*, *T2: Response rate information*, and *T3: Enrollment & response rate information* are indicator variables taking a value of one if the respondent is in the respective treatment group, zero otherwise. *Treatments (T1 | T2 | T3)* is an indicator variable taking a value of one if the respondent is in any of the three treatment groups, zero otherwise. Panel A shows treatment effects for all treatment groups pooled. Panel B shows treatment effects separately for each treatment group. *Pre-specified Controls* include gender in three categories (female, male, diverse), age in three categories (between 18 and 39 years old, between 40 and 59 years old, at least 60 years old), education in three categories (completed lower, medium, or higher education), and wealth in two categories (respondent owns property or not). We use survey weights to ensure national representativeness. See Appendix D for detailed variable descriptions. *Scaled treatment effect* expresses the treatment effect relative to the mean of the respective outcome in the control group in percent. Robust standard errors in parentheses. Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table B4: Treatment Effect Heterogeneity for Individual Policy Reforms

	Centralized Admission	**Additional Slots**	**Preferential Enrollment**	**Financial Incentives**
	(1)	(2)	(3)	(4)
Panel A: Treatments combined				
Treatments (T1 | T2 | T3)	-0.031	-0.044	0.017	-0.080
	(0.057)	(0.045)	(0.046)	(0.058)
Treatments (T1 | T2 | T3) × Underestimator	0.125	0.079	0.100	0.222**
	(0.109)	(0.090)	(0.082)	(0.104)
Underestimator	-0.230**	-0.129*	-0.271***	-0.376***
	(0.095)	(0.077)	(0.070)	(0.090)
Panel B: Treatments separately				
T1: Enrollment rate information	0.030	-0.056	0.036	-0.065
	(0.070)	(0.057)	(0.056)	(0.071)
T2: Response rate information	-0.081	-0.064	0.016	-0.126*
	(0.068)	(0.057)	(0.057)	(0.071)
T3: Enrollment & response rate information	-0.042	-0.010	-0.001	-0.048
	(0.069)	(0.058)	(0.055)	(0.070)
T1: Enrollment rate information × Underestimator	0.067	0.190*	0.160	0.326**
	(0.133)	(0.109)	(0.101)	(0.129)
T2: Response rate information × Underestimator	0.176	0.076	-0.021	0.171
	(0.134)	(0.112)	(0.104)	(0.131)
T3: Enrollment & response rate information × Underestimator	0.131	-0.027	0.173*	0.178
	(0.133)	(0.114)	(0.099)	(0.124)
Underestimator	-0.230**	-0.129*	-0.271***	-0.376***
	(0.095)	(0.077)	(0.070)	(0.090)
Pre-specified Controls	Yes	Yes	Yes	Yes
N	4,634	4,713	4,739	4,714

Notes: Table shows heterogeneous treatment effects on individual policy reforms by prior beliefs. Results are based on multivariate OLS regressions. Outcome variables are defined as follows: Column (1): Answer on a five-point Likert scale to the statement "Instead of individual child care center managers, a central office at the community level should decide which child gets a child care slot."; Column (2): Answer on a five-point Likert scale to the statement "The number of child care slots should be further expanded using taxpayers' money."; Column (3): Answer on a five-point Likert scale to the statement "Families with a migration background should be given preference in the allocation of child care slots."; Columns (4): Answer on a five-point Likert scale to the statement "Child care centers should receive more support from taxpayers to accommodate children with an immigrant background."; Column (5): An index combining support for all reforms. *Underestimator* is an indicator variable taking a value of one if the respondent answered both of the belief questions for migrants (*Prior enrollment rate*, *Prior response rate*) with values higher than the actual values (12 out of 100 and 63 out of 100, respectively), otherwise respondents are classified as *Overestimators*. *T1: Enrollment information*, *T2: Response rate information*, and *T3: Enrollment & response rate information* are indicator variables taking a value of one if the respondent is in the respective treatment group, zero otherwise. *Treatments (T1 | T2 | T3)* is an indicator variable taking a value of one if the respondent is in any of the three treatment groups, zero otherwise. Panel A shows treatment effects for all treatment groups pooled. Panel B shows treatment effects separately for each treatment group. *Pre-specified Controls* include gender in three categories (female, male, diverse), age in three categories (between 18 and 39 years old, between 40 and 59 years old, at least 60 years old), education in three categories (completed lower, medium, or higher education), and wealth in two categories (respondent owns property or not). We use survey weights to ensure national representativeness. See Appendix D for detailed variable descriptions. Robust standard errors in parentheses. Significance levels: * $p <$ .10, ** $p <$ .05, *** $p <$ .01.

Table B5: Treatment Effect Heterogeneity by Prior Beliefs (PCA)

	Reform Index				
	Full Sample (1)	Underestimator (2)	Underestimator (3)	Overestimator (4)	Overestimator (5)
Treatments Treatments (T1 | T2 | T3)	-0.051	0.183**		-0.047	
	(0.059)	(0.088)		(0.059)	
× Underestimator	0.228**				
	(0.105)				
Underestimator	-0.459***				
	(0.089)				
T1: Enrollment rate information			0.325***		-0.008
			(0.110)		(0.072)
T2: Response rate information			0.055		-0.098
			(0.114)		(0.073)
T3: Enrollment response rate information			0.180*		-0.036
			(0.106)		(0.071)
Pre-specified Controls	Yes	Yes	Yes	Yes	Yes
N	4,554	1,336	1,336	3,218	3,218

