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Educational Inequality and Public Policy Preferences: Evidence From Representative Survey Experiments

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Educational Inequality and Public Policy Preferences: Evidence from Representative Survey Experiments[*]

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Abstract

To study how information about educational inequality affects public concerns and policy preferences, we devise survey experiments in representative samples of the German population. Providing information about the extent of educational inequality strongly increases concerns about educational inequality but only slightly affects support for equity-oriented education policies, which is generally high. The small treatment effects are not due to respondents' failure to connect policies with educational inequality or aversion against government interventions. Support for compulsory preschool is the one policy with a strong positive information treatment effect, which is increased further by informing about policy effectiveness.

Keywords: inequality, education, information, survey experiment

JEL classification: D30, H52, I24, H11, D63, D83, D72, P16

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1. Introduction

Over the past decades, income and wealth inequality has increased in many industrialized countries (e.g., Piketty and Saez, 2014). The reasons for this trend are manifold, but increasing wage premia for higher education and cognitive skills seem to account for a large share of rising earnings inequality (Autor, 2014). At the same time, there is mounting evidence that factors outside of an individual's control determine educational achievement to a large extent. In particular, family background is a strong predictor of children's educational performance all over the world (e.g., Schuetz et al., 2008; Björklund and Salvanes, 2011; OECD, 2016). Since educational inequality has important implications for economic inequality and the inequality of opportunity (e.g., Nickell, 2004; Corak, 2013), education policies that attenuate the influence of family background have taken center stage in the political debate. One often given reason for supporting stronger education polices is the belief that equalizing education outcomes will lead to reduced income disparities and greater opportunities for children in poor families.

This paper investigates determinants of public preferences for education policies aimed at fostering equality of opportunity. Traditionally, governments try to mitigate inequalities in income and other economic outcomes through redistribution. Policies aimed at equality of *outcomes*, such as progressive taxation or minimum wages, might yield economic inefficiencies since they can distort labor supply and human capital accumulation decisions (e.g., Bovenberg and Jacobs, 2005). The trade-off between equity and efficiency hardly applies to policies aimed at equality of *opportunity*, which have the goal to detach the opportunity to turn effort into economic success from individual circumstances such as family background.[1,2] Consequently, economists have been advocating policies that equalize access to education in order to tackle income inequality (e.g., Alvaredo et al., 2018). But while a large strand of empirical literature has studied the public's preferences for policies aimed at equality of outcomes (e.g., Alesina and La Ferrara, 2005; Alesina and Giuliano, 2011; Kuziemko et al., 2015), the determinants of public preferences for – and thus, the political feasibility of – policies aimed at equality of opportunity are largely unexplored.

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[1] The central idea of the concept of equality of opportunity is that individuals should be compensated for deficits in circumstances which are beyond their control (e.g., family background, race, or gender) but not for differences deriving from effort to turn opportunities into actual advantages (e.g., Roemer, 1998). In a laboratory experiment, Cappelen et al. (2007) find that about 40 percent of participating university students exhibit preferences that can be classified as "strict egalitarians" (i.e., favoring equality of outcomes) and another roughly 40 percent as "liberal egalitarians" (i.e., favoring equality of opportunity).

[2] Of course, equity-oriented education policies might also have indirect effects on economic efficiency, e.g., when education policies are financed by taxes that affect efficiency. Arguably, though, these indirect effects are less severe than the immediate efficiency effects of traditional redistributive policies and may be overcompensated by positive efficiency effects of the educational investments.

We study how the German public's concerns about educational inequality and its preferences for equity-oriented education policies are affected by information about the extent of educational inequality. Given that the public often holds biased beliefs about the extent of inequality in society (e.g., Norton and Ariely, 2011), we focus on how information on actual educational inequality shapes public policy preferences. To this end, we conduct survey experiments among representative samples of the German voting-age population (N=7,380). In the experiments, randomly selected treatment groups are informed about the association between parents' socioeconomic status and their children's educational achievement before answering questions about concerns about educational inequality and preferences for a series of equity-oriented education policies. The control group answers the same questions without receiving information.

We find that a majority of the German public is concerned about the extent of educational inequality and that providing factual information about educational inequality increases these concerns even further. In the uninformed control group, 55.4 percent view educational inequality as a serious or very serious problem (as opposed to a medium problem or less on a five-point scale). Even from this high baseline level, information provision strongly increases concerns by 12.4 percentage points to 67.8 percent. The information effect, which we replicate in two independent and representative samples, varies with respondents' prior beliefs about the extent of educational inequality: The treatment has the largest effect on respondents who initially underestimated the extent of educational inequality and decreases with higher belief accuracy. This pattern is particularly pronounced among respondents who are relatively confident that their beliefs are correct, suggesting that the treatment effect is driven by genuine information updating, rather than priming or demand effects. Resurveying respondents in a follow-up survey about two weeks after the experiment, we find that the information effect on respondents' beliefs and concerns about educational inequality persists, further validating an interpretation of genuine information effects.

Going beyond concerns about educational inequality to preferences for equity-oriented education policies, we find that baseline support for many education policies aimed at reducing educational inequality is high. Focusing on policies that target equality of educational opportunity in the sense of preventing disadvantages that result from children's family circumstances (Coleman, 1975), we elicit preferences for eight equity-oriented education policies: providing free preschool for children from low-income families, introducing compulsory preschool, increasing government spending for schools with many disadvantaged students, postponing ability tracking, providing bonuses for teachers who teach in schools with

many disadvantaged students, introducing whole-day schooling for all students, teaching students with learning disabilities in regular classrooms, and increasing spending on need-based scholarships for disadvantaged university students. Among the control group, six of the eight policies have majority appeal, suggesting that implementing policies aimed at equality of opportunity in the education sector is politically feasible, even when the electorate holds biased beliefs about factual educational inequality.

In contrast to the effects on concerns about educational inequality, however, information treatment effects on these preferences for equity-oriented education policies are small. Informing participants about the extent of educational inequality raises a policy index that combines all eight policies by 2.4 percentage points (from a baseline support of 63.0 percent). While reaching statistical significance for the policy index (in particular when exploiting the full range of measured policy preferences from strong opposition to strong support), information treatment effects on the separate policy proposals are quantitatively small and mostly insignificant. The only exception is introducing compulsory preschool, where support increases by a strongly significant 5.7 percentage points (baseline 65.1 percent). Interestingly, making preschool compulsory is the one policy option that constrains families' choices – by prescribing preschool attendance – rather than just offering additional support. Our pattern of results resembles the earlier findings on public preferences for policies aimed at equality of outcomes by Kuziemko et al. (2015) who find that correcting biased beliefs about income inequality through information provision has large effects on concerns about inequality, but only little effect on tax and transfer policy preferences.

To better understand why the information treatment and the ensuing increased concerns about educational inequality do not translate into education policy preferences to a larger extent, we investigate three possible explanations. In a first additional experiment, we address the possibility that respondents may fail to connect their concerns about educational inequality with actual education policies. We test for the potential disconnect by explicitly informing a randomly chosen subgroup of the treatment group that the education policies are meant to reduce educational inequality. This information has no additional effect on respondents' policy support, indicating that disconnect between respondents' concerns and the education policies meant to address them does not account for the small treatment effects on policy preferences.

In a second additional experiment, we show that the lack of treatment effects on policy preferences is also unlikely to be due to the possibility that respondents may doubt the effectiveness of the proposed policies. Focusing on preferences for introducing compulsory preschool, treated respondents either receive information about the extent of educational

inequality (as in the main experiment), or information about recent scientific findings that preschool does decrease educational inequality, or both pieces of information. In comparison to the uninformed control group, information on educational inequality and on the equity-enhancing effect of preschool both significantly increase support for compulsory preschool, by 7.2 and 5.1 percentage points, respectively. Importantly, providing both pieces of information increases support by 12.6 percentage points, roughly the sum of the separate effects.[3] The additivity of treatment effects implies that the effectiveness information, while affecting policy preferences, does not alter the size of the treatment effect of informing about the extent of educational inequality. That is, the effect of information about educational inequality on policy preferences is unaffected by whether respondents doubt that the policy effectively mitigates educational inequality.

Our data also do not support a third possible explanation, namely that distrust towards the government or towards educational institutions accounts for the lack of information effects on policy preferences. Arguably, respondents who support the governing political parties have greater trust in the government, and teachers arguably have greater trust in the education system than the general population. In a complementary dataset, we show that these subgroups are indeed more satisfied with how schools teach children from disadvantaged backgrounds. Our subgroup analysis reveals that the information treatment does not have differential effects on supporters of the governing parties or on an oversample of teachers (N=713). That is, treatment effects do not depend on whether respondents have more or less trust in the government or in the effectiveness of educational institutions. Furthermore, if anything, treatment effects on policy preferences are larger for those respondents who do not prefer public school spending to increase, speaking against a role for aversion against increased government spending in explaining the small treatment effects on policy preferences.

Overall, our results suggest that preferences for education policies are hardly affected by correcting biased beliefs about the current extent of educational inequality, even though concerns about educational inequality are. The only exception is that being informed about educational inequality raises support for introducing compulsory preschool, a policy initiative that would commit parents to send their children to preschool. The fact that no such effects are found for policies in different areas that would simply increase funding for disadvantaged groups might suggest that respondents do not favor unconditional financial support that leaves disadvantaged groups' choice sets unchanged. This explanation is also consistent with the

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[3] This treatment effect remains significant in the follow-up survey about two weeks after the experiment.

finding that treatment effects on preferences for compulsory preschool are restricted to those who do *not* generally prefer government spending on schooling to increase.

Our results contribute to two strands of economics research. A large literature studies the determinants of public preferences for redistribution (see Clark and D'Ambrosio, 2015, for a recent overview). Among others, historical experience, culture, prospects of upward mobility, and socioeconomic background have been identified to shape redistributive preferences (e.g., Alesina and La Ferrara, 2005; Alesina and Guiliano, 2011; Luttmer and Singhal, 2011; Roth and Wohlfart, 2017). More recently, several papers have used large-scale survey experiments to investigate whether factual information about the extent of inequality affects preferences for redistribution (e.g., Cruces et al., 2013; Kuziemko et al., 2015; Bublitz, 2016; Karadja et al., 2017). These studies generally investigate policies aimed at equality of *outcomes*. Our focus on preferences for equity-oriented education policies extends this growing experimental literature to the dimension of policies aimed at equality of *opportunity*.[4] We are aware of only one other experimental and representative study, conducted contemporaneously to and independently of ours, that investigates preferences for policies aimed at equality of opportunity, focusing on beliefs about intergenerational mobility: Alesina et al. (2018) find that a pessimistic perception treatment on intergenerational mobility tends to increase support for policies aimed at equality of opportunity among left-wing respondents, but not among right-wing respondents. To the best of our knowledge, ours is the first paper that provides causal evidence on how information on factual educational inequality affects public concerns and preferences for various education policies, the very policies aimed at increasing equality of opportunity.

More generally, our analysis is related to the literature that studies the effects of education policies on educational inequality (for reviews of the literature, see, e.g., Woessmann, 2008; Björklund and Salvanes, 2011). For example, international evidence suggests that the extent of educational inequality is particularly large in Germany, our country of investigation, and that reduced educational inequality is associated with more extensive preschool education and with postponed between-school ability tracking (Schuetz et al., 2008). We add a political-economy dimension to this literature by studying the determinants of the electorate's support for these and other policies that might mitigate educational inequality.

The remainder of the paper is structured as follows. Section 2 introduces the opinion survey and the experimental design. Section 3 presents and discusses the results. Section 4 concludes.

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[4] Related strands of literature study fairness attitudes using laboratory experiments or vignette studies (see Roemer and Trannoy, 2015, for an overview).

2. Data and Empirical Strategy

This section describes the opinion survey, the experiments, and the econometric model.

2.1 The Opinion Survey

The research in this paper is based on two waves of the ifo Education Survey, an annual opinion survey on education policy that we conduct in Germany. The two waves of the survey were fielded by the leading German professional polling firm in the area of social sciences between April and June of 2016 and 2017, respectively. The sample covers a total of 7,380 respondents (3,302 in 2016 and 4,078 in 2017) who are representative for the German voting-age population (18 years and older). Respondents complete the survey on a computer device. Item non-response is very low at 1 percent on average, and in our experiments, treatment status does not predict non-response in the dependent variables of interest (see balancing tests in section 2.4).

