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Bibliografische Information der Deutschen Nationalbibliothek

Die Deutsche Nationalbibliothek verzeichnet diese Publikation
in der Deutschen Nationalbibliografie; detaillierte bibliografische
Daten sind im Internet über
http://dnb.d-nb.de
abrufbar.

ISBN: 978-3-95942-057-0

Druck: ifo Institut, München

ifo Institut im Internet:
http://www.cesifo-group.de

Vorwort

Den vorliegenden Aufsatz hat Katharina Werner im Rahmen ihrer Tätigkeit am ifo Zentrum für Bildungsökonomik verfasst. Die Arbeit ist im März 2018 fertiggestellt und als Doktorarbeit an der Ludwig-Maximilians-Universität eingereicht worden. Ziel der Arbeit ist es, wichtige ökonomische Fragen zu Bildungspolitik und Bildungsentscheidungen zu untersuchen. Ein erster Schwerpunkt ist der Zusammenhang zwischen der öffentlichen Meinung zu staatlichen Bildungsausgaben und dem Informationsstand der Wahlbevölkerung. Zunächst wird untersucht wie sich die Präferenzen für öffentliche Bildungsausgaben in Deutschland und den USA unterscheiden. Dabei zeigt sich, dass die Bereitschaft für höhere Bildungsausgaben davon abhängt ob die Bevölkerung über die aktuelle Höhe der Ausgaben korrekt informiert ist. Das folgende Kapitel zeigt, dass die öffentliche Meinung zu Ausgaben für frühkindliche Bildung im Widerspruch zu wissenschaftlichen Erkenntnissen steht. Insgesamt befürworten 45 Prozent der Deutschen höhere Ausgaben für Kindergärten und Grundschulen, während die Mehrheit von 55 Prozent zusätzliche Gelder lieber in weiterführende Schulen, Berufsschulen oder Universitäten investieren möchte. Werden die Befragten im Rahmen eines Survey-Experiments darüber informiert, dass wissenschaftliche Studien für Ausgaben im frühkindlichen Bereich einen stärkeren positiven Einfluss auf den zukünftigen Wohlstand der Gesellschaft finden als für Ausgaben in späteren Bildungsbereichen, verschiebt sich die Mehrheit zugunsten von Ausgaben für Kindergärten und Grundschulen. Ein zweiter Schwerpunkt der Doktorarbeit ist die Fragestellung, wie die Abhängigkeit des Bildungserfolgs von Kindern vom Bildungsniveau der Eltern reduziert werden kann. Hierbei wird zunächst den Zusammenhang zwischen Chancenungleichheit und bildungspolitische Präferenzen untersucht, wobei sich zeigt, dass die Mehrheit der Deutschen Reformen befürwortet, die darauf abzielen die Ungleichheit im Bildungsbereich zu reduzieren. Im letzten Kapitel wird untersucht, ob sich die Kluft in Bildungsaspiration durch wirtschaftliche Unkenntnis erklärt. Generell zeigen sich große Unterschiede darin, welchen Bildungsabschluss sich die Deutschen für ihre Kinder wünschen, die sich auch durch Informationen zu den Erträgen und Kosten eines Studiums nicht verringern lassen.

Keywords: education spending, inequality, information, survey experiments
JEL-No: I22, I24, H52, J24, H75, D72, D83, P16, D30, H11, D63, D72

Danksagung

Mein Dank gilt zunächst meinen Gutachtern, Ludger Wößmann, Joachim Winter und Guido Schwerdt für ihre Unterstützung, sowie meinen Koautoren Philipp Lergetporer und Martin West für die produktive Zusammenarbeit.

Ein besonderer Dank gilt außerdem allen meinen Kollegen und Kolleginnen am ifo Zentrum für Bildungsökonomik, ganz besonders Ulrike Baldi-Cohrs, Franziska Binder, Franziska Kugler, Elisabeth Grewenig und Sarah Kersten für die hervorragende Zusammenarbeit bei der Erhebung der Daten.