Notes: Table shows treatment effects on the *Reform Index* constructed with a Principal Component Analysis by prior beliefs. Results are based on multivariate OLS regressions. Note that there are more missing values in the PCA-based index than in our baseline reform index used throughout the paper. This is because our baseline index only excludes participants who did not answer any policy reform question, whereas the PCA-based index excludes participants who did not answer at least one policy reform question. *Underestimator* is an indicator variable taking a value of one if the respondent answered both of the belief questions for migrants (*Prior enrollment rate*, *Prior response rate*) with values higher than the actual values (12 out of 100 and 63 out of 100, respectively), otherwise respondents are classified as *Overestimators.* In Column (1) we run OLS regressions on the full sample. In Columns (2)–(5), we estimate treatment effects for the subsamples of *Underestimators* (Columns (2) and (3)) and *Overestimators* (Columns (4) and (5)) separately. *T1: Enrollment information*, *T2: Response rate information*, and *T3: Enrollment & response rate information* are indicator variables taking a value of one if the respondent is in the respective treatment group, zero otherwise. *Treatments (T1 | T2 | T3)* is an indicator variable taking a value of one if the respondent is in any of the three treatment groups, zero otherwise. *Reform Index* is defined as follows: Applying the procedure of Kling et al. (2007), we first z-standardize the four stated policy reform support variables (*Centralized admission*, *Increase slots*, *Preferential treatment*, and *Financial incentives*) by subtracting the control-group mean and dividing by the control-group standard deviation. Then, we calculate an equally weighted average of the standardized variables and, finally, z-standardize this average. An index is computed for all respondents who have a valid response to at least one reform variable. *Pre-specified Controls* include gender in three categories (female, male, diverse), age in three categories (between 18 and 39 years old, between 40 and 59 years old, at least 60 years old), education in three categories (completed lower, medium, or higher education), and wealth in two categories (respondent owns property or not). We use survey weights to ensure national representativeness. See Appendix D for detailed variable descriptions. Robust standard errors in parentheses. Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table B6: Treatment Effect Heterogeneity by Demographics and Policy Variables

	Reform Index			
	(1)	(2)	(3)	(4)
Treatments (T1 | T2 | T3)	-0.073	0.078*	0.043	-0.009
	(0.049)	(0.047)	(0.041)	(0.035)
× Female	0.161**			
	(0.071)			
× Parent		-0.166**		
		(0.073)		
× Right-wing voter			-0.355***	
			(0.111)	
× Trust Index				0.074**
				(0.036)
Female	-0.076			
	(0.061)			
Parent		0.108*		
		(0.063)		
Right-wing voter			-0.336***	
			(0.097)	
Trust Index				0.227***
				(0.031)
Pre-specified Controls	Yes	Yes	Yes	Yes
N	4,767	4,767	4,767	4,764

Notes: Table shows treatment effects on the *Reform Index* interacted with different background characteristics. Results are based on multivariate OLS regressions. *Treatments (T1 | T2 | T3)* is an indicator variable taking a value of one if the respondent is in any of the three treatment groups, zero otherwise. *Reform Index* is defined as follows: Applying the procedure of Kling et al. (2007), we first z-standardize the four stated policy reform support variables (*Centralized admission*, *Increase slots*, *Preferential treatment*, and *Financial incentives*) by subtracting the control-group mean and dividing by the control-group standard deviation. Then, we calculate an equally weighted average of the standardized variables and, finally, z-standardize this average. An index is computed for all respondents who have a valid response to at least one reform variable. *Pre-specified Controls* include gender in three categories (female, male, diverse), age in three categories (between 18 and 39 years old, between 40 and 59 years old, at least 60 years old), education in three categories (completed lower, medium, or higher education), and wealth in two categories (respondent owns property or not). We use survey weights to ensure national representativeness. See Appendix D for detailed variable descriptions. Robust standard errors in parentheses. Significance levels: * $p <$ .10, ** $p <$ .05, *** $p <$ .01.

Table B7: Treatment Effect Heterogeneity by Regional Characteristics

	Reform Index			
	(1)	(2)	(3)	(4)
Treatments (T1 | T2 | T3)	0.024	0.021	0.025	0.034
	(0.099)	(0.052)	(0.058)	(0.049)
× Migrant incentive	-0.020			
	(0.106)			
× No fees		-0.025		
		(0.071)		
× Intermediate			-0.017	
			(0.082)	
× Rural			-0.029	
			(0.102)	
× Share Rationed (in percent)				0.002
				(0.003)
Migrant incentive	-0.033			
	(0.090)			
No fees		0.124**		
		(0.061)		
Intermediate			-0.089	
			(0.070)	
Rural			-0.106	
			(0.089)	
Share Rationed (in percent)				-0.001
				(0.002)
Pre-specified Controls	Yes	Yes	Yes	Yes
N	4,767	4,767	4,561	3,685

Notes: Table shows treatment effects on the *Reform Index* by regional characteristics. Results are based on multivariate OLS regressions. *Migrant incentive:* indicates whether the federal state ($k = 16$) of the respondent provides an additional financial incentive to child care centers for taking up migrant children (nine federal states provide such an incentive); *No fees*: indicates whether the respondent's federal state has regulations ensuring that child care is free of charge for certain age ranges (11 federal states have such regulations in place); *Intermediate* and *Rural:* Based on an urbanity variable with three categories (city, intermediate, rural) indicating the urban classification of the county ($k = 398$) of the respondent, provided by Eurostat (see Eurostat and Dijkstra et al. (2019) for the methodology); *Share rationed*: defined as stated demand from parents for child care minus actual usage of child care in a county in percent (based on survey data from the Child Care Study (KiBS), funded by the Federal Ministry for Family Affairs); information on slot rationing is missing for 1,094 (22.4%) respondents because the KiBS data do not provide information on slot rationing for their respective county. *T1: Enrollment information*, *T2: Response rate information*, and *T3: Enrollment & response rate information* are indicator variables taking a value of one if the respondent is in the respective treatment group, zero otherwise. *Treatments (T1 | T2 | T3)* is an indicator variable taking a value of one if the respondent is in any of the three treatment groups, zero otherwise. *Reform Index* is defined as follows: Applying the procedure of Kling et al. (2007), we first z-standardize the four stated policy reform support variables (*Centralized admission*, *Increase slots*, *Preferential treatment*, and *Financial incentives*) by subtracting the control-group mean and dividing by the control-group standard deviation. Then, we calculate an equally weighted average of the standardized variables and, finally, z-standardize this average. An index is computed for all respondents who have a valid response to at least one reform variable. *Pre-specified Controls* include gender in three categories (female, male, diverse), age in three categories (between 18 and 39 years old, between 40 and 59 years old, at least 60 years old), education in three categories (completed lower, medium, or higher education), and wealth in two categories (respondent owns property or not). We use survey weights to ensure national representativeness. Robust standard errors in parentheses. Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table B8: Treatment Effect Heterogeneity by Non-Linear Response Rate Beliefs

	Reform Index		
	(1)	(2)	(3)
Treatments (T1 | T2 | T3)	-0.071	-0.229*	-0.100
	(0.079)	(0.122)	(0.074)
× Prior response rate	0.001	0.010**	
	(0.001)	(0.005)	
× Prior response rate squared		-0.801*	
		(0.455)	
× Prior response rate 2nd quartile			0.180*
			(0.105)
× Prior response rate 3rd quartile			0.139
			(0.104)
× Prior response rate 4th quartile			0.125
			(0.105)
Pre-specified Controls	Yes	Yes	Yes
Control Mean	52.77	52.77	52.77
N	4,497	4,497	4,497