To derive generalizable statements for the political economy of educational inequality, we conducted a mixed-mode survey which ensures our sample represents the German voting-age population as closely as possible.[5] The part of the population that uses the internet is sampled from an online panel and polled with an online survey. To account for the fact that a significant share of the German population (19 percent in 2016 and 17 percent in 2017) does not use the internet, these individuals are polled at their homes by trained interviewers. The interviewers provide these respondents with a tablet computer and ask them to complete the survey autonomously. In case the respondents require help with handling the device, the interviewers assist as much as needed. Throughout the paper, we employ survey weights that are calibrated to match official statistics with respect to age, gender, parental status, school degree, federal state, and municipality size. Inclusion of these weights does not substantially change the results of this paper. In the 2016 survey, we additionally surveyed an oversample of 713 school teachers because they constitute a key interest group in the politics of education policy (Peterson et al., 2014).

To investigate the persistence of treatment effects, we resurveyed 2,363 participants of the online part of the 2017 wave (64 percent) at a later point in time. The follow-up survey, which re-elicited some outcomes without providing any information treatment, was completed between 5 and 41 days after the main survey, with a median time lag of 12 days.

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[5] Representativeness is important because an understanding of the political economy typically requires obtaining preferences throughout the entire population. For instance, the distribution of preferences is crucial in the framework of median voter models (Meltzer and Richard, 1981).

Columns 1 and 4 of Table 1 present descriptive statistics for sociodemographic characteristics of the control group of the 2016 and 2017 survey wave, respectively. These characteristics include age, gender, migration background, city size, income, family status, parental education, own education, employment status, parent status, political party preference, voting behavior, and preference measures of risk tolerance and patience.[6]

2.2 The Survey Experiments

Even though many determinants of educational success are arguably outside the direct influence of policy makers, there is ample evidence that favorable institutional conditions can compensate at least part of the educational inequality that arises from individuals' family background. However, the political feasibility of equity-oriented education reforms requires that the electorate (i) recognizes that educational inequality is a problem and (ii) agrees on what corrective policies to implement. Since previous research shows that the public often underestimates the extent of societal inequality (e.g., Norton and Ariely, 2011), the electorates' ignorance of educational inequality might be an important obstacle to education reforms. We address these politico-economic determinants of education policy in our survey experiments. First, we randomly provide information on the actual extent of educational inequality to alleviate the electorates' ignorance about educational inequality. Second, we elicit respondents' concerns whether educational inequality is a problem. Third, we measure preferences for various education policies.

2.2.1 The Information Treatment

We conducted a survey experiment in both the 2016 and the 2017 waves of the ifo Education Survey that was designed to correct respondents' beliefs about the extent of educational inequality. Following the literature (e.g., Schuetz et al., 2008; Björklund and Salvanes, 2011), we define educational inequality as the relationship between children's educational achievement and their parents' socioeconomic status. Specifically, the randomized information treatment informs respondents that the gap in mathematics achievement between 15-year old children in the lowest and highest decile of family socioeconomic status is equivalent to about four years of learning.[7] Throughout the survey, respondents in the control

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[6] Risk tolerance and patience are elicited with experimentally validated survey questions on an eleven-point scale (see Falk et al., 2016).

[7] To calculate the achievement gap, we made use of data from the Program for International Student Assessment (PISA) conducted by the Organisation for Economic Co-operation and Development (OECD) in 2012. We used the PISA index of economic, social and cultural status (ESCS), a composite measure of home possessions including books at home, the highest parental occupation, and the highest parental education (see OECD, 2014a,

group answer the same questions as treated respondents, but they do not receive any information about educational inequality.

Respondents read the treatment information on a separate screen (depicted in Appendix Figure A1). The lower part of the screen shows a graphical depiction of the information, whereas the upper part presents the following information: "Numerous studies show that educational success in the early childhood, school, and university area strongly depends on which social background and family income circumstances the children and adolescents come from. For instance, an educational achievement study has shown that the mathematical achievement of 15-year-old students from difficult social backgrounds on average lags roughly 4 school years behind the mathematical achievement of those from good social backgrounds (comparison of the lowest and highest ten percent of social background in the population)." To avoid recall bias, the text information was provided to the treatment groups also on the following screens that elicited concerns about educational inequality and policy preferences.

To gauge respondents' information status at baseline, earlier on in the survey we elicited the prior beliefs of all participants about the extent of educational inequality in school-year equivalents.[8] We also asked how confident respondents were about the accuracy of their beliefs (from "1 very unsure" to "7 very sure").[9] These measures of respondents' prior beliefs allow us to investigate the channels through which the information treatment operates.

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pp. 351-354, for technical details). German children in the lowest decile of this index reached an average score of 445 points in mathematics and children in the highest decile 573 points (own calculations based on the PISA 2012 dataset). Since one year of learning is roughly equivalent to 30 PISA points, the difference amounts to about four school years. Measuring educational inequality as socioeconomic differences in PISA achievement scores has two major advantages. First, in contrast to attainment measures such as the college enrollment rate, educational achievement is largely independent from individual preferences for different educational degrees. This is particularly important in Germany, where a large apprenticeship sector offers a valued alternative to academic degrees (Lergetporer et al. 2018). Second, the PISA data are internationally comparable, which facilitates cross-country comparisons of educational inequality. In the public debate, differences in educational achievement are frequently expressed in terms of school-year equivalents. For instance, the New York Times recently published an interactive figure of achievement differences in school years by parental socio-economic status for the United States (see New York Times, 29 April 2016, https://www.nytimes.com/interactive/2016/04/29/upshot/money-race-and-success-how-your-school-district-compares.html [accessed 30 January 2018]). In section 3, we provide evidence that respondents indeed process and remember the information as intended.

[8] The wording of the question was as follows: "The next question concerns the comparison of educational success of children and adolescents with different social backgrounds and family income circumstances. What is your best guess, how much does the mathematical achievement of 15-year-old students from difficult social backgrounds on average lag behind the mathematical achievement of those from good social backgrounds? Think of a comparison of the lowest and highest ten percent of social background in the population. The difference is equivalent to an achievement lag of roughly … school years. (The answer "0" means that there is no difference.)"

[9] To make the correction of false beliefs less immediate and thus to reduce the possibility of backfire effects (see Nyhan and Reifler, 2010), belief elicitation was administered well before the survey experiment.

2.2.2 Eliciting Concerns about Educational Inequality

A necessary condition for advocacy of political reform is that the status quo, in this case the current extent of educational inequality, is perceived as problematic or dissatisfactory. Put differently, one should not expect any treatment effects on policy preferences if the provided information does not affect respondents' concerns about educational inequality.

We measure concerns for educational inequality by adapting a similar question on economic inequality from Kuziemko et al. (2015). Specifically, the question reads as follows: "What do you think, is the inequality of opportunities for children from different social backgrounds in the German education system a serious problem?" Respondents choose one of the following five answer categories: "not a problem at all", "a small problem", "a medium problem", "a serious problem," or "a very serious problem." We elicit these concerns in both survey waves (2016 and 2017).

2.2.3 Eliciting Preferences for Education Policies

Even if respondents agree that educational inequality is a problem, it is unclear ex ante which kind of policies they support in order to attenuate educational inequality. Therefore, we focus on a broad spectrum of education policies that may be aimed at increasing equality of opportunities by reducing the influence of family background on student achievement.[10]

We selected eight specific policies at three educational levels: preschool, school, and university. At the preschool level, we elicit preferences for (i) providing free preschool for children from low-income families and (ii) introducing compulsory preschool.[11] The policies at the school level include (iii) increasing government spending for schools with many disadvantaged students, (iv) postponing ability tracking from grade four to grade six,[12] (v) providing bonuses for teachers who teach in schools with many disadvantaged students, (vi)

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[10] Identifying such policies is not straightforward. For many policies aimed at equality of opportunity, such as introducing compulsory preschool, the link between policy and outcome is quite indirect. In contrast, policies aimed at equality of outcomes such as progressive taxation or estate taxes are usually closely related to the outcome that they address; e.g., progressive income taxes aim at generating more equality in income. This is not to say, however, that the distributional consequences of policies aimed at reducing economic inequality are always clear-cut. A case in point is the uncertainty surrounding the distributional consequences of minimum wage regulations (see, e.g., Autor et al., 2016).

[11] A unified perspective on life-cycle skill formation (e.g., Cunha et al., 2006) suggests that early childhood education programs, particularly those targeted at disadvantaged children, have strong potential for mitigating educational inequality. Cornelissen et al. (2018) and Felfe and Lalive (2018) provide recent evidence for the equity-enhancing effects of universal childcare in Germany.

[12] Hanushek and Woessmann (2006), Schuetz et al. (2008), and Piopiunik (2014) provide evidence on the equity-enhancing effect of later tracking; see Pekkarinen (2014) for a review.

introducing whole-day schooling until 4 pm for all students,[13] and (vii) teaching students with learning disabilities together with students without learning disabilities in regular classrooms. Finally, at the university level, we include the proposal to (viii) extend public scholarship programs to support low-income university students.[14]

While the evidence base for the equality implications of these different policies varies, all of the policies have been discussed as political responses to educational inequality.[15] Respondents state whether they "strongly favor", "somewhat favor", "neither favor nor oppose", "somewhat oppose," or "strongly oppose" each policy. These policy preferences were elicited in the 2016 wave of the ifo Education Survey.

2.2.4 Additional Experiments

Our hypothesis to be tested is that increased concerns about educational inequality lead to higher support for equity-oriented education policies. However, earlier evidence suggests several reasons for why treatment effects on concerns might not be sufficient for shifting policy preferences. We therefore extended our basic experimental design to address two such reasons.

The first possible explanation for a lack of information treatment effects on policy preferences is that respondents might not connect their concerns about inequality with the policies meant to address them (e.g., Bartles, 2005; Kuziemko et al., 2015). To test the relevance of this channel in our setting, we randomly split respondents in the treatment group of the 2016 wave into two subgroups before eliciting their policy preferences. The first subgroup is simply reminded about the extent of educational inequality when evaluating the policies. The second subgroup receives additional information to bridge the potential disconnect between inequality concerns and policies. The additionally provided information reads as follows: "The following reform proposals frequently have the goal to increase the equality of opportunity in the education system." Comparing policy preferences between the two treatment subgroups sheds light on whether the disconnect hypothesis is relevant in our setting.[16]

The second potential reason is that, even if respondents appreciate that the education policies are meant to address educational inequality, they might doubt their effectiveness in

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[13] An argument for expanding whole-day schools is that they improve the quality of afternoon activities for children from disadvantaged backgrounds and therefore equalize opportunity (e.g., Blau and Currie, 2006).

[14] See, e.g., Dynarski (2003), Fack and Grenet (2015), and Angrist et al. (2016) for evidence that student aid affects college attendance and completion.

[15] While the list of equity-oriented policies investigated in this paper is by no means exhaustive, they include some of the most common proposals to increase equality of opportunities in Germany.

[16] Note that the two treatments are identical in all preceding stages of the experiment, i.e., belief elicitation, information provision, and elicitation of concerns.

doing so. Such doubts might exist because the mechanisms through which education policies affect inequality of educational opportunity are often not particularly obvious. We conducted an additional experiment within the 2017 wave of the ifo Education Survey to assess whether doubts about policy effectiveness attenuate information treatment effects on policy preferences. Focusing on preferences for introducing compulsory preschool, we provide three randomly selected treatment groups with different pieces of information before eliciting policy support in the same way as in the uninformed control group. The first treatment again informs about the extent of educational inequality. Respondents in the second treatment group are informed that "A recent study shows that preschool participation strongly improves the later opportunities of children from difficult social backgrounds. At the same time, particularly these children are less often enrolled in a preschool by their parents." This information is based on the evidence of effects of preschool attendance in Germany presented in Cornelissen et al. (2018). The third treatment provides both pieces of information together. Comparing preferences for compulsory preschool across treatments reveals the complementarity of information on educational inequality and on policy effectiveness in shaping public policy preferences.[17]

2.3 Econometric Model

Because of the random assignment of participants to control and treatment groups, we can use the following basic regression model to estimate the causal effect of the information treatment:

$$y_i = \alpha_0 + \alpha_1 \text{Treatment}_i + \delta' X_i + \varepsilon_i \tag{1}$$

where y_i is the outcome of interest for individual i, $Treatment_i$ is an indicator of whether individual i received the information treatment, X_i is a vector of control variables, and ε_i is an error term. The average treatment effect, estimated as coefficient α_1, is identified because of the random assignment of treatment status. Therefore, adding control variables, X_i, should not alter the estimates of the treatment effect, though it might increase precision. Thus, we present estimation results with and without additional covariates.