The Role of Information for Public Preferences on Education

Evidence from Representative Survey Experiments

Inaugural-Dissertation
zur Erlangung des Grades
Doctor oeconomicae publicae (Dr. oec. publ.)

eingereicht an der
Ludwig-Maximilians-Universität München
2018

vorgelegt von

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Referent: Prof. Dr. Ludger Wößmann
Koreferent: Prof. Dr. Joachim Winter
Promotionsabschlussberatung: 11.07.2018

Table of Contents

List of Figures

List of Tables

1 Introduction

In societal debate, education has emerged as one of the key factors for the future success of economies. For example, both in Germany and the United States 97 percent of the population believe that the education attainment of youths is important for the future prosperity of their country (Henderson et al., 2015). This popular opinion is backed by scientific findings, which show that the human capital of the population is a key determinant of long-run economic growth (Hanushek and Woessmann, 2012).

The average level of education in Germany has risen steadily, and there is little to suggest this trend will abate in coming years. While less than 5 percent of a cohort graduated with a *Abitur* degree in the 1950s, about half of each cohort now attains university entrance qualifications (Aktionsrat Bildung, 2011). Since 2015, enrolling in university education is the most common education path for secondary school graduates (Autorengruppe Bildungsberichterstattung, 2014). Global trends, such as the rise of technology, increase the demand for highly skilled labor and provide continued incentives for students to invest in education. However, the mathematics performance of German elementary schools students has declined since 2011 (Stanat et al., 2017). At the same time, educational success in Germany depends on socio-economic status more than in most developed countries (OECD, 2016a). With an ever increasing demand for highly skilled workers, the question of how to set up an education system that can cost-efficiently help students develop their full potential is increasingly crucial.

The rising importance of education for labor market success is mirrored in the rapid growth of literature in the economics of education (Hanushek et al., 2016). As the knowledge base on effective education policy expands, the implementation of successful education policies gains particular relevance. Over time, governments are increasingly relying on scientific expertise to provide answers to the pressing questions of policy design. For example, the U.S. is currently working towards a comprehensive approach to evidence-based policy, noting that the use of evidence is fundamental for an effective government (Abraham et al., 2017). The literature on the economics of education works towards a better understanding of how education should be financed, how it is acquired, and how optimal public education policy could be set up successfully. These are the very questions that motivate this project.

Education is typically provided by public schools under close control of the government. Even schools within the private sector are often both heavily subsidized and regulated.

Consequently, the institutional structure and the allocation of resources to schools are determined by the political process. This implies that in democratic societies, the result of elections plays a considerable role in shaping education policy. Therefore, a better understanding of public preferences for education can help to provide new insights into the feasibility of education reforms.

In particular, this thesis investigates the effects of information provision on the German public's preferences on two aspects of education: public education spending and inequality in education attainment. Discussions of the proper role of the state and the appropriate size of government are probably as old as the economics profession itself. This discussion extends to the issue of education spending, which comprises a substantial fraction of public expenditures. Chapter 2 therefore investigates public opinion for spending on education and the effect of information on current spending levels in a framework comparing the U.S. and Germany. Similarly, chapter 3 contributes to our understanding of the political economy of education spending by providing a comprehensive picture of the German public's preferences on the allocation of increases in education spending across education levels from early education to university.

The education attainment of children often depends on their parents' educational background, leading to inequality in education outcomes that corroborates existing inequality in socio-economic status. The third and fourth chapter therefore focus on approaches to mitigate inequality in education attainment. Chapter 3 investigates the role of information on the current extent of educational inequality for concerns about educational inequality and public preferences for equality-oriented policies. Chapter 4 addresses a complementary explanation of education inequality: educational aspirations might differ by socio-economic status due to ignorance of returns and costs of higher education.

The first chapter proceeds as follows. Section 1.1 introduces some considerations regarding the political economy of education policy. Section 1.2 describes the data that allows us to shed light on preferences for education in Germany. Section 1.3 discusses the methodology of survey experiments as a means to identify the effects of information. Section 1.4 concludes with a brief summary of the main hypotheses and findings summarized in this thesis.

1.1 Political Economy of Education Economics

This section introduces a simple theoretical framework to clarify ideas on how education policy is determined by public preferences (section 1.1.1) and how public preferences depend on information (section 1.1.2).