Notes: Table shows treatment effects on an index combining support for all policy reforms interacted with linear and quadratic terms of the response rate beliefs in Columns (1) and (2). In Column (3), we provide results when interacting the treatment indicator with quartiles of the response rate belief. Results are based on multivariate OLS regressions. *Treatments (T1 | T2 | T3)* is an indicator variable taking a value of one if the respondent is in any of the three treatment groups, zero otherwise. *Reform Index* is defined as follows: Applying the procedure of Kling et al. (2007), we first z-standardize the four stated policy reform support variables (*Centralized admission*, *Increase slots*, *Preferential treatment*, and *Financial incentives*) by subtracting the control-group mean and dividing by the control-group standard deviation. Then, we calculate an equally weighted average of the standardized variables and, finally, z-standardize this average. An index is computed for all respondents who have a valid response to at least one reform variable. *Pre-specified Controls* include gender in three categories (female, male, diverse), age in three categories (between 18 and 39 years old, between 40 and 59 years old, at least 60 years old), education in three categories (completed lower, medium, or higher education), and wealth in two categories (respondent owns property or not). We use survey weights to ensure national representativeness. See Appendix D for detailed variable descriptions. Robust standard errors in parentheses. Significance levels: * $p <$ .10, ** $p <$ .05, *** $p <$ .01.

Appendix C. Details about the Survey

Appendix C.1. Sampling Method

The survey was conducted by the survey company Verian (known as Kantar Public until November 2023). Verian holds a reputable position as a survey company in Germany. For example, the company is responsible for the election reporting of Infratest dimap for ARD (a consortium of public broadcasters in Germany), conducting the German Socio-Economic Panel (SOEP), IAB Establishment Panel, and contributing to the European Social Survey (ESS). Such quota-representative samples from commercial survey providers, often recruited through loyalty panels, are commonly used in economic research (Stantcheva, 2022). Reassuringly, Grewenig et al. (2023) show that online samples drawn to match population characteristics represent the entire population (of onliners and offliners) well.

Our survey was conducted using Verian's Payback Online Panel. This panel comprises approximately 150,000 members who are actively recruited from the Payback loyalty program. As the largest consumer bonus program in Germany, Payback includes around 25 million consumers, representing roughly half of all German households.

The survey sampled respondents using two quotas to ensure representativeness. For the first quota, cells were constructed to reflect the German population by gender (male and female), education background (lower, middle, higher), and age (three age bins: 18 to 39 years, 40 to 59 years, and 60+ years). For the second quota, cells represent 30 regional areas in Germany, ensuring that the sample distribution matched the share of inhabitants in the total German voting-age population across these regions. These regions are: Berlin, Brandenburg, Bremen, Hamburg, Mecklenburg-Western Pomerania, Lower Saxony, Rhineland-Palatinate, Saarland, Saxony, Saxony-Anhalt, Schleswig-Holstein, Thuringia, Upper Bavaria, Lower Bavaria, Upper Franconia, Middle Franconia, Lower Franconia, Upper Palatinate, Swabia, Freiburg, Karlsruhe, Stuttgart, Tübingen, Darmstadt, Giessen, Kassel, Arnsberg, Detmold, Düsseldorf, Cologne, and Münster.

The sample was drawn using a random selection process within the specified quota cells from the Payback Online Panel. Compared to the broader German population, the distribution of the sample by gender, age, education, and region of residence aligns well with the target quotas derived from census data and the Microcensus of the German Federal Statistical Office. To account for minor deviations from the general population (e.g., due to the quality checks described below), survey weights provided by the survey company were applied in all empirical analyses.

The sampling method can be classified as probability sampling within the constraints of an online access panel. Specifically, while the initial recruitment into the Payback Online Panel may involve non-probability elements, the selection of respondents for our survey involved random sampling within predefined quotas. This approach ensures that each individual within the target population has a known and non-zero chance of being selected, aligning with the principles of probability sampling.

Our gross sample consists of $n = 5,059$ respondents of which we drop $n = 237$ respondents because they either responded to less than 60% of questions or they were below 40% of the median survey completion time (both conditions are quality checks implemented by the survey company). Note that we initially sampled another $n = 1,260$ respondents for a different treatment which is not analyzed in this paper (see Appendix E). Our final sample consists of $n = 4,822$ respondents.

Appendix C.2. Prior Belief Questions

In this section, we provide screenshots of the survey questions and the English translation of the questions.

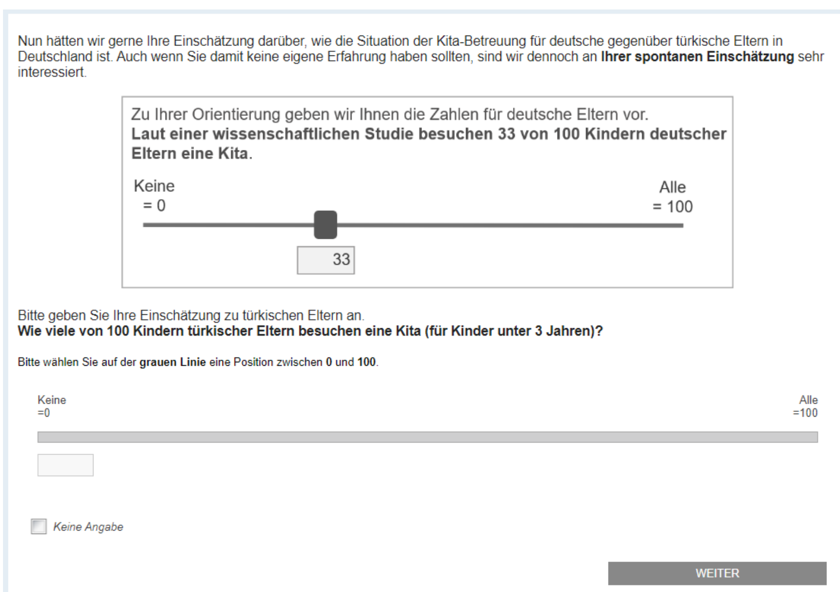
Appendix C.2.1. Prior Belief about Enrollment Rate

Translation: We would now like to ask for your assessment of the situation regarding child care for German vs. Turkish parents in Germany. Even if you have no personal experience with this, we are still very interested in your spontaneous assessment.