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[17] Again, we elicited respondents' concerns about educational inequality prior to the experiment on policy preferences. Respondents in the information treatment of the experiment on concerns were randomly assigned to the simple treatment or the combined treatment of information and effectiveness in the experiment on policy preferences. Similarly, respondents from the control group in the experiment on concerns were randomly assigned to the control group or to the effectiveness treatment. This contingent randomization facilitates clean identification of the effects of inequality information versus effectiveness information on support for compulsory preschool. Also note that the separate presentation of the question for eliciting preferences for compulsory preschool in the 2017 wave differed from the presentation as part of a list of policies in the 2016 wave.

To analyze heterogeneities in treatment effects across subgroups of respondents, we extend our basic regression model to:

$$y_i = \beta_0 + \beta_1 \text{Treatment}_i + \beta_2 \text{Subgroup}_i + \beta_3 \text{Treatment}_i * \text{Subgroup}_i + \delta' X_i + \eta_i \quad (2)$$

where *Subgroup$_i$* equals one if respondent *i* is member of the respective subgroup and zero otherwise. In this specification, the effect of information provision for the baseline group is given by β_1, and β_3 measures the additional effect for the respective subgroup.

2.4 Test of Randomization

To test whether the randomization successfully balanced respondents' observable characteristics across the control and treatment groups, we investigate whether covariates differ across experimental groups. Columns 2 and 3 of Table 1 report differences between the control group and the treatment groups in the 2016 survey, and columns 5 to 7 for the 2017 survey, as the coefficients γ_1 of the following regression model:

$$\text{Covariate}_i = \gamma_0 + \gamma_1 \text{Treatment}_i + \varepsilon_i \quad (3)$$

We estimate this regression for each of the treatment groups and each covariate separately in both survey years. It is reassuring that only eight of the 150 regressions yield a coefficient γ_1 that is significant at the 5 percent level, which would be expected by pure chance. In addition, as indicated at the bottom of Table 1, item non-response is independent of treatment status, which indicates that our results are not driven by non-random survey attrition. In sum, the balancing tests suggest that random assignment worked as intended.[18]

3. Results

We present three sets of results. First, we analyze how information on the extent of educational inequality affects the public's concerns about the issue. Second, we investigate how this information shapes public support for equity-oriented education policies. Third, we provide analyses of three potential explanations for the small information treatment effects on policy preferences.

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[18] For ease of exposition, Table 1 displays covariate balance only across the treatments of the experiments on education policy preferences (which are nested in the treatments of the concern experiment; see section 2.2.4 for details). Covariates are also balanced in the concern experiment: only six out of 60 pairwise comparisons between the control group and information treatment group are significant at the 5 percent level (results available upon request).

3.1 Information Provision and Concerns about Educational Inequality

In presenting the effect of information provision on the extent to which respondents view educational inequality as a problem, we start with baseline results, followed by analyses of heterogeneous treatment effects by prior beliefs about the provided information and of persistence of the information treatment effects in the follow-up survey.

3.1.1 Experimental Results

Table 2 reports the causal effect of providing information about the current extent of educational inequality on respondents' concerns about educational inequality. The estimates are based on equation (1) and use stacked data from both survey waves.[19] Odd-numbered columns show the unconditional regressions, even-numbered columns include a set of covariates.[20] For comparison, the reported control mean refers to the mean of the outcome variable in the uninformed control group.[21]

As it turns out, the majority of respondents in the control group – 55.4 percent – perceives unequal educational opportunity for children from different social backgrounds as a serious or very serious problem. Only 13.8 percent think it is no problem or a small problem (with the remaining category of the five-point scale referring to a medium problem).[22] Thus, a majority of the public seems to be aware that the German education system provides unequal opportunities and perceives this situation as dissatisfactory. It is noteworthy that concerns are particularly pronounced among frequent voters and among those who consider education topics important for their vote choice (see Appendix Table A1).[23] Partisans of the conservative party

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[19] About 12 percent of respondents participated in both survey waves. Throughout our analysis of stacked data, standard errors are clustered at the individual level. Excluding these respondents does not alter our results (results available upon request).

[20] The set of covariates includes respondents' age, gender, migration background, education, income, employment status, partner in household, parent status, city size, parental education, political party preference, voting behavior, risk tolerance, and patience.

[21] While all models in this paper are estimated as linear probability models, (ordered) probit models yield qualitatively identical results (results available upon request).

[22] Interestingly, these numbers closely resemble the German public's concerns about inequality in general. Bublitz (2016) finds that 61 percent of the German population consider inequality a (very) serious problem and 14 percent think that it is no or a small problem. We are grateful to the author for providing us with this particular information.

[23] Appendix Table A1 presents regressions of perceiving educational inequality as a problem on sociodemographic characteristics in the control group. Older respondents and those living in large cities are more concerned about educational inequality. The track of attended school also turns out to be a predictor. These findings are consistent with previous studies on economic inequality that find that personal history predicts attitudes towards redistribution (e.g., Alesina and Giuliano, 2011). Interestingly, own and parental university backgrounds do not predict concerns about educational inequality, and the same is true for income, employment, and parental status. Respondents' patience is positively associated with concerns.

(CDU/CSU) express less concern about educational inequality being a problem. These associations corroborate the relevance of educational inequality for the political-economy process.

The information treatment on the extent of educational inequality has a large and highly significant effect on respondents' expressed concerns about educational inequality. Columns 1 and 2 of Table 2 show that information on educational inequality increases the share of those viewing educational inequality as a (very) serious problem by 12 percentage points. Conversely, the share of respondents who think that it is no or a small problem decreases by 5 percentage points (columns 3 and 4). As expected, the inclusion of covariates does not affect the qualitative results. Furthermore, the treatment effect is insensitive to the coding of the outcome variable: The effect remains large and highly significant if concerns are treated as a continuous five-point measure or if a separate coefficient is estimated for each answer category (Appendix Table A2). The insignificant coefficient on the interaction term between the information treatment indicator and a dummy for the 2017 survey wave in Appendix Table A3 shows that treatment effects are very similar across the two survey waves. Given the recent emphasis in the economics literature on replication to avoid false positive results (e.g., Maniadis et al., 2014), we consider the fact that the treatment effect is prevalent in two independent and representative samples particularly reassuring.

Inspection of treatment effects by subpopulations does not indicate substantial effect heterogeneity (not shown). While respondents with left-leaning political preferences are significantly more likely to perceive educational inequality as a serious problem, the size of the information treatment effect does not differ significantly between respondents with left-leaning and right-leaning political preferences.[24] This is in contrast to the finding of Alesina et al. (2018) whose perception treatment affects the concerns about unequal opportunity of left-leaning, but not right-leaning respondents in a five-country sample. Interestingly, concerns about educational inequality do not differ significantly between respondents with above-median and below-median income, and treatment effects do not differ substantially.[25] Similarly, concerns

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[24] Left-leaning political preferences are measured as indicating a preference for SPD, Grüne, or Linke on the question, "Many people in Germany tend to vote for a particular political party, even if they sometimes vote for another party. In general, with which party do you agree most?"

[25] In fact, the treatment effect on viewing educational inequality as a serious problem is marginally significantly larger (by 5.0 percentage points) among high-income respondents, but the treatment effects do not differ significantly by income when the outcome is measured as a categorical variable on a five-point scale or as viewing educational inequality as a small problem at best.

and treatment effects do not differ between respondents with and without a university entrance degree (*Abitur*).

In sum, providing information on the actual extent of educational inequality has a large and positive effect on expressed concerns about educational inequality. This suggests that, while the majority of respondents in the uninformed control group is concerned about educational inequality, respondents' concerns are based on overoptimistic beliefs about the actual extent of educational inequality. To shed light on the role of belief updating, we next investigate treatment effect heterogeneities by respondents' prior beliefs.

3.1.2 Heterogeneous Treatment Effects by Prior Beliefs

One potential concern with the experimental results presented above is that the information treatment effect could reflect priming or experimenter demand effects rather than genuine information updating. To explore this possibility, we elicited respondents' prior beliefs about educational inequality early in the survey. In this section, we first present evidence on the public's ignorance about educational inequality and then estimate whether the information treatment effect varies with respondents' prior beliefs, i.e., with their information status at baseline.

Respondents severely underestimate the extent of educational inequality. The modal belief is that 15-year-old children from difficult and good social backgrounds differ in their achievement by an equivalent of two school years of learning (see Appendix Figure A2). The vast majority of respondents (84 percent) underestimate educational inequality in Germany, and only 5 percent correctly estimate that the achievement gap amounts to the equivalent of four school years. This finding is consistent with the large treatment effect on concerns for educational inequality in the previous section, suggesting that the average respondent was informed by the treatment that educational inequality is more pronounced than she had believed in advance.

To analyze whether treatment effects systematically vary by respondents' prior beliefs, we estimate regressions based on equation (2) that interact the treatment indicator with a continuous measure of belief accuracy. Belief accuracy is measured in relative terms as respondents' stated belief about the achievement difference divided by the actual difference of four school years. Table 3 and Figure 1 display the key finding. The figure plots the linear estimate of how the probability that a respondent is concerned about educational inequality depends on her prior belief, separately for the control group and for the treatment group (see column 1 of Table 3). The positive slope of both lines in Figure 1 reflects the intuitive result

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that those who estimate higher levels of educational inequality are more likely to view it as a problem. The difference between the two lines shows the size of the information treatment effect for different prior beliefs.

The treatment effect is largest for respondents whose prior belief was that educational inequality is small. These respondents learn that actual inequality is higher than they previously thought, which leads them to be more concerned about educational inequality. For individuals with correct beliefs, the treatment effect is much smaller and statistically insignificant for the few respondents who overestimate the extent of educational inequality. This pattern of effect heterogeneities by prior beliefs suggests that the information treatment effect on respondents' concerns operates largely through genuine information-based updating, as opposed to effects such as priming or demand effects.

In addition, this pattern of results is mostly driven by respondents who were relatively confident about the accuracy of their prior beliefs. Appendix Figure A3 depicts treatment effects separately for those who were relatively confident about their beliefs (left panel) and those who were relatively unconfident (right panel) (see columns 2 and 3 of Table 3).[26] Intuitively, the pattern of heterogeneous treatment effects by prior beliefs is particularly pronounced among those who were confident in their beliefs. This result is in line with the above interpretation that the treatment operates through updating of – confidently held – false beliefs.[27]

3.1.3 Persistence of Information Treatment Effects

To investigate whether the effect persists beyond the immediate survey horizon, we resurveyed the online sample of the 2017 wave of the ifo Education Survey about two weeks after the main survey. The follow-up survey re-elicits respondents' beliefs about the extent of educational inequality and their concerns about the issue, but does not contain any information treatment.

Participation in the follow-up survey is high: 2,363 of the 3,696 online respondents (64 percent) participated again. Appendix Table A4 shows that participation in the follow-up survey is unrelated to whether respondents received the information treatment in the main survey. Similarly, covariates of the follow-up sample are balanced across experimental groups (see

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[26] Respondents who indicate a value of confidence between 5 and 7 on the scale from 1 ("very unsure") to 7 ("very sure") are classified as confident (28 percent). As expected, belief accuracy and confidence are positively correlated (results available upon request).

[27] The fact that treatment effects vary by respondents' confidence also underlines the importance of distinguishing between "misinformation" (i.e., respondents confidently holding false beliefs) and "uninformedness" (i.e., respondents stating a random guess) when analyzing belief updating (see Kuklinski et al., 2000).

Appendix Table A5). Thus, non-random selection into follow-up survey participation does not bias our estimates of treatment effect persistence.

Table 4 shows the effects of information provision during the main survey on beliefs and concerns about the extent of educational inequality in the follow-up survey. The information treatment significantly increases respondents' beliefs about the achievement gap between children from difficult and good social backgrounds (column 1). Given that respondents initially underestimated the actual gap of four school years, the positive treatment effect implies that information provision persistently improves beliefs. Consistently, the information treatment increases the confidence with which respondents hold their beliefs in the follow-up survey (column 2).

Importantly, the treatment effect on concerns also persists. Information provision in the main survey significantly increases the share of those who think that educational inequality is a (very) serious problem in the follow-up survey (column 3). At 5.7 percentage points rather than 12.0 percentage points, this effect is smaller in magnitude than the immediate treatment effect, but still substantial and highly significant.