1.1.1 Theoretical Benchmark: Median Voter Theorem

A standard way to model the role of public opinion in political decision making is to assume that rational individuals vote for the party that best represents their preferences (Krueger and Blinder, 2004). The benchmark model of political economy is the median voter framework (see the seminal work of Meltzer and Richard, 1981). It shows that under certain assumptions, the median voter's preferences will emerge as the outcome of a majority vote. The theorem assumes that policy preferences for an issue are unimodal, which implies that voters always prefer a policy that is closer to their ideal policy to one that is farther away. It then follows that out of two parties competing for vote shares, each individual will vote for the party whose platform is closest to her preferred policy. It turns out that the only equilibrium of this model is that both parties campaign on the median voter's platform. The intuition of this result is that if one party offered a platform other than the median, it would be optimal for the other party to shift its platform to a point marginally closer to the median and command a winning majority. As this is true for both parties, in equilibrium both have a 50 percent chance to win the election. Note that the outcome no longer depends on the winning party in the equilibrium, as both campaign on the same platform. While the assumptions of the simple model need not be met in many real-world situations (see Kemp, 2002 for a review of the model's theoretical and empirical challenges), it remains one of the most used benchmarks in this literature and is a helpful guide for the interpretation of the findings in this thesis.

1.1.2 (Lack of) Information

Political preferences may be more malleable than assumed in the simple median voter framework if voters have imperfect information. In the standard model introduced above, voters are rational decision-makers and well-informed about the policy issue at hand. However, not all relevant information is easily accessible to voters (Stiglitz, 2000). At the same time, models of bounded rationality show that even if the information is attainable, it might not be efficient for voters to make themselves aware of all information that is available (Simon, 1955). This opens up the question what information voters attain to inform their opinions.

Empirically, ignorance of the electorate is well documented. The literature in political science has long established that voters seem to lack basic information that should be expected to influence political preferences (e.g. Gilens, 2001). In economics, the literature has highlighted the fact that imperfect information can impede the electorate's ability to monitor policy, for example if the electorate is unable to correctly deduce the tax rate and hence the price they pay for government services (Chetty et al., 2009).

This thesis contributes to our understanding of how information influences public opinion. Two main challenges have to be solved to provide insightful evidence. Most importantly, information acquisition is likely endogenous, meaning that citizens who are better informed about a particular issue might plausibly differ along a variety of characteristics. Therefore, we can draw only limited conclusions from observed correlations between information and public preferences. Therefore, the chapters in this thesis exploit the random provision of information, which are exogenous to citizens' unobserved characteristics. Secondly, ignorance might be rational. As mentioned above, acquiring information is likely to be costly, for example in terms of opportunity or cognitive costs. This implies that individuals cannot be expected to know information that is irrelevant to their preferences or decisions. Only if we find that providing information changes political preferences, we can deduce that prior knowledge on this issue was imperfect and that the information is indeed relevant for the decision at hand.

1.2 Data

This section describes some characteristics of the survey data (section 1.2.1) and highlights notable features of the sampling approach (section 1.2.2).

1.2.1 ifo Education Survey

This thesis draws on a dataset that was collected by the ifo Center for the Economics of Education in cooperation with TNS Infratest (now Kantar Public) from 2014 to 2017. Each year, the survey sampled a repeated cross-section of participants aged 18 and older. Overall, the survey questionnaire administered more than 30 questions on education policy. In addition, it included a variety of background characteristics that allow us to investigate correlates of public opinion. Respondents were split into a maximum of four experimental conditions, with randomization into control and treatment groups determined independent across survey experiments that took place within the same survey wave. Detailed description of the particular features of the different survey waves can be found in the respective chapters below.