For your orientation, we provide you with the figures for German parents: According to a scientific study, 33 out of 100 children of German parents attend a child care center.

Please now give your assessment for Turkish parents.
How many out of 100 children of Turkish parents attend a child care center (for children under the age of 3)?

Figure C1: Survey Question: Prior Belief about Enrollment Rate



Nun hätten wir gerne Ihre Einschätzung darüber, wie die Situation der Kita-Betreuung für deutsche gegenüber türkische Eltern in Deutschland ist. Auch wenn Sie damit keine eigene Erfahrung haben sollten, sind wir dennoch an **Ihrer spontanen Einschätzung** sehr interessiert.

Zu Ihrer Orientierung geben wir Ihnen die Zahlen für deutsche Eltern vor.
Laut einer wissenschaftlichen Studie besuchen 33 von 100 Kindern deutscher Eltern eine Kita.

Keine = 0 | Alle = 100

33

Bitte geben Sie Ihre Einschätzung zu türkischen Eltern an.
Wie viele von 100 Kindern türkischer Eltern besuchen eine Kita (für Kinder unter 3 Jahren)?

Bitte wählen Sie auf der **grauen Linie** eine Position zwischen **0** und **100**.

Keine =0 | Alle =100

Keine Angabe

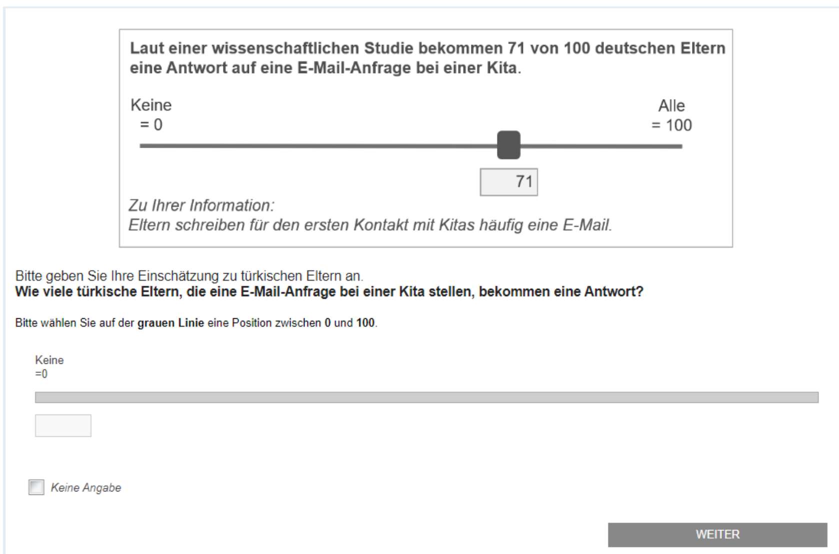
WEITER

Appendix C.2.2. Prior Belief about Response Rate

Translation: According to a scientific study, 71 out of 100 German parents receive a reply to an email inquiry from a child care center. For your information: Parents often write an e-mail for their first contact with child care centers.

Please give your assessment of Turkish parents.
How many Turkish parents who send an e-mail inquiry to a child care center receive a reply?

Figure C2: Survey Question: Prior Belief about Response Rate



Appendix C.3. Details about the Treatments

Appendix C.3.1. Treatment 1: Enrollment Rate Information

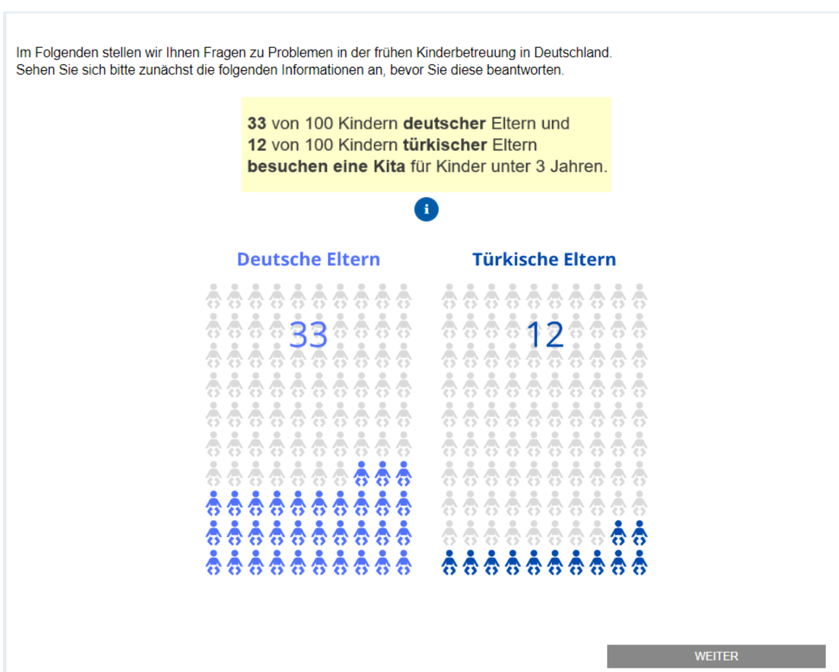
Translation: In the following, we ask questions about problems in early child care in Germany. Please take a look at the following information before you answer these questions.

The text box (see Figure C3) shows the following text: 33 out of 100 children of German parents and 12 out of 100 children of Turkish parents attend a child care center for children under the age of 3.

If respondents clicked on the blue information symbol, the following text would appear: "The figures shown are taken from an internationally published study by German scientists (Jessen et al., 2020). The study is based on an evaluation of data from the Child Care Study (KiBS) - funded by the Federal Ministry for Family Affairs. As part of this study, the child care needs of around 33,000 parents in Germany have been surveyed at regular intervals in a representative sample since 2015.

Source: Jessen, J., Schmitz, S., & Waights, S. (2020). Understanding Day Care Enrollment Gaps. Journal of Public Economics, 190, 104252."