In sum, the information treatment in the main survey leads to persistent updating of beliefs and concerns about educational inequality in the follow-up survey. This implies that participants indeed understand and remember the provided information. Furthermore, this persistence makes it highly unlikely that our strong treatment effect in the main survey is driven by demand effects or priming effects, as those are unlikely to persist over two weeks.[28]

3.2 Information Provision and Public Policy Preferences

Next, we investigate whether the information provision that increased concerns about educational inequality also has a causal impact on public preferences for education policies that aim to increase equality of opportunity in the education system. We start by investigating the correlation between concerns and policy preferences and then present our experimental estimates.

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[28] This interpretation is in line with previous studies that take information treatment effects in follow-up surveys as evidence for genuine information effects, net of priming or demand effects (e.g., Haaland and Roth, 2017, Cavallo et al., 2017). Furthermore, Mummolo and Peterson (2018) show that survey experiments are robust to experimenter demand effects, and de Quidt et al. (2018) provide evidence from online experiments that respondents' economic preference measures are hardly affected by experimenter demand effects.

3.2.1 The Association between Concerns and Policy Preferences

Consistent with the high level of concern about educational inequality in the control group, the different equity-oriented education policies are popular with the public. Among the eight considered policies, only the introduction of bonuses for teachers in disadvantaged schools and whole-day schooling do not have majority support (see the control-group means reported in Table 5). This high level of support for education policies is consistent with previous papers showing that policies aimed at equality of opportunity are relatively popular, in particular compared to policies aimed at equality of outcomes (e.g., Alesina et al., 2018).

The preferences for equity-oriented education policies are closely associated with concerns about educational inequality. Table 5 shows regressions of policy preferences on concerns in the control group. The dependent variables in columns 2 to 9 are dummies coded one if the respondent (strongly) favors the respective policy, and zero otherwise. The policy index in column 1 is the mean of these outcome variables. Across all policies, support is 12 percentage points higher if respondents consider educational inequality a (very) serious problem (column 1). This correlation is significant for seven out of the eight individual policies. The only exception is the proposal to provide bonuses for teachers who teach in schools with many disadvantaged students (column 6), which might emerge from the fact that increases in teacher salary are generally unpopular with the German public (see West et al., 2016) or because respondents who are concerned about inequality might be more skeptical towards bonus policies.

3.2.2 Experimental Results

Despite the large information treatment effects on concerns and the strong association between concerns and policy preferences, we do not find strong effects of the information treatment on the policy preferences. Table 6 presents regressions of support for the different education policies on treatment indicators based on equation (1). On average across the eight policies, providing information about the extent of educational inequality increases support for equity-oriented education policies by a marginally significant 2.4 percentage points (from a baseline support of 63.0 percent, column 1). Among the eight individual policies, the only (marginally) significant treatment effect exists for the proposal to introduce compulsory preschool, where support is increased by 4.2 percentage points (baseline 64.3 percent, column 3). While estimates for all other policies also point in the positive direction, none reaches statistical significance, and most are very small.

We can exploit variation beyond the population shares that support the respective policies by measuring policy preferences as continuous five-point measures. As shown in Table 7, precision increases in this specification, with estimates of information treatment effects reaching statistical significance at the 5 percent level for the policy index and at the 1 percent level for preferences for compulsory preschool. In addition, the estimates for spending for disadvantaged schools, later tracking, and whole-day schooling reach marginal significance in this specification. Still, with the exception of compulsory preschool, all these estimates are very small. For the policy index, the average marginal effect of going from one category to the next on the five-point scale is 1.9 percentage points, even smaller than the effect on the share of policy supporters (both estimated by linear probability models). On the five-point measure, the provided information increases the policy index from 3.61 to 3.68.[29] The one exception with a noteworthy effect is again compulsory preschool, where the average marginal effect for the five-point measure equals the one for the support share.

In the 2017 wave, we asked again about preferences for compulsory preschool (but not for the other policies). The first four columns of Table 8 replicate the significant effect of informing about educational inequality on support for compulsory preschool in the 2017 wave. The effect is slightly larger in the replication, but not statistically significantly so.[30] Thus, in the pooled sample, the information treatment increases support for compulsory preschool by a highly significant 5.7 percentage points (column 4).

One feature that distinguishes the introduction of compulsory preschool from the other policy proposals is that it constrains families' choices, requiring all of them to send their children to preschool. There are basically no treatment effects in Tables 6 and 7 for policy proposals that would provide unconditional financial support to disadvantaged groups without such requirements – free preschool for low-income children, additional spending for disadvantaged schools, bonuses for teachers at disadvantaged schools, and need-based scholarships. The two largest estimates apart from compulsory preschool, with marginally significant effects on the five-point measure – later tracking and whole-day schooling for all students – are also policies that are not targeted at disadvantaged groups. The final policy proposal without evidence of treatment effects, coeducation of children with and without

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[29] Computing the index as the equally weighted average of each policy's *z*-score (Kling et al., 2007), the information treatment increases the policy index by 6.3 percent of a standard deviation (results available upon request).

[30] The difference in treatment effect sizes might reflect that the question was presented on its own in the 2017 survey, whereas it was part of an item battery of policy proposals in the 2016 survey.

learning disability, does in fact target a different dimension of inequality (disability) than the one addressed by the provided information (social background).

Again, we do not find strong evidence of heterogeneous treatment effects by subpopulations (not shown). In particular, while respondents with above-median income tend to show significantly lower support for equity-oriented education policies on average (their policy index is 0.11 lower on the five-point measure),[31] the information treatment effects do not differ significantly between respondents with above-median and below-median income. The one exception is that the treatment effect on whole-day schooling is significantly larger for respondents with above-median income. Similarly, respondents with a university entrance diploma are less supportive of equity-oriented education policies than respondents without (0.12 lower policy index, significant for each policy except free preschool, teacher bonuses, and need-based scholarships), but information treatment effects do not differ significantly by educational attainment. Finally, respondents with left-leaning political preferences have significantly higher support for equity-oriented education policies on average (0.20 higher policy index, significant for each individual policy except compulsory preschool and teacher bonuses), but again treatment effects are homogeneous with respect to political preferences.[32]

Given the overall small effects of information provision on policy preferences, we next explore three potential explanations for why experimentally increased concerns about educational inequality fail to translate into higher support for education policy preferences.

3.3 Investigating Explanations for the Small Treatment Effects on Policy Preferences

In this section, we test three potential explanations for the limited information treatment effects on policy preferences. First, we investigate the role of a potential disconnect between respondents' concerns about educational inequality and education policies. Second, we test whether respondents' doubts that the policies are effective in mitigating educational inequality can rationalize our findings. Third, we assess the role of respondents' low trust in educational institutions or in the government.[33]

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[31] Among the individual policy proposals, respondents with above-median income show significantly lower support for compulsory preschool, bonuses for teachers in disadvantaged schools, and whole-day schooling.

[32] The interaction between information treatment and left-leaning political preferences does not reach statistical significance for the policy index or any of the individual policies. There is some indication of a positive interaction for compulsory preschool in the 2016 wave, but this does not carry through to the 2017 wave or to the pooled analysis of the 2016 and 2017 waves.

[33] Of course, there might be other explanations for the small treatment effects on policy preferences, and we do not claim that the subsequent analysis is exhaustive. Importantly, the insignificant treatment effects are not due to a lack of statistical power. For instance, our sample size allows us to detect treatment effects of three percentage points on the policy index (with $\alpha=0.05$ and power=0.80).

3.3.1 Disconnect between Concerns and Education Policies

Previous research on preferences for policies aimed at equality of outcomes has argued that, while the public might be concerned about inequality, it is ignorant about the distributional consequences of different public policies such as tax reforms (e.g. Bartels, 2005). A priori, it seems that this disconnect between concerns and policy preferences might be even more pronounced for policies aimed at equality of opportunity, because the effect of educational policies on differences in the education achievement of students from different backgrounds is often relatively indirect. To test whether this can explain our limited treatment effects, we explicitly inform a random subgroup of the treatment group that the policies they evaluate frequently have the goal to increase the equality of opportunity in the education system.

This information about the connection between the proposed policies and educational inequality has no additional effect on respondents' policy preferences. The second row in Tables 6 and 7 shows the additional effect of the connection information, over and above the information about the current extent of educational inequality. The only significant coefficient in column 9 shows that informing about the connection actually decreases support for need-based scholarships compared to only informing about the extent of inequality. However, the combined effect of both pieces of information compared to the uninformed control group is not significantly different from zero also in this case.

In sum, these experimental results suggest that respondents' failure to connect their concerns about educational inequality with education policies does not explain the small information treatment effects on policy preferences.

3.3.2 Doubts about Policy Effectiveness

Even if respondents are aware that the policy proposals are meant to address educational inequality, they might be skeptical about the effectiveness of the policies. Therefore, doubts about policy effectiveness might be another potential reason for why increased concerns about educational inequality fail to translate into policy preferences. To test this possibility, in the 2017 wave we enact another experiment that provides information to respondents about recent scientific evidence on the equity-enhancing effects of universal child care (Cornelissen et al., 2018).

Columns 5-7 of Table 8 show that, just like the information treatment on the extent of educational inequality, being informed about the effectiveness of preschool participation also significantly increases support for the introduction of compulsory preschool. Being informed about a study showing that preschool participation strongly improves opportunities of children

from difficult social backgrounds, who are less likely to enroll in preschool, significantly increases support for compulsory preschool by 5.1 percentage points.[34] The estimates of the two experimental treatments do not differ significantly from one another.

Providing both pieces of information – about current educational inequality and about policy effectiveness – in a combined treatment yields a significant and large increase in policy support by 12.6 percentage points. This treatment effect is significantly larger than the separate effects of informing about educational inequality and of informing about policy effectiveness, respectively. At the same time, it is quantitatively and statistically indistinguishable from the sum of the two separate treatment effects. This result implies that information about the extent of educational inequality and about policy effectiveness are complements in shaping policy preferences. Put differently, informing about the extent of educational inequality does not have a larger effect on policy preferences if respondents are also informed that the proposed policy successfully alleviates inequality.

The combined treatment effect of informing about both educational inequality and policy effectiveness on policy preferences for compulsory preschool actually persists in the follow-up survey. While smaller than the immediate effect, Appendix Table A6 shows that support for compulsory preschool is significantly larger about two weeks after the experimental treatment in the main survey in the treatment group that had received the combined information.[35]

Overall, we find that while respondents' doubts about whether education policies effectively mitigate educational inequality might be an important determinant of policy preferences, they do not seem to be a reason for the limited information treatment effects on policy preferences in the previous section.

3.3.3 Low Trust in Educational Institutions or the Government

A third potential explanation for the limited responsiveness of education policy preferences to information about educational inequality might be that respondents mistrust the education system or the government in general to alleviate educational inequality.[36] While we do not have

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[34] Note that the effectiveness treatment has two aspects. First, it provides respondents with a better understanding of how compulsory preschool would mitigate differences in outcomes for children from different social backgrounds. Second, the treatment cites scientific evidence that support the equity-enhancing effects of the policy proposal (similar to, for instance, Elias et al., 2015, Haaland and Roth, 2017, and Werner, 2018).

[35] See Appendix Table A4 (column 2) and Appendix Table A5 for evidence that non-random selection into the follow-up survey does not drive this result.

[36] Note that the government in Germany is heavily engaged in the education sector. Education is the third largest category of government expenditure (after social protection and health care; see OECD, 2017, p. 77), and the public school system enrolls more than 90 percent of all students (96 percent in primary school, 91 percent in lower secondary, and 92 percent in upper secondary school; see OECD, 2014b, p. 416).

a direct measure for respondents' trust, we explore this channel by presenting heterogeneous treatment effect estimates for an oversample of teachers (N=713) and for partisans of the government parties. While a third of respondents state that they do not favor any political party, about a fifth of respondents each indicate that they generally agree with one of the two parties currently in government, CDU/CSU and SPD. If respondents who favor one of the governing parties have greater trust in government, the heterogeneity of treatment effects with regard to party preferences allows us to test whether distrust in government is a potential explanation for the lack of treatment effects. Similarly, if teachers as employees of the education system have more trust in the education system than the general population, we again would expect heterogeneities in treatment effects if trust in educational institutions was a driving factor for information treatment effects.

Complementary evidence indicates that teachers and partisans of the governing parties are indeed more satisfied with how schools teach children from disadvantaged backgrounds. In the 2014 wave of the ifo Education Survey, we asked respondents how they would grade the public schools for attending to the needs of students from high-income and low-income families. Grades are generally better for attending to the needs of high-income students. Importantly, respondents who work in the education sector, as well as those who support the governing parties, are significantly more likely to give schools one of the two top grades for their efforts in attending to the needs of low-income students. This corroborates the assumption that these subgroups are more trusting that public schools can alleviate educational inequality.