Figure C3: Treatment 1: Enrollment Rate Information



Appendix C.3.2. Treatment 2: Response Rate Information

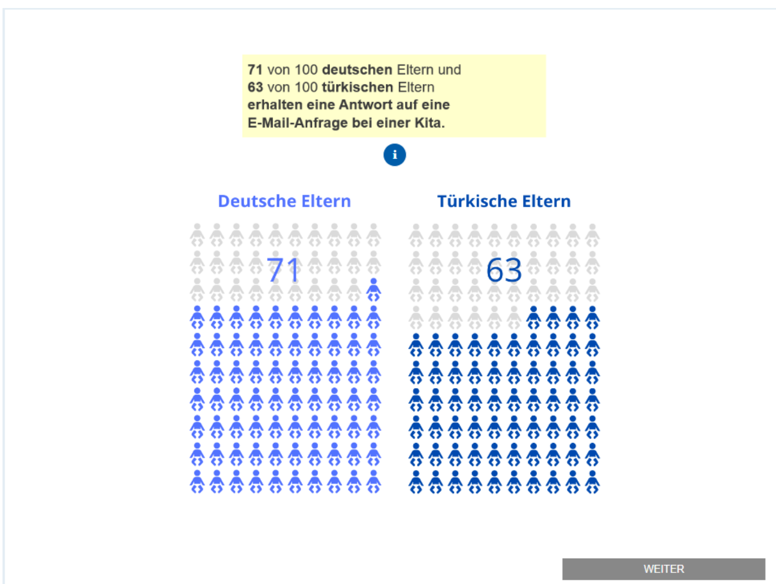
Translation: In the following, we ask you questions about problems in early child care in Germany. Please take a look at the following information before you answer these questions.

The text box (see Figure C4) shows the following text: 71 out of 100 German parents and 63 out of 100 Turkish parents who send an e-mail inquiry to a child care center receive a response.[17]

If respondents clicked on the blue information symbol, the following text would appear: "In 2020, researchers from several German research institutes sent emails to a representative sample of child care centers across Germany. The emails were typical parent requests that differed only in the name of the sender. The names indicated either German or Turkish origin. By tracking the number of responses, the researchers were able to calculate the results shown.

Source: Hermes, H., Lergetporer, P., Mierisch, F., Peter, F., & Wiederhold, S. (2022). Discrimination on the Child Care Market: A Nationwide Field Experiment. mimeo."

Figure C4: Treatment 2: Response Rate Information



[17]We compute the raw response rate difference from the study by Hermes et al. (2023) as follows: We compare the response rate for emails from migrants ($N = 4,661$, 63.3% response rate) to the response rate for emails from natives ($N = 4,682$, 70.8%) for all emails without the higher education signal, and round to integers.

Appendix C.3.3. Treatment 3: Enrollment & Response Rate Information

Translation: In the following, we ask questions about problems in early child care in Germany. Please take a look at the following information before you answer these questions.

We then proceed by showing first *Treatment 1: Enrollment Rate Information* (see Figure C3) and then *Treatment 2: Response Rate Information* (see Figure C4).

Appendix C.4. Outcome Measures

Appendix C.4.1. Outcome: Reform Support

Translation: "How much do you agree with the following policies for child care under age 3 in Germany?" Respondents are then asked to choose one out of six answer categories (five-point Likert scale from "I fully disagree" to " I fully agree", and an option for "No answer/Not specified") for each of the four policy measures. The policy measures respondents have to assess are (see Figure C5):

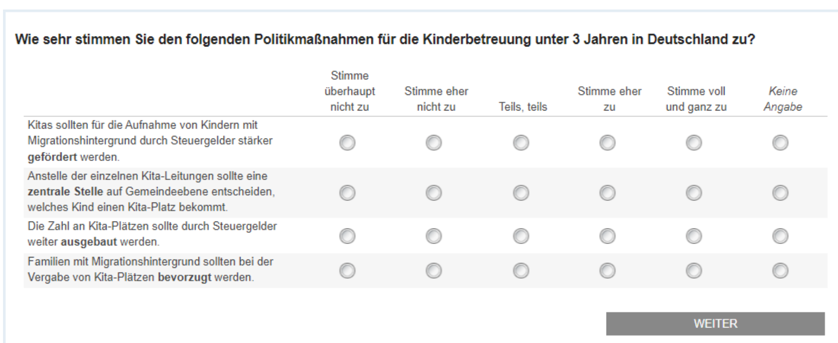
(i) "Instead of the individual child care center managers, a central office at the municipal level should decide which child gets a slot in a child care center."

(ii) "The number of slots in child care centers should be further expanded using tax money."

(iii) "Families with a migration background should be given preference in the allocation of child care slots."

(iv) "Child care centers should get additional funding for the admission of children with a migration background."

Figure C5: Outcome: Reform Support



Wie sehr stimmen Sie den folgenden Politikmaßnahmen für die Kinderbetreuung unter 3 Jahren in Deutschland zu?

	Stimme überhaupt nicht zu	Stimme eher nicht zu	Teils, teils	Stimme eher zu	Stimme voll und ganz zu	*Keine Angabe*
Kitas sollten für die Aufnahme von Kindern mit Migrationshintergrund durch Steuergelder stärker **gefördert** werden.						
Anstelle der einzelnen Kita-Leitungen sollte eine **zentrale Stelle** auf Gemeindeebene entscheiden, welches Kind einen Kita-Platz bekommt.						
Die Zahl an Kita-Plätzen sollte durch Steuergelder weiter **ausgebaut** werden.						
Familien mit Migrationshintergrund sollten bei der Vergabe von Kita-Plätzen **bevorzugt** werden.						

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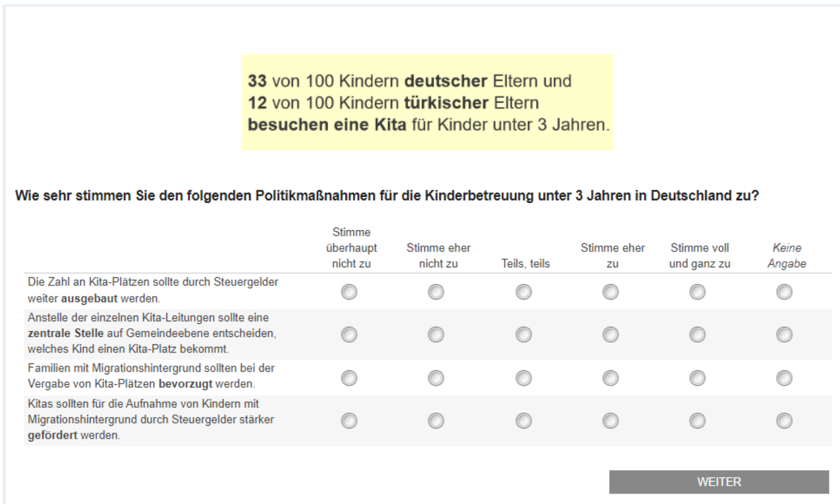
Depending on the treatment group, we repeat the provided treatment information in the form of a text box. The picture shows an example of the reform support elicitation including the repetition of the provided treatment information in the form of a text box for *Treatment 1: Enrollment Rate Information* (see C3). The control group receives no such additional text box.

Figure C6: Outcome: Reform Support + Information Box Example



33 von 100 Kindern **deutscher** Eltern und
12 von 100 Kindern **türkischer** Eltern
besuchen eine Kita für Kinder unter 3 Jahren.

Wie sehr stimmen Sie den folgenden Politikmaßnahmen für die Kinderbetreuung unter 3 Jahren in Deutschland zu?

	Stimme überhaupt nicht zu	Stimme eher nicht zu	Teils, teils	Stimme eher zu	Stimme voll und ganz zu	*Keine Angabe*
Die Zahl an Kita-Plätzen sollte durch Steuergelder weiter **ausgebaut** werden.	○	○	○	○	○	○
Anstelle der einzelnen Kita-Leitungen sollte eine **zentrale Stelle** auf Gemeindeebene entscheiden, welches Kind einen Kita-Platz bekommt.	○	○	○	○	○	○
Familien mit Migrationshintergrund sollten bei der Vergabe von Kita-Plätzen **bevorzugt** werden.	○	○	○	○	○	○
Kitas sollten für die Aufnahme von Kindern mit Migrationshintergrund durch Steuergelder stärker **gefördert** werden.	○	○	○	○	○	○

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Appendix C.4.2. Outcome: Reasons for Unequal Chances

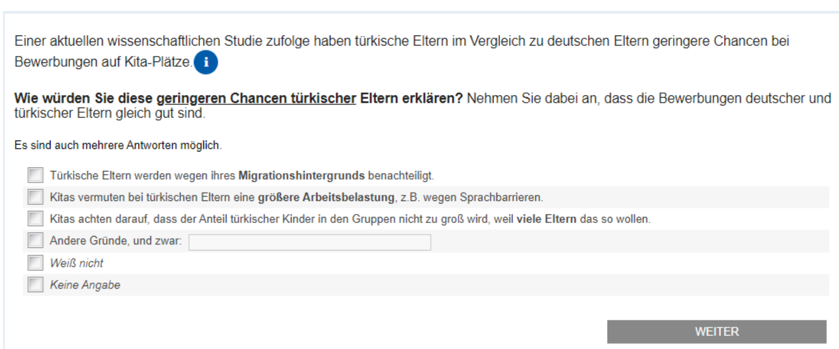
Translation: "According to a recent scientific study, Turkish parents have lower chances of applying for child care slots compared to German parents. How would you explain these lower chances for Turkish parents? Assume that the applications of German and Turkish parents are equally good." Respondents could then select multiple of the following reasons (see Figure C7):

(i) "Turkish parents are disadvantaged because of their cultural background."
(ii) "Child care centers assume that Turkish parents come along with a greater workload, e.g., because of language barriers."
(iii) "Child care centers make sure that the share of Turkish children in the groups is not too large, accommodating what many parents want."
(iv) "Other following reason: [open text field]"
(v) "Don't know."
(vi) "Not specified."

If respondents clicked on the blue information symbol, the following text would appear: "In 2020, researchers from several German research institutes sent emails to a representative sample of child care centers across Germany. The emails were typical parent requests that differed only in the name of the sender. The names indicated either German or Turkish origin. By tracking the number of responses, the researchers were able to calculate the results shown.

Source: Hermes, H., Lergetporer, P., Mierisch, F., Peter, F., & Wiederhold, S. (2022). Discrimination on the Child Care Market: A Nationwide Field Experiment. mimeo."

Figure C7: Outcome: Reasons for Unequal Chances



Appendix D. Data Section

Table D1: Variable Definitions

Variable Name	Variable Definition	Missing Values
Prior Beliefs		
Prior enrollment rate	Answer to the question "How many out of 100 children of Turkish parents attend a child care center (for children under 3)?" on a slider in integers from 0 to 100 (see Figure C1).	Variable is missing for 247 (5.6%) respondents due to item non-response.
Prior response rate	Answer to the question "How many Turkish parents who send an e-mail request to a child care center get a response?" on a slider in integers from 0 to 100 (see Figure C2).	Variable is missing for 302 (6.8%) respondents due to item non-response.
Overestimate enrollment rate	Indicator variable taking a value of one if the respondent answered the prior question *Prior enrollment rate* with values higher than the real enrollment rate of migrants of 12 out of 100.	Variable is missing for 247 (5.6%) respondents due to item non-response.
Overestimate response rate	Indicator variable taking a value of one if the respondent answered the prior question *Prior response rate* with values higher than the real response rate to inquiries of migrant parents of 63 out of 100.	Variable is missing for 302 (6.8%) respondents due to item non-response.
Underestimator	Underestimator is an indicator variable taking a value of one if the respondent answered both of the belief questions (*Prior enrollment rate*, *Prior response rate*) with values higher than the actual values (12 out of 100 and 63 out of 100, respectively), otherwise respondents are classified as *Overestimators*.	None

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Table D1: Continued

Variable Name	Variable Definition	Missing Values
Outcome Variables		
Reform Support		
Centralized admission	Answer on a five-point Likert scale to the statement “Instead of individual child care center managers, a central office at the community level should decide which child gets a child care slot.”.	Variable is missing for 174 (3.9%) respondents due to item non-response.
Increase slots	Answer on a five-point Likert scale to the statement “The number of child care slots should be further expanded using taxpayers’ money.”.	Variable is missing for 104 (2.4%) respondents due to item non-response.
Preferential treatment	Answer on a five-point Likert scale to the statement “Families with a migration background should be given preference in the allocation of child care slots.”.	Variable is missing for 79 (1.8%) respondents due to item non-response.
Financial incentives	Answer on a five-point Likert scale to the statement “child care centers should receive more support from taxpayers to accommodate children with an immigrant background.”.	Variable is missing for 104 (2.4%) respondents due to item non-response.
Reform Index	Applying the procedure of Kling et al. (2007), we first z-standardize the four stated policy reform support variables (*Centralized admission*, *Increase slots*, *Preferential treatment*, and *Financial incentives*) by subtracting the control-group mean and dividing by the control-group standard deviation. Then, we calculate an equally weighted average of the standardized variables and, finally, z-standardize this average. An index is computed for all respondents who have a valid response to at least one reform support variable.	Variable is missing for 55 (1.1%) respondents due to item non-response on all reform support variables.