Table 9 reports estimates of heterogeneous treatment effects on policy preferences for the subgroups of teachers and governing-party supporters based on equation (2). As is evident from the mostly insignificant coefficients on the interaction terms in panels A and B, the information treatment does not have heterogeneous effects on teachers or on supporters of the governing parties. Thus, our descriptive analysis does not support the notion that respondents' trust in educational institutions or in the government mediates treatment effects on policy preferences. This is in contrast to Kuziemko et al. (2015) who find that small information treatment effects on redistributive policy preferences in the United States can partially be explained by respondents' low trust in the government. The fact that this result is not born out in our analysis is consistent with the fact that trust in government is generally much higher in Germany than it is in the United States.[37]

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[37] In Germany, 55 percent of the population state to have confidence in the national government, whereas only 30 percent of the United States population do so (OECD, 2017, p. 215).

As some of the proposed policies would require additional public spending, a related possible reason for the small treatment effects on policy preferences might be respondents' aversion to increases in education spending. Panel C of Table 9 presents estimates of heterogeneous treatment effects for respondents who do and do not support increases in public school spending.[38] Contrary to expectations, the coefficient on the interaction term is marginally significantly negative, suggesting that the information treatment might have slightly smaller effects for respondents who support increases in education spending. This is driven by heterogeneities in the treatment effect on preferences for compulsory preschool, where the information increases support for the introduction of this policy by 12.4 percentage points among respondents who do not support increases in school spending. Again, we find no evidence to suggest that the effects of the information treatment are small because respondents are concerned about increases in public education spending. Quite to the contrary, the fact that the effect on compulsory preschool is restricted to the subgroup of those who do not support additional spending is consistent with the interpretation that the particularity of compulsory preschool is that it does not provide unconditional financial support.

4. Conclusion

Unequal educational opportunity for children from different social backgrounds is a key determinant of persistent economic inequality in society. But in contrast to public preferences for redistribution through policies aimed at equality of *outcomes*, little is known about the determinants of preferences for equity-oriented education policies. We administered representative survey experiments in Germany, a country with substantial inequality of educational opportunity, to study the public's concerns about educational inequality and preferences for educational policies aimed at equality of *opportunity*.

While the majority of the German public is concerned about educational inequality, respondents underestimate its actual extent. Correcting these biased beliefs through randomized information provision has a large, replicable, and persistent effect on concerns about educational inequality. There is also evidence that the provided information increases support for education policies, but the effects are mostly quantitatively small. The one exception is a substantial treatment effect on support for compulsory preschool, a policy that constrains families' choices by prescribing preschool attendance. We show that respondents' doubts about

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[38] Preferences for increases in public school spending were elicited early in the survey, prior to the experiment on educational inequality. On the question, "In your opinion, should public spending for schools in Germany increase, decrease, or stay the same?", 69 percent respond that spending should greatly increase or increase.

the policies' effectiveness to mitigate educational inequality partially explain preferences for education policy, but do not contribute to our understanding of why increased concerns fail to translate into support for education policy. Alternative explanations, such as respondents' disconnect between their concerns and the policies which are meant to address them, lacking trust in governmental institutions, or aversion to increased education spending also do not seem to be relevant in our setting.

Strategies to mitigate societal inequality are at the forefront of scientific and political discourse. In these debates, education policies have received considerable attention, partly because they might attenuate inequality without distorting economic efficiency (e.g., Bovenberg and Jacobs, 2005; Alvaredo et al., 2018). From a policy perspective, our findings that the German electorate conceives educational inequality as a problem, and that it consequently supports many equity-oriented education policies, suggests that policy makers have leeway to implement education reforms to foster equity. This is particularly true if they inform the public about the extent of educational inequality and about the effectiveness of the proposed policies.

We see two particularly interesting open questions for future research. First, it would be interesting to see whether our main conclusion is also born out in other countries with high educational inequality, such as the United States. Second, the fact that several educational reforms with majority appeal have not been enacted warrants more research on the political processes that determine education policy making. A potential explanation is that any equity-enhancing effects of education policies materialize only in the very long run. This is in contrast to other redistributive policies, such as tax reforms, whose expected effects on societal inequality are more immediate and short-term. Further research into the political economy of reforms whose benefits accrue over the very long run might be insightful to provide a better understanding of the feasibility of education policy reform.

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Figure 1: Heterogeneous information treatment effects by prior beliefs
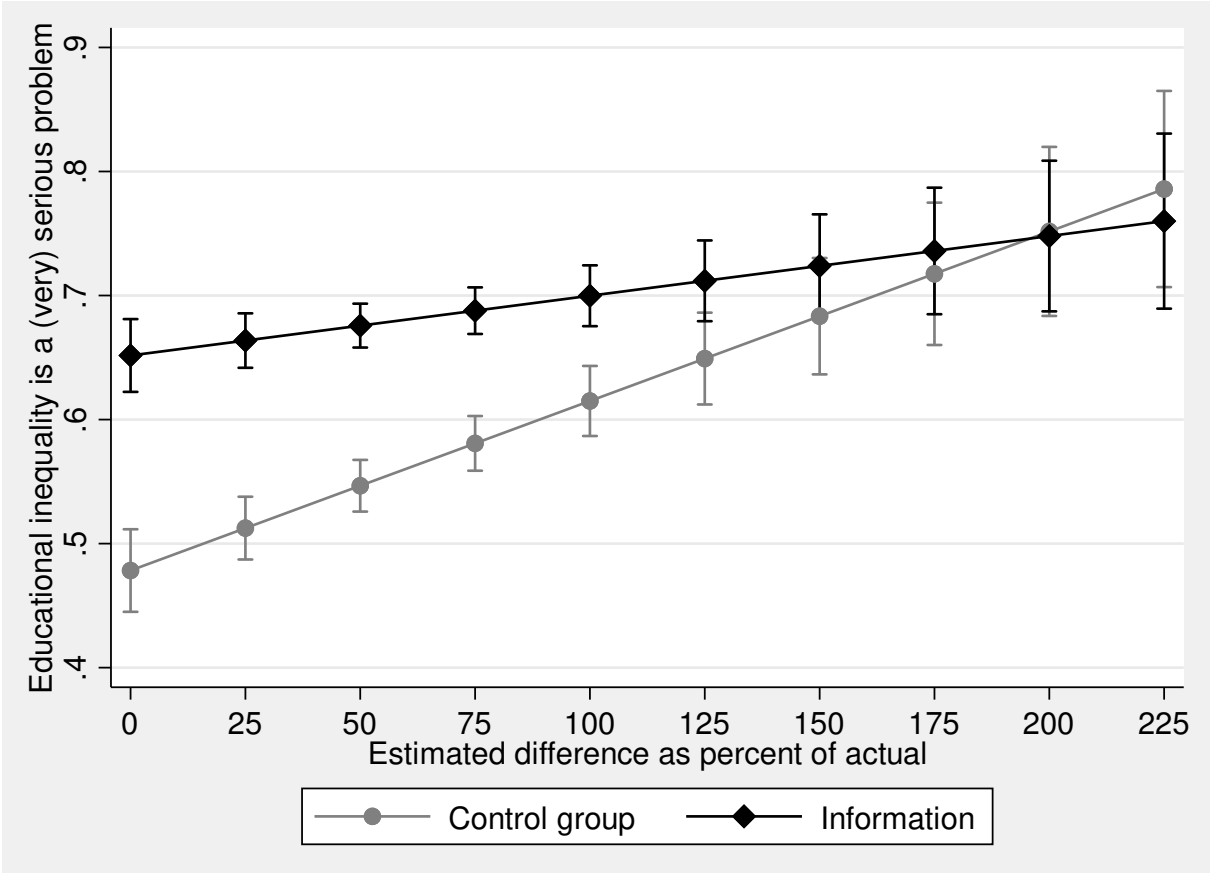
Notes: Concerns about educational inequality by experimental condition and prior beliefs about educational inequality. Horizontal axis: prior beliefs about the achievement gap between children from difficult and good social background, as a percentage of the actual achievement difference of four school years. Vertical axis: predicted concern that educational inequality is a serious or very serious problem (and 95 percent confidence intervals). Predictions based on linear probability model reported in column 1 of Table 3. Randomized experimental group "information": respondents informed that 15-year-olds from low socioeconomic backgrounds lag behind students from high socioeconomic backgrounds by four school years. Data source: ifo Education Survey 2016, 2017.

Table 1: Summary statistics and balancing tests

	2016 Survey			2017 Survey			
	Control group	Information	Information + Connect	Control group	Information	Effectiveness	Information + Effectiveness
	Mean	Difference	Difference	Mean	Difference	Difference	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age	50.53	-0.59	0.59	50.03	0.54	0.36	0.98
Female	0.52	-0.02	0.00	0.50	-0.00	0.01	0.03
Born in Germany	0.94	0.02	0.01	0.94	0.01	0.00	0.00
City size ≥ 100,000	0.33	0.01	-0.04*	0.33	-0.02	-0.01	-0.01
Monthly household income (€)	2,084	97	135*	2,286	39	-72	-40
Partner in household	0.52	0.07**	0.07***	0.54	0.03	-0.00	-0.00
Parent(s) w/ university degree	0.23	0.01	0.02	0.25	0.01	0.04	0.02
Highest educ. attainment							
No degree/basic degree	0.41	-0.04	-0.01	0.38	-0.01	0.02	-0.01
Middle school degree	0.29	0.03	0.00	0.28	0.04*	0.02	0.05**
Univ. entrance degree	0.30	0.01	0.00	0.34	-0.03	-0.04	-0.03
University degree	0.10	0.04***	0.03**	0.14	-0.01	0.01	0.01
Employment status							
Full-time employed	0.32	0.03	0.02	0.34	0.01	0.00	0.03
Part-time employed	0.13	0.00	-0.01	0.13	-0.01	-0.00	-0.01
Self-employed	0.03	0.01	0.00	0.05	-0.02**	-0.01	-0.01
Unemployed	0.06	-0.00	0.00	0.06	-0.01	-0.02*	-0.02
Parent status							
No children	0.41	-0.01	-0.01	0.42	0.02	0.01	0.00
At least one child < 18	0.19	0.02	0.02	0.21	-0.03	-0.01	-0.02
All children > 18	0.40	-0.02	-0.01	0.37	0.01	0.01	0.02

(continued on next page)

Table 1 (continued)

	2016 Survey			2017 Survey			
	Control group	Information	Information + Connect	Control group	Information	Effectiveness	Information + Effectiveness
	Mean	Difference	Difference	Mean	Difference	Difference	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Political party preferences							
CDU/CSU	0.20	0.00	-0.01	0.24	0.03	0.01	0.03
SPD	0.19	-0.01	0.01	0.20	0.00	-0.02	0.00
Linke	0.07	0.01	-0.01	0.08	-0.01	0.01	0.00
Grüne	0.08	0.00	-0.01	0.05	0.01	0.00	-0.01
Other	0.14	0.01	0.00	0.15	-0.02	-0.02	-0.03
None	0.32	-0.01	0.02	0.28	0.00	0.03	0.00
Frequent voter	0.76	0.00	0.01	0.82	-0.01	-0.01	-0.01
Educ. important for vote	0.76	0.02	0.04*	0.70	0.03	0.01	0.05**
Risk tolerance	4.46	-0.23*	-0.30**	4.11	0.19	0.11	0.03
Patience	5.90	0.04	0.11	6.01	0.10	-0.12	-0.01
Non-response: Concerns	0.01	-0.00	0.01	0.01	-0.00	-0.00	-0.00
Non-response: Policy preferences[a]	0.02	-0.01*	0.01	0.01	-0.00	-0.00	-0.00
Teacher	246	232	235	n.a.	n.a.	n.a.	n.a.
Observations[b]	1,121	1,102	1,079	1,026	1,004	1,017	1,031

Notes: Columns (1), (4): weighted group means. Columns (2)-(3) and (5)-(7): difference in means between the control group and the respective treatment group. Significance levels based on linear regressions of the respective background variables on the respective treatment indicator. [a] 2016 survey: average share of missing answers to the eight policy questions; 2017 survey: share of missing answers to question on compulsory preschool. [b] The number of observations does not include the oversample of 713 teachers in the 2016 survey. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Data source: ifo Education Survey 2016, 2017.