(continued on next page)

Table D1: Continued

Variable Name	Variable Definition	Missing Values
Reasons for Unequal Chances		
Cultural background	Indicator variable taking a value of one if the respondent agreed to/checked the statement "Turkish parents are disadvantaged because of their migration background.", zero otherwise.	None
More effort	Indicator variable taking a value of one if the respondent agreed to/checked the statement "Child care centers suspect a greater workload among Turkish parents, e.g., because of language barriers.", zero otherwise.	None
Parental preferences	Indicator variable taking a value of one if the respondent agreed to/checked the statement "Child care centers make sure that the proportion of Turkish children in the groups is not too large, because many parents want it that way.", zero otherwise.	None
Treatment Variables		
T1: Enrollment rate information	Indicator variable taking a value of one if the respondent was shown the visual representation of the enrollment rate of migrants into early child care (see Figure C3), zero otherwise.	None
T2: Response rate information	Indicator variable taking a value of one if the respondent was shown the visual representation of the response rate of child care center managers to inquiries by migrant parents (see Figure C4), zero otherwise.	None
T3: Enrollment & response rate information	Indicator variable taking a value of one if the respondent was shown the visual representation of the enrollment rate of migrants into early child care and the visual representation of the response rate of child care center managers to inquiries by migrant parents (see Figure C3, and Figure C4), zero otherwise.	None
Treatments (T1 | T2 | T3)	Indicator variable taking a value of one if the respondent is in any of the three treatment groups, zero otherwise.	None

(continued on next page)

Table D1: Continued

Variable Name	Variable Definition	Missing Values
Demographic and Policy Variables		
Female	Categorical variable taking a value of one if the respondent states to be female, two if the respondent states to be diverse, zero if the respondent states to be male.	None
Migrant	Indicator variable taking a value of one if the respondent has a migration background (she, or either of her parents were born outside of Germany), zero otherwise.	None
Parent	Indicator variable taking a value of one if the respondent is a parent (at least one child under the age of 18 is living in the respondent's household), zero otherwise.	None
Age	Categorical variable taking the value of two, if the respondent is at least 60 years old, the value of one if the respondent is between 40 and 59 years old, and zero if the respondent is between 18 and 39 years old.	None
Education	Categorical variable taking a value of two if the respondent has attained "Higher education" (college entrance qualification, Abitur), the value of one if the respondent has attained "Medium education" ("Realschulabschluss"), and zero if the respondent has attained lower education (drop out, still in school, or upper secondary education "Hauptschulabschluss")	None
Right-wing voter	Indicator variable taking a value of one if the respondent stated to vote for a right-wing party (AfD, NPD, Dritter Weg, or Die Rechte), zero otherwise.	None
Left-wing voter	Indicator variable taking a value of one if the respondent stated to vote for a left-wing party (Die Linke, Die Partei, KPD, DKP, or MLDP), zero otherwise.	None
Property owner	Indicator variable taking a value one if the respondent owns a house, zero otherwise.	None
Region	The particpant's place of residence at the NUTS2-level.	None
Household Size	Number of people living in the respondent's household.	None
Trust index	Index combining six questions regarding trust in institutions: the Bundestag (i.e., the German Parliament), judiciary, police, politicians, parties, and the European Parliament (each measured on a scale from 0 to 10). We apply the procedure by Kling et al. (2007) to compute the index (available for all respondents who have a valid response to at least one trust question).	Variable is missing for 3 respondents due to item non-response.

Appendix E. Enrollment Wish

In addition to the three main treatments presented in this paper, we conducted a fourth randomized treatment with information regarding the enrollment *wish* of migrant parents. Specifically, we disclosed that 40 out of 100 Turkish parents (as compared to 44 out of 100 native parents) wished to enroll their children in early child care, and combine this information with the data on enrollment rates for natives (33/100) and migrants (12/100) (Jessen et al., 2020). The rationale behind this treatment was to offer respondents insights into the degree of slot rationing, i.e., the enrollment rate conditional on demand for enrollment.

Before providing respondents with evidence on the enrollment wishes of both groups, we elicited their prior beliefs regarding the enrollment wish of migrant parents, mirroring our approach for gauging beliefs on enrollment rate and response rate (see Figure C1). The average prior belief about enrollment wishes for Turkish parents was reasonably accurate, but also showed substantial variation (mean: 42.8, SD: 28.4). Estimating the impact of this treatment on reform support, we found no average treatment effects, in line with the other treatments.

In contrast to our main analysis in the paper, there is no straightforward way of analyzing the heterogeneity of the treatment effect by prior beliefs for this treatment arm because the interplay of these two beliefs (enrollment wish and enrollment rate) is complex and ambiguous. For example, respondents might have the belief that the enrollment rate for migrants is low, because demand is low as well, i.e., differences in enrollment would be driven by preferences instead of unequal enrollment chances. Providing such respondents with the accurate information about the enrollment wish and the enrollment rate creates an ambiguous shift in their perception of inequalities because the direction and intensity of the shift relies on both the *combination* of their prior beliefs and the potentially *differential updating* of their beliefs based on the treatment information. To avoid this ambiguity regarding the interpretation of treatment-induced belief updating, we decided to exclude this treatment from our main analysis. Detailed results are available upon request.

Appendix F. Causal Forest Estimation for Treatment Effect Heterogeneity

Intuition of the Causal Forest Approach. In our study, we use a Causal Forest following Athey and Imbens (2016); Wager and Athey (2018); Athey and Wager (2019). A Causal Forest builds on the idea of a Random Forest using decision trees. In statistical terms, a decision tree is a hierarchical structure that recursively partitions data based on the most relevant features or attributes. Each decision branches into further subsets until a predefined number of partitioning decisions is reached. This process creates one decision tree, estimating heterogeneous treatment effects for each of the partitioned subgroups.

Despite each tree offering independent estimates, the strength of Causal Forest lies in synthesizing the information from different trees. While individual trees might exhibit variability or errors due to their specialization or limited view, the collective information from all trees helps create a more robust and balanced estimation of treatment effects across various subgroups. By aggregating the predictions from multiple trees, the Causal Forest reduces the emphasis on any single tree's findings and instead emphasizes areas where multiple trees converge or agree. This ensemble approach lessens the impact of chance associations or spurious findings that often arise in multiple comparisons, thereby offering a more robust estimation of treatment effects without inflating the risk of false discoveries. The essence of the analysis lies in this partitioning: it highlights which characteristics influence the impact of the treatment on different subgroups of the population, ultimately estimating a treatment effect for each individual conditional on its characteristics — the Conditional Average Treatment Effect (CATE).

Application of the Causal Forest. In our study, we implement the Causal Forest using the *grf* package by Tibshirani et al. (2018) to assess the heterogeneity of treatment effects driving our findings. Following the framework outlined by Athey and Wager (2019), we select available variables that potentially could drive treatment effect heterogeneity to construct decision trees. We choose the following variables to include in our analysis: Female, Migrant, Parent, Age, Education, Right-wing Voter, Left-wing Voter, Property Owner, Household Size, Region, Prior Enrollment, and Prior Response Rate (see Table D1).

Given the Causal Forest's requirement for non-missing observations, we impute missing values in the Prior Enrollment Rate and Prior Response Rate through predictive regressions on other covariates employed in the Causal Forest.[18] After handling missing data, our analysis contains 4,767 observations (due to missing values in the reform index).

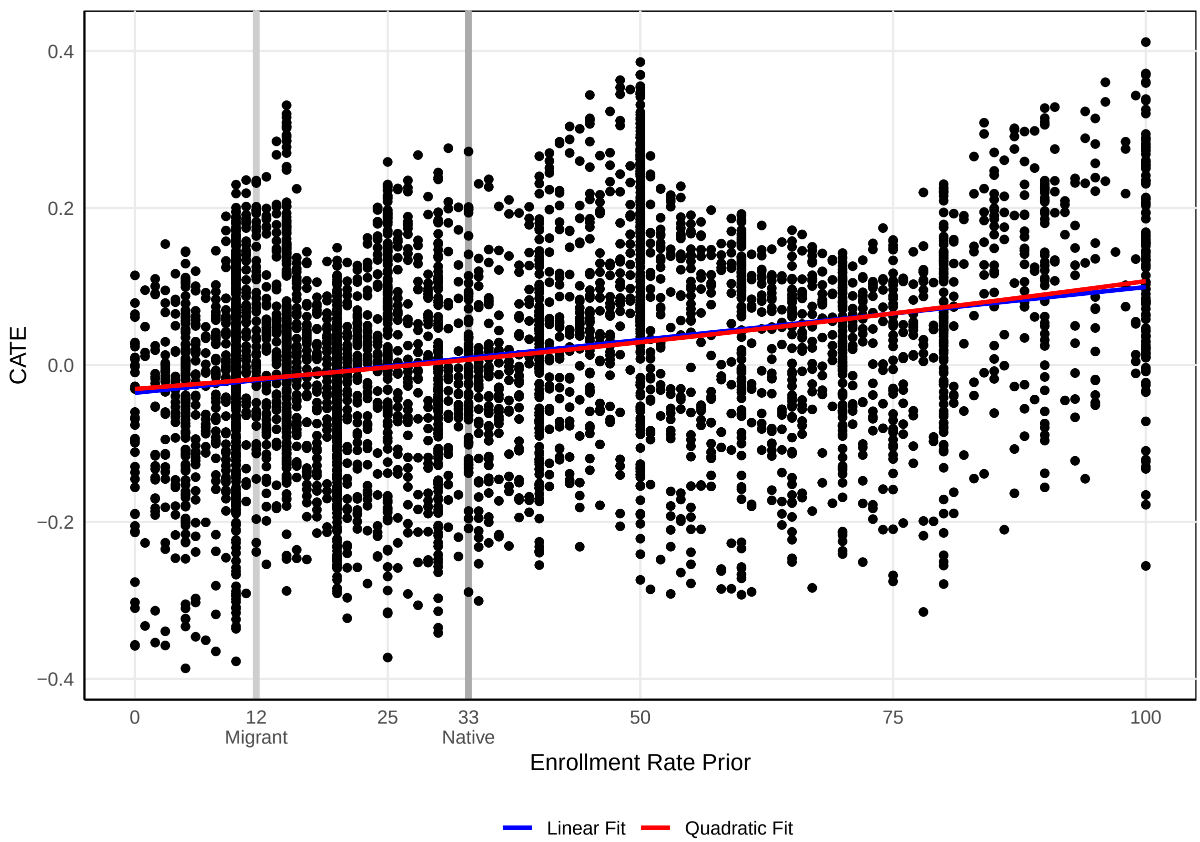
[18] We also conduct the analysis on 4,434 observations excluding observations with missing values and find qualitatively similar results.

We build a forest of 25,000 trees, incorporating the sampling weights to achieve a sample representative for the overall German population. Otherwise, we apply the recommended default settings of the *grf* package, and employ an "honest" approach by splitting the sample into equal halves for training and testing to prevent overfitting.

We find that the two variables with the by far highest importance for explaining the treatment effect heterogeneity are the prior beliefs about the enrollment rate and the response rate. For further investigation, we generate Figures 4 and F1, plotting the individual CATEs against the response rate and enrollment rate belief. In doing so, we investigate the functional form of treatment effect heterogeneity along these variables.

The CATEs for response rate beliefs (Figure 4) are presented and discussed in Section 5.3. For enrollment rate priors, we observe an increasing treatment effect for higher priors with a linear functional form (see Figure F1). In other words, the stronger respondents underestimate inequality in enrollment (i.e., the higher their enrollment rate prior), the more they upward-adjust their policy support upon learning the true enrollment rate. Further, for the respondents overestimating inequality in enrollment (i.e., those who underestimate the true enrollment rate for migrants), we see a (slightly) negative treatment effect on policy support. This pattern is consistent with underestimators (overestimators) learning that the problem of inequality in access to early child care is more (less) of a problem than they initially thought.

Figure F1: Scatter Plot of CATEs and Enrollment Rate Beliefs



Notes: Figure shows individual CATEs (y-axis) plotted against respondents' enrollment rate belief percentiles (x-axis). CATEs are the result of a Causal Forest with 25,000 trees as described above. The blue line is a fitted line minimizing mean squared errors. The red line is a quadratic fitted line minimizing mean squared errors. Vertical lines indicate the actual enrollment rates of migrants and natives, taken from Jessen et al. (2020).

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