Table 2: Effect of information treatment on concerns that educational inequality is a problem

	Educational inequality is a (very) serious problem		Educational inequality is a small/no problem	
	(1)	(2)	(3)	(4)
Information	0.124***	0.120***	-0.050**	-0.048***
	(0.014)	(0.014)	(0.010)	(0.010)
Covariates	No	Yes	No	Yes
Control mean	0.554	0.554	0.138	0.138
Observations	7,327	7,327	7,327	7,327
R^2	0.017	0.063	0.006	0.033

Notes: Linear probability models. Dependent variable: columns (1)-(2): dummy variable coded 1="a very serious problem" or "a serious problem", 0 otherwise; columns (3)-(4): dummy variable coded 1="not a problem at all" or "a small problem", 0 otherwise. Randomized experimental treatment "information": respondents informed that 15-year-olds from low socioeconomic backgrounds lag behind students from high socioeconomic backgrounds by four school years. Control mean: mean of the outcome variable for the control group. Covariates include age, gender, migration background, education, income, employment status, partner in household, parent status, city size, parental education, political party preference, voting behavior, risk tolerance, and patience. Missing values of covariates are imputed. All regressions include imputation dummies and survey wave fixed effects. Regressions weighted by survey weights. Robust standard errors (clustered at the individual level) in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Data source: ifo Education Survey 2016, 2017.

Table 3: Heterogeneity of information treatment effect by information status at baseline

	Educational inequality is a (very) serious problem			Educational inequality is a small/no problem		
	All	Confident about belief	Not confident about belief	All	Confident about belief	Not confident about belief
	(1)	(2)	(3)	(4)	(5)	(6)
Information	0.173***	0.250***	0.142***	-0.058***	-0.095***	-0.044**
	(0.023)	(0.042)	(0.027)	(0.017)	(0.032)	(0.019)
Prior belief (% of actual)	0.137***	0.177***	0.112***	-0.048***	-0.088***	-0.030
	(0.023)	(0.040)	(0.029)	(0.017)	(0.031)	(0.020)
Information × Prior belief	-0.089***	-0.163***	-0.055	0.016	0.084*	-0.014
	(0.031)	(0.056)	(0.038)	(0.022)	(0.045)	(0.026)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,094	2,073	5,021	7,094	2,073	5,021
R^2	0.074	0.097	0.076	0.042	0.072	0.038

Notes: Linear probability models. Dependent variable: columns (1)-(3): dummy variable coded 1="a very serious problem" or "a serious problem", 0 otherwise; columns (4)-(6): dummy variable coded 1="not a problem at all" or "a small problem", 0 otherwise. Sample in columns (2) and (5): subgroup of respondents who are relatively sure that their stated belief is close to correct, as indicated by choosing a value between 5 and 7 on a scale from 1="very unsure" to 7="very sure"; sample in columns (3) and (6): subgroup of respondents who chose a value between 1 and 4. Randomized experimental treatment "information": respondents informed that 15-year-olds from low socioeconomic backgrounds lag behind students from high socioeconomic backgrounds by four school years. Prior belief: continuous variable measuring prior beliefs about achievement differences between children from difficult and good social backgrounds as a percentage of the actual difference of four school years. Covariates include age, gender, migration background, education, income, employment status, partner in household, parent status, city size, parental education, political party preference, voting behavior, risk tolerance, and patience. Missing values of covariates are imputed. All regressions include imputation dummies and survey wave fixed effects. Regressions weighted by survey weights. Robust standard errors (clustered at the individual level) in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10. Data source: ifo Education Survey 2016, 2017

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Table 4: Effect of information treatment in main survey on beliefs and concerns in follow-up survey

| | Belief about educational inequality | Confidence about belief | Educational inequality is a … | |
| | | | (very) serious problem | small/no problem |
	(1)	(2)	(3)	(4)
Information	0.524***	0.564***	0.057***	-0.016
	(0.073)	(0.067)	(0.020)	(0.014)
Covariates	Yes	Yes	Yes	Yes
Control mean	2.513	3.303	0.551	0.131
Observations	2,050	2,052	2,363	2,363
R^2	0.039	0.108	0.049	0.031

Notes: Linear probability models. Dependent variable (recorded in follow-up survey conducted about two weeks after the main survey): column (1): belief about the achievement gap between children from difficult and good social backgrounds in school-year equivalents; column (2): confidence about belief on seven-point Likert scale (1="very unsure", 7="very sure"); column (3): dummy variable coded 1="a very serious problem" or "a serious problem", 0 otherwise; column (4): dummy variable coded 1="not a problem at all" or "a small problem", 0 otherwise. Randomized experimental treatment "information": respondents informed that 15-year-olds from low socioeconomic backgrounds lag behind students from high socioeconomic backgrounds by four school years. Control mean: mean of the outcome variable for the control group in the follow-up survey. Covariates include age, gender, migration background, education, income, employment status, partner in household, parent status, city size, parental education, political party preference, voting behavior, risk tolerance, and patience. Missing values of covariates are imputed. All regressions include imputation dummies. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Data source: ifo Education Survey 2017.

Table 5: Correlations between concerns about educational inequality and policy preferences

Support for education policies:		Preschool		School					University
	Policy index	Free pre-school for low-income children	Com-pulsory preschool	Spending for disad-vantaged schools	Later tracking	Bonuses for teachers at disadvan-taged schools	Whole-day schooling for all students	Coeducation of children with/ out learning disability	Need-based scholarships
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Concerned about educational inequality	0.122*** (0.021)	0.141*** (0.033)	0.133*** (0.036)	0.176*** (0.032)	0.163*** (0.035)	0.032 (0.036)	0.099*** (0.036)	0.095*** (0.037)	0.137*** (0.031)
Covariates	No	No	No	No	No	No	No	No	No
Control mean	0.630	0.761	0.643	0.773	0.666	0.427	0.482	0.500	0.800
Observations	1,106	1,106	1,106	1,104	1,102	1,103	1,102	1,103	1,102
R^2	0.049	0.027	0.019	0.043	0.029	0.001	0.010	0.009	0.028

Notes: Sample: control group. Dependent variable: column (1): average support across policies; columns (2)-(9): dummy variable coded 1="strongly favor" or "somewhat favor" the respective policy, 0 otherwise. Concerned about educational inequality: dummy variable coded 1 if educational inequality is viewed as "a very serious problem" or "a serious problem", 0 otherwise. Control mean: mean of the outcome variable for the control group. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Data source: ifo Education Survey 2016.

Table 6: Effects of information treatment on education policy preferences: Share supporting respective policy

Support for education policies:		Preschool		School					University
	Policy index	Free pre-school for low-income children	Com-pulsory preschool	Spending for disad-vantaged schools	Later tracking	Bonuses for teachers at disadvan-taged schools	Whole-day schooling for all students	Coeducation of children with/ out learning disability	Need-based scholarships
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Information	0.024*	0.013	0.042*	0.032	0.029	0.019	0.025	0.007	0.017
	(0.013)	(0.021)	(0.023)	(0.020)	(0.023)	(0.024)	(0.024)	(0.025)	(0.020)
Information+Connect	-0.016	-0.019	0.003	-0.024	-0.006	-0.013	-0.017	-0.013	-0.041**
	(0.013)	(0.022)	(0.023)	(0.021)	(0.023)	(0.024)	(0.025)	(0.025)	(0.021)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.630	0.761	0.643	0.773	0.666	0.427	0.482	0.500	0.800
Observations	3,269	3,264	3,266	3,260	3,251	3,259	3,257	3,254	3,257
R^2	0.115	0.057	0.051	0.066	0.049	0.079	0.089	0.050	0.076

Notes: Linear probability models. Dependent variable: column (1): average support across policies; columns (2)-(9): dummy variable coded 1="strongly favor" or "somewhat favor" the respective policy, 0 otherwise. Randomized experimental treatment "information": respondents informed that 15-year-olds from low socioeconomic backgrounds lag behind students from high socioeconomic backgrounds by four school years. Randomized experimental treatment "information+connect": respondents additionally informed that the policies have the goal to increase the equality of educational opportunity. Control mean: mean of the outcome variable for the control group. Covariates include age, gender, migration background, education, income, employment status, partner in household, parent status, city size, parental education, political party preference, voting behavior, risk tolerance, and patience. Missing values of covariates are imputed. All regressions include imputation dummies. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Data source: ifo Education Survey 2016.

Table 7: Effects of information treatment on education policy preferences: Outcomes measured on five-point scale

Support for education policies:		Preschool		School					University
	Policy index	Free pre-school for low-income children	Com-pulsory preschool	Spending for disad-vantaged schools	Later tracking	Bonuses for teachers at disadvan-taged schools	Whole-day schooling for all students	Coeducation of children with/ out learning disability	Need-based scholarships
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Information	0.076**	0.016	0.168***	0.082*	0.098*	0.056	0.116*	-0.024	0.068
	(0.032)	(0.055)	(0.064)	(0.049)	(0.057)	(0.062)	(0.065)	(0.066)	(0.049)
Information+Connect	-0.045	-0.091	-0.011	-0.065	-0.012	-0.008	-0.061	0.015	-0.119**
	(0.034)	(0.057)	(0.062)	(0.053)	(0.058)	(0.064)	(0.067)	(0.068)	(0.054)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	3.608	4.028	3.664	4.005	3.714	3.024	3.167	3.232	4.056
Observations	3,269	3,264	3,266	3,260	3,251	3,259	3,257	3,254	3,257
R^2	0.112	0.067	0.044	0.082	0.058	0.079	0.089	0.043	0.075

Notes: Linear probability models. Dependent variable: column (1): average support across policies; columns (2)-(9): categorical variable coded 1="strongly oppose" through 5="strongly favor". Randomized experimental treatment "information": respondents informed that 15-year-olds from low socioeconomic backgrounds lag behind students from high socioeconomic backgrounds by four school years. Randomized experimental treatment "information+connect": respondents additionally informed that the policies have the goal to increase the equality of educational opportunity. Control mean: mean of the outcome variable for the control group. Covariates include age, gender, migration background, education, income, employment status, partner in household, parent status, city size, parental education, political party preference, voting behavior, risk tolerance, and patience. Missing values of covariates are imputed. All regressions include imputation dummies. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Data source: ifo Education Survey 2016.

Table 8: Effects of information and effectiveness treatments on preferences for compulsory preschool

	Waves 2016 and 2017				Wave 2017		
	Five-point scale		(Strongly) support		Five-point scale	(Strongly) support	(Strongly) oppose
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Information	0.242***	0.203***	0.073***	0.057***	0.219***	0.072***	-0.063***
	(0.066)	(0.046)	(0.024)	(0.017)	(0.063)	(0.023)	(0.021)
Effectiveness					0.176***	0.051**	-0.051**
					(0.064)	(0.023)	(0.021)
Information+Effectiveness					0.364***	0.126***	-0.109***
					(0.061)	(0.022)	(0.020)
Wave 2016	0.047		-0.015				
	(0.067)		(0.024)				
Information × Wave 2016	-0.074		-0.031				
	(0.092)		(0.034)				
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	3.641	3.641	0.651	0.651	3.640	0.664	0.244
Observations	4,225	4,225	4,225	4,225	4,062	4,062	4,062
R^2	0.062	0.062	0.057	0.055	0.066	0.059	0.051

Notes: Linear probability models. Samples: columns (1)-(4): control group and Information treatment group in waves 2016 and 2017; columns (5)-(7): control group and all treatment groups in wave 2017. Dependent variable: policy preference for compulsory preschool, coding: columns (1), (2), (5): categorical variable coded 1="strongly oppose" through 5="strongly favor"; columns (3), (4), (6): dummy variable coded 1="strongly favor" or "somewhat favor", 0 otherwise; column (7): dummy variable coded 1="strongly oppose" or "somewhat oppose", 0 otherwise. Randomized experimental treatment "information": respondents informed that 15-year-olds from low socioeconomic backgrounds lag behind students from high socioeconomic backgrounds by four school years. Randomized experimental treatment "effectiveness": respondents informed that a recent study shows that preschool participation strongly improves the later opportunities of children from disadvantaged backgrounds, but that these children are less likely to be enrolled in preschool. Randomized experimental treatment "information+effectiveness": respondents receive both pieces of information. Control mean: mean of the outcome variable for the control group. Covariates include age, gender, migration background, education, income, employment status, partner in household, parent status, city size, parental education, political party preference, voting behavior, risk tolerance, and patience. Missing values of covariates are imputed. All regressions include imputation dummies. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10. Data source: ifo Education Survey 2017.

Table 9: Heterogeneity of information treatment effects across subgroups

Support for education policies:		Preschool		School					University
	Policy index	Free pre-school for low-income children	Com-pulsory preschool	Spending for disad-vantaged schools	Later tracking	Bonuses for teachers at disadvan-taged schools	Whole-day schooling for all students	Coeducation of children with/out learning disability	Need-based scholarships
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Teachers									
No teacher (baseline)	0.019	0.001	0.052**	0.021	0.032	0.010	0.022	-0.001	-0.001
	(0.012)	(0.019)	(0.021)	(0.019)	(0.021)	(0.022)	(0.022)	(0.022)	(0.018)
Information × Teacher	-0.004	-0.010	-0.013	-0.052*	0.007	0.065	-0.023	0.006	0.002
	(0.024)	(0.039)	(0.044)	(0.030)	(0.051)	(0.052)	(0.052)	(0.050)	(0.035)
Panel B: Government supporters									
No gov. supporter (baseline)	0.011	0.020	0.050*	0.007	0.001	0.019	0.014	-0.005	-0.022
	(0.015)	(0.025)	(0.027)	(0.025)	(0.026)	(0.027)	(0.027)	(0.028)	(0.024)
Information × Gov. supporter	0.023	-0.049	0.009	0.038	0.083*	-0.018	0.020	0.016	0.053
	(0.025)	(0.038)	(0.044)	(0.037)	(0.044)	(0.045)	(0.046)	(0.046)	(0.036)
Panel C: Educ. spending supporters									
No edu. spend. sup. (baseline)	0.047*	0.022	0.124***	0.054	0.071*	0.024	0.022	0.010	0.038
	(0.025)	(0.039)	(0.040)	(0.038)	(0.040)	(0.039)	(0.039)	(0.040)	(0.039)
Information × Edu. spend. sup.	-0.048*	-0.038	-0.107**	-0.055	-0.063	-0.026	-0.005	-0.024	-0.064
	(0.028)	(0.044)	(0.047)	(0.043)	(0.047)	(0.047)	(0.047)	(0.048)	(0.043)

Notes: Linear probability models. Dependent variable: column (1): average support across policies; columns (2)-(9): dummy variable coded 1="strongly favor" or "somewhat favor" the respective policy, 0 otherwise. Randomized experimental treatment "information": respondents informed that 15-year-olds from low socioeconomic backgrounds lag behind students from high socioeconomic backgrounds by four school years. Estimates based on equation (2) with the respective subgroup indicated in each panel: panel A: teachers; panel B: partisans of the governing parties; panel C: respondents who support increases in public school spending. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Data source: ifo Education Survey 2016.

Figure A1: Illustration of the information treatment

Zahlreiche Studien zeigen, dass Bildungserfolg im frühkindlichen, schulischen und universitären Bereich stark damit zusammenhängt, aus welchen sozialen Hintergründen und familiären Einkommensverhältnissen die Kinder und Jugendlichen kommen. Zum Beispiel hat eine Bildungsstudie gezeigt, dass die Mathematikleistungen von 15-jährigen Schülerinnen und Schülern aus schwierigen sozialen Verhältnissen im Durchschnitt etwa 4 Schuljahre hinter den Mathematikleistungen von jenen aus guten sozialen Verhältnissen zurückliegen (Vergleich der sozial niedrigsten und höchsten 10 Prozent der Bevölkerung).
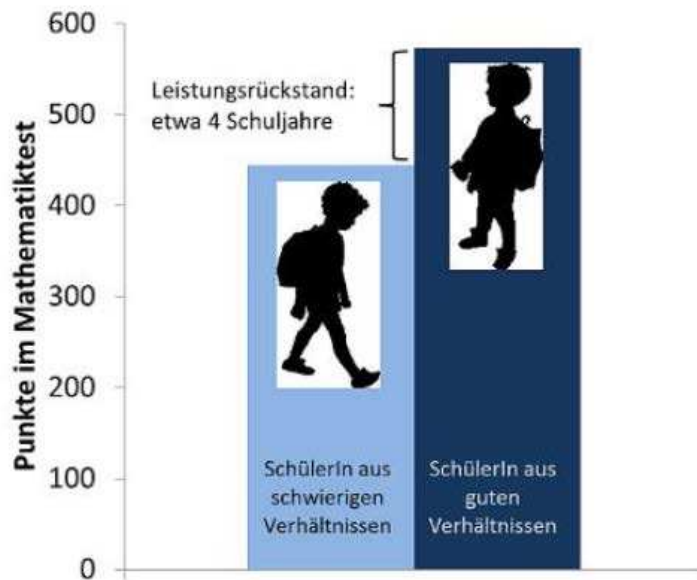
Source: ifo Education Survey 2016, 2017.

Figure A2: Respondents' prior beliefs about educational inequality
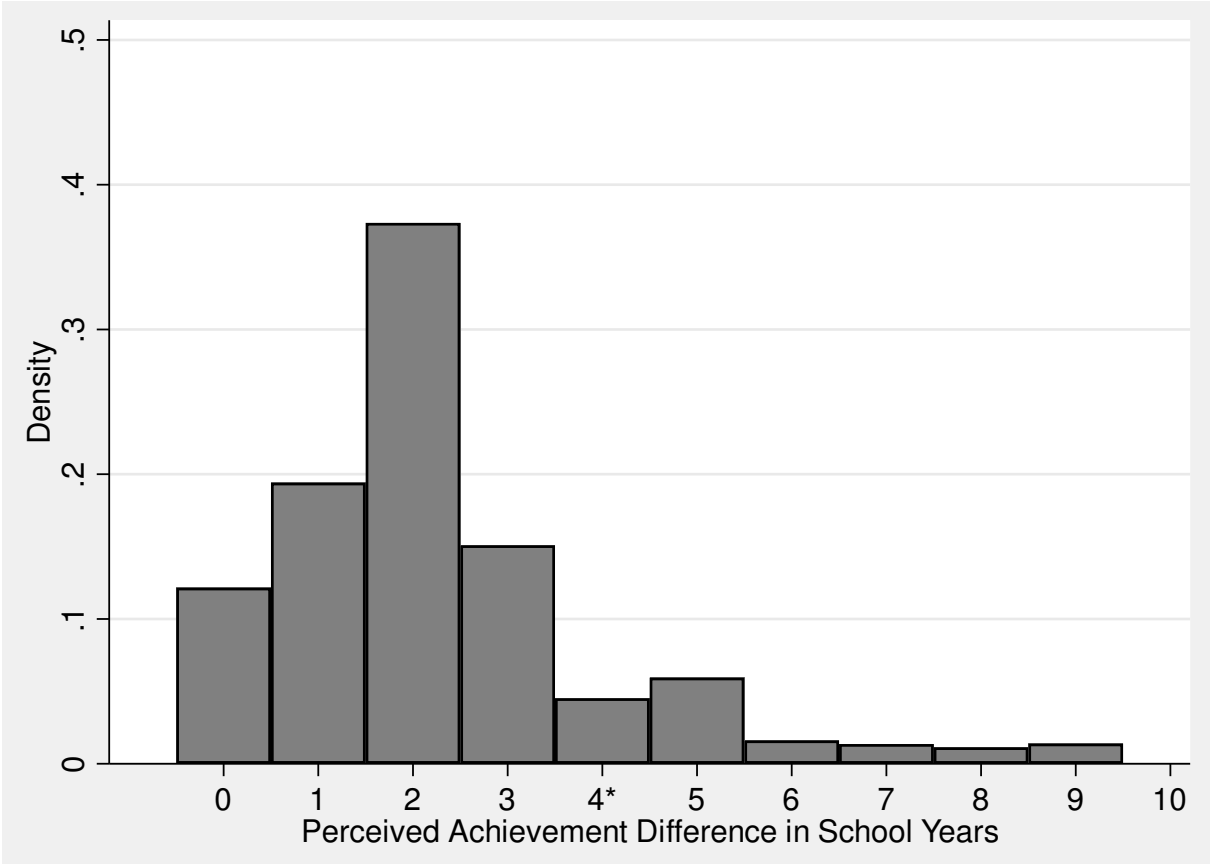
Notes: Histogram of the weighted distribution of beliefs about the achievement gap between children from difficult and good social backgrounds. Wording: "The next question concerns the comparison of educational success of children with different social backgrounds and family income. What is your best guess, how many school years do 15-year-old students from difficult social backgrounds lag behind students from good social backgrounds in their average mathematical achievements? Think of the highest and lowest ten percent of social background in the population. (The answer "0" means that there is no difference.)" * denotes the correct answer (4 school years). Data source: ifo Education Survey 2016, 2017.

Figure A3: Heterogeneous information treatment effects by prior beliefs and confidence
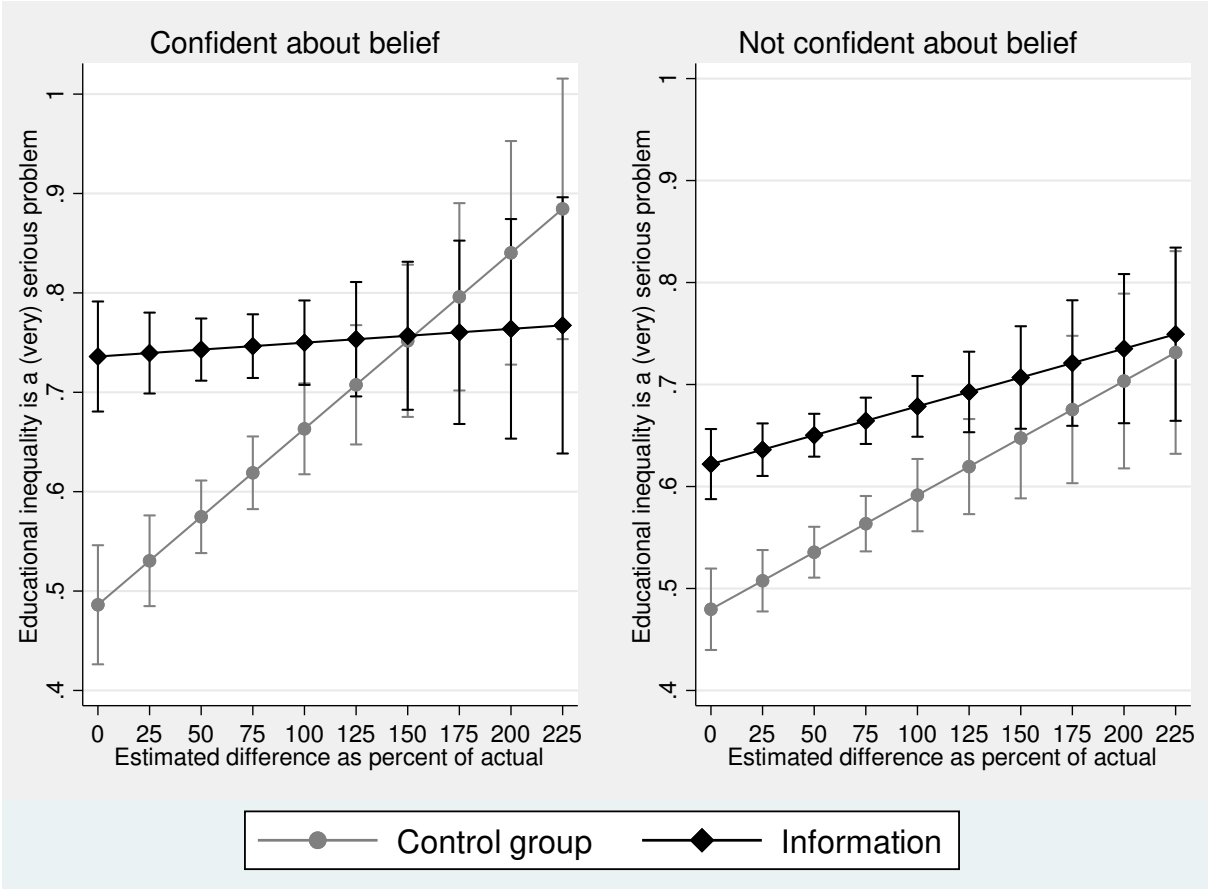
Notes: Concerns about educational inequality by experimental condition, prior beliefs about educational inequality, and confidence about prior beliefs. Sample in left panel: subgroup of respondents who are relatively sure that their stated belief is close to correct, as indicated by choosing a value between 5 and 7 on a scale from 1="very unsure" to 7="very sure"; sample in right panel: subgroup of respondents who chose a value between 1 and 4. Horizontal axis: prior beliefs about the achievement gap between children from difficult and good social background, as a percentage of the actual achievement difference of four school years. Vertical axis: predicted concern that educational inequality is a serious or very serious problem (and 95 percent confidence intervals). Predictions based on linear probability models reported in columns 2 (confident about belief) and 3 (not confident about belief) of Table 3. Randomized experimental group "information": respondents informed that 15-year-olds from low socioeconomic backgrounds lag behind students from high socioeconomic backgrounds by four school years. Data source: ifo Education Survey 2016, 2017.

Table A1: Who perceives educational inequality as a problem?

	Educational inequality is a problem			
	Five-point scale (1)		Binary (2)	
Age	0.008***	(0.002)	0.004***	(0.001)
Female	0.034	(0.044)	0.019	(0.022)
Born in Germany	0.030	(0.106)	-0.019	(0.048)
City size ≥ 100,000	0.085**	(0.043)	0.039*	(0.022)
Monthly household income (1000 €)	-0.020	(0.019)	-0.012	(0.009)
Partner in household	-0.013	(0.050)	-0.017	(0.024)
Parent(s) with university degree	-0.051	(0.051)	-0.019	(0.025)
Middle school degree	0.120**	(0.052)	0.036	(0.026)
University entrance degree	0.120*	(0.063)	0.072**	(0.032)
University degree	0.001	(0.067)	-0.032	(0.034)
Full-time employed	-0.069	(0.055)	-0.013	(0.026)
Part-time employed	0.033	(0.065)	0.023	(0.033)
Self-employed	-0.052	(0.103)	-0.018	(0.054)
Unemployed	0.134	(0.109)	0.074	(0.049)
At least one child < 18	0.092	(0.059)	0.013	(0.030)
All children > 18	0.004	(0.062)	0.002	(0.032)
CDU/CSU partisan	-0.257***	(0.054)	-0.135***	(0.027)
SPD partisan	-0.064	(0.055)	0.003	(0.028)
Frequent voter	0.174***	(0.061)	0.102***	(0.028)
"Education" important for vote	0.210***	(0.049)	0.092***	(0.025)
Risk tolerance	-0.015	(0.009)	-0.007	(0.004)
Patience	0.034***	(0.010)	0.012***	(0.004)
Wave 2017 dummy	Yes		Yes	
Constant	2.780***	(0.180)	0.205**	(0.082)
Observations	3,146		3,146	
R^2	0.066		0.060	

Notes: Linear probability models. Sample: control group. Dependent variable: column (1): categorical variable coded 1="not a problem at all" through 5="a very serious problem"; column (2): dummy variable coded 1="a very serious problem" or "a serious problem", 0 otherwise. Missing values are imputed. All regressions include imputation dummies. Regressions weighted by survey weights. Robust standard errors (clustered at the individual level) in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Data source: ifo Education Survey 2016, 2017.

Table A2: Effect of information treatment on concerns that educational inequality is a problem: Robustness of outcome coding

	Five-point scale	Educational inequality is …				
		a very serious problem	a serious problem	a medium problem	a small problem	not a problem at all
	(1)	(2)	(3)	(4)	(5)	(6)
Information	0.249***	0.075***	0.044***	-0.071***	-0.042***	-0.007
	(0.027)	(0.011)	(0.014)	(0.013)	(0.009)	(0.004)
Wave 2017	0.004	-0.020*	0.018	0.023*	-0.014*	-0.006
	(0.027)	(0.011)	(0.014)	(0.012)	(0.008)	(0.004)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	3.567	0.175	0.379	0.310	0.115	0.023
Observations	7,327	7,327	7,327	7,327	7,327	7,327
R^2	0.071	0.044	0.018	0.034	0.025	0.019

Notes: Linear probability models. Dependent variable: column (1): categorical variable coded 1="not a problem at all" through 5="a very serious problem"; columns (2)-(6): dummy variable coded 1=answer category given in respective table header, 0 otherwise. Randomized experimental treatment "information": respondents informed that 15-year-olds from low socioeconomic backgrounds lag behind students from high socioeconomic backgrounds by four school years. Control mean: mean of the outcome variable for the control group. Covariates include age, gender, migration background, education, income, employment status, partner in household, parent status, city size, parental education, political party preference, voting behavior, risk tolerance, and patience. Missing values of covariates are imputed. All regressions include imputation dummies and survey wave fixed effects. Regressions weighted by survey weights. Robust standard errors (clustered at the individual level) in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Data source: ifo Education Survey 2016, 2017.

Table A3: Heterogeneity of information treatment effect by survey year

	Educational inequality is a (very) serious problem		Educational inequality is a small/no problem	
	(1)	(2)	(3)	(4)
Information	0.101***	0.096***	-0.030*	-0.028*
	(0.022)	(0.021)	(0.016)	(0.016)
Wave 2017	-0.031	-0.026	0.000	0.001
	(0.022)	(0.022)	(0.017)	(0.016)
Information × Wave 2017	0.039	0.041	-0.034*	-0.035*
	(0.029)	(0.028)	(0.020)	(0.020)
Covariates	No	Yes	No	Yes
Control mean (in wave 2016)	0.574	0.574	0.138	0.138
Observations	7,327	7,327	7,327	7,327
R^2	0.017	0.063	0.007	0.034

Notes: Linear probability models. Dependent variable: columns (1)-(2): dummy variable coded 1="a very serious problem" or "a serious problem", 0 otherwise; columns (3)-(4): dummy variable coded 1="not a problem at all" or "a small problem", 0 otherwise. Randomized experimental treatment "information": respondents informed that 15-year-olds from low socioeconomic backgrounds lag behind students from high socioeconomic backgrounds by four school years. Control mean: mean of the outcome variable for the control group in the 2016 survey. Covariates include age, gender, migration background, education, income, employment status, partner in household, parent status, city size, parental education, political party preference, voting behavior, risk tolerance, and patience. Missing values of covariates are imputed. All regressions include imputation dummies and survey wave fixed effects. Regressions weighted by survey weights. Robust standard errors (clustered at the individual level) in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Data source: ifo Education Survey 2016, 2017.

Table A4: Prediction of participation in the follow-up survey

	Participation in follow-up survey			
	Experiment on concerns for educational inequality (1)		Experiment on preferences for compulsory preschool (2)	
Treatment status in the main survey				
Information	-0.013	*(0.016)*	-0.011	*(0.022)*
Effectiveness			-0.010	*(0.022)*
Information+Effectiveness			-0.026	*(0.022)*
Covariates				
Age	0.006***	*(0.001)*	0.006***	*(0.001)*
Female	-0.017	*(0.017)*	-0.018	*(0.017)*
Born in Germany	-0.011	*(0.040)*	-0.011	*(0.040)*
City size ≥ 100,000	-0.024	*(0.016)*	-0.025	*(0.016)*
Monthly hh. income (1000 €)	0.005	*(0.006)*	0.004	*(0.006)*
Partner in household	-0.021	*(0.018)*	-0.020	*(0.018)*
Parent(s) with university degree	-0.013	*(0.018)*	-0.012	*(0.018)*
Middle school degree	0.009	*(0.021)*	0.010	*(0.021)*
University entrance degree	0.014	*(0.026)*	0.015	*(0.026)*
University degree	0.002	*(0.025)*	0.002	*(0.025)*
Full-time employed	0.046**	*(0.019)*	0.046**	*(0.019)*
Part-time employed	0.028	*(0.026)*	0.029	*(0.026)*
Self-employed	0.017	*(0.041)*	0.018	*(0.041)*
Unemployed	0.047	*(0.039)*	0.047	*(0.039)*
At least one child < 18	-0.001	*(0.022)*	-0.001	*(0.022)*
All children > 18	-0.020	*(0.025)*	-0.019	*(0.025)*
CDU/CSU partisan	-0.004	*(0.020)*	-0.004	*(0.020)*
SPD partisan	0.002	*(0.021)*	0.003	*(0.021)*
Frequent voter	0.069***	*(0.022)*	0.068***	*(0.022)*
"Education" important for vote	-0.024	*(0.018)*	-0.025	*(0.018)*
Risk tolerance	-0.013***	*(0.003)*	-0.013***	*(0.003)*
Patience	0.007**	*(0.003)*	0.007**	*(0.003)*
Constant	0.359***	*(0.063)*	0.363***	*(0.064)*
Observations	3,696		3,696	
R^2	0.054		0.054	

Notes: Linear probability models. Sample: online sample. Dependent variable: dummy variable coded 1=respondent participated in follow-up survey, 0 otherwise. Treatment status in the main survey: column (1): treatment in experiment on concerns about educational inequality; column (2): treatments in experiment on compulsory preschool. Missing values are imputed. All regressions include imputation dummies. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. Data source: ifo Education Survey 2017.

Table A5: Summary statistics and balancing tests: Follow-up survey

	2017 Survey			
	Control group	Information	Effectiveness	Information + Effectiveness
	Mean	Difference	Difference	Difference
	(1)	(2)	(3)	(4)
Age	47.77	0.72	0.21	0.41
Female	0.49	-0.02	-0.01	-0.01
Born in Germany	0.97	0.00	-0.02	-0.01
City size $\geq$ 100,000	0.33	-0.02	0.00	-0.01
Monthly household income (€)	2,422	85	-205**	-27
Partner in household	0.56	0.06*	0.029	0.02
Parent(s) with university degree	0.27	0.00	0.04	0.02
Highest educ. attainment				
No degree/basic degree	0.31	-0.02	0.03	0.01
Middle school degree	0.31	0.05	0.03	0.03
Univ. entrance degree	0.38	-0.03	-0.06**	-0.05
University degree	0.16	0.00	0.01	0.01
Employment status				
Full-time employed	0.38	-0.01	0.03	0.04
Part-time employed	0.15	-0.03	-0.02	-0.01
Self-employed	0.06	-0.02	-0.02	-0.02
Unemployed	0.06	-0.02	-0.01	-0.02
Parent status				
No children	0.42	0.02	0.02	0.04
At least one child < 18	0.25	-0.05*	-0.03	-0.05*
All children > 18	0.33	0.03	0.01	0.01
Political party preferences				
CDU/CSU	0.22	0.04	-0.01	0.04
SPD	0.20	0.00	0.01	-0.02
Linke	0.08	0.00	-0.00	0.00
Grüne	0.07	-0.01	-0.02	-0.02
Other	0.16	-0.04*	-0.02	0.00
None	0.27	0.00	0.03	0.00
Frequent voter	0.85	-0.01	-0.03	-0.04
Educ. important for vote	0.69	0.03	0.04	0.02
Risk tolerance	4.09	0.13	0.28*	0.22
Patience	6.20	0.05	-0.23	0.07
Observations	612	583	590	578

Notes: Follow-up survey. Column (1): group means. Columns (2)-(4): difference in means between the control group and the respective treatment group. Significance levels based on linear regressions of the respective background variables on the respective treatment indicator. Significance levels: *** p<0.01, ** p<0.05, * p<0.10. Data source: ifo Education Survey 2017.

Table A6: Effects of information treatments in main survey on preferences for compulsory preschool in follow-up survey

	Five-point scale	(Strongly) support compulsory preschool	(Strongly) oppose compulsory preschool
	(1)	(2)	(3)
Information	0.047	0.022	-0.006
	(0.070)	(0.026)	(0.023)
Effectiveness	0.121*	0.034	-0.037
	(0.070)	(0.026)	(0.023)
Information+Effectiveness	0.139**	0.046*	-0.029
	(0.070)	(0.026)	(0.023)
Covariates	Yes	Yes	Yes
Control mean	3.740	0.699	0.214
Observations	2,362	2,362	2,362
R^2	0.039	0.040	0.023

Notes: Linear probability models. Dependent variable (recorded in follow-up survey conducted about two weeks after the main survey): policy preference for compulsory preschool, coding: column (1): categorical variable coded 1="strongly oppose" through 5="strongly favor"; column (2): dummy variable coded 1="strongly favor" or "somewhat favor", 0 otherwise; column (3): dummy variable coded 1="strongly oppose" or "somewhat oppose", 0 otherwise. Randomized experimental treatment "information": respondents informed that 15-year-olds from low socioeconomic backgrounds lag behind students from high socioeconomic backgrounds by four school years. Randomized experimental treatment "effectiveness": respondents informed that a recent study shows that preschool participation strongly improves the later opportunities of children from disadvantaged backgrounds, but that these children are less likely to be enrolled in preschool. Randomized experimental treatment "information+effectiveness": respondents receive both pieces of information. Control mean: mean of the outcome variable for the control group. Covariates include age, gender, migration background, education, income, employment status, partner in household, parent status, city size, parental education, political party preference, voting behavior, risk tolerance, and patience. Missing values of covariates are imputed. All regressions include imputation dummies. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10. Data source: ifo Education Survey 2017.