1.2.2 Representativeness

The survey was designed to replicate the structure of the residential voting-age population of Germany as closely as possible. In order to gain insights into the political process, it is necessary to provide a comprehensive picture of public opinion. The majority of respondents was sampled through an online platform and answered the questions on their personal computing device. In order to avoid the exclusion of those parts of the population that do not use the internet, we conducted a mixed-mode survey (Bosnjak, 2017). Respondents that reported to not use the internet during a random-sampling household interview completed our survey on a technological device provided by the interviewer. In case the respondent felt unable to operate the survey device, the interviewers were instructed to help as much as needed, resulting in a traditional computer-assisted personal interview (CAPI) for these respondents. Throughout this thesis, survey weights are used to ensure the data are representative for the German population along a number of key dimensions. In particular, the sample is weighted according to official statistics on age, gender, educational attainment, region of residence, and municipality size. Overall, the sampling strategy increases our confidence that the dataset represents the diversity of political opinions in Germany reasonably well.

1.3 Identification of Causal Effects

In this thesis, the effect of information on political preferences is evaluated through survey experiments. While the construction of a proper counterfactual is a thorny challenge in many empirical studies, randomization provides a conveniently easy solution (Fisher, 1935). Experiments offer a way for research to identify effects with high internal validity, meaning that the observed changes in the outcome variable can be confidently attributed to the manipulation by the experimenter. The fact that experiments allow the researcher to design an intervention and cleanly identify its effects is useful for establishing causal effects. While correlational analysis can be highly useful to improve predictions of an outcome variable of interest, experimental interventions are useful to understand the causal drivers of any correlation.

In general, experiments can be classified according to their setting, their subject pools and their outcomes (Harrison and List, 2004). Survey experiments, like the ones used in this thesis, are usually artefactual field experiments that record stated preferences. They are field experiments because they generally observe participants in their natural environment. A natural worry for any experiment is to anticipate potential biases that result from non-random selection into participation in a study. In this regard, an advantage of survey

experiments is that they do not require participants to travel to a laboratory, which substantially lowers the costs of participation. This allows survey experiments to rely on a diverse subject pool. In contrast to participants in framed or natural field experiments, respondents in survey experiments are fully aware that their behavior is monitored. Another important characteristic of surveys is that they often measure, rather than observe, the outcomes of interest. A potential concern with stated outcomes is that as survey answers usually have no direct consequences, respondents might find it cheap to give incorrect answers. Therefore, it has become common practice in laboratory experiments to incentivize respondents in order to ensure that participants' decisions mimic real-world trade-offs. While it is uncommon to incentivize participants in surveys, we recently conducted research suggesting that incentives for correct answers have only limited effects on stated beliefs and leave preferences unchanged (Grewenig et al., 2018).

Overall, the methodology of surveys is especially well-suited to study questions of public opinion. First of all, opinions are never revealed. Even though it might be possible to deduce certain beliefs from revealed behavior, such analysis will always require strong assumptions on the relationship between the underlying opinion and the individuals' actions. For example, if vote choices are observed, it remains unclear to what extent different policy issues or external factors influenced voters' choices (Carlsson et al., 2016). In this regard, surveys offer complementary evidence by measuring opinions and beliefs directly. Second, politicians rely on survey results to conduct economic policy (Krueger and Blinder, 2004). Lastly, the diverse participant pool of surveys is particularly important for research in political economy because these questions typically require obtaining the distribution of preferences throughout the entire population.

1.4 Outline of this Thesis

This section provides a brief overview on the research agenda summarized in this thesis, including the hypotheses, experimental design and findings of each of the four remaining chapters.

The aim of chapter 2 is to test whether information on the current level of public spending affects public preferences for education, and whether the effects differ between Germany and the United States. The focus of the chapter is on general education spending and teacher salaries, which constitute a substantial share of public expenditures. Survey experiments show that in both countries, support for increased education spending and teacher salaries falls when respondents receive information about current spending levels. Treatment effects vary by prior knowledge in a manner consistent with information effects

rather than priming. Support for salary increases is inversely related to salary levels across American states, suggesting that higher salaries could explain much of Germans' lower support for increases in teacher salary levels. In an additional experiment, we show that the majority of respondents favors additional spending for class-size reductions. Information about the tradeoffs between specific spending categories shifts preferences from class-size reduction towards more spending on books and materials, or increased teacher salaries. Additional experiments in the German survey indicate that information effects extend to specific reform proposals and to other areas of public spending, including social security, public safety, and defense. We find that despite a lack of awareness for current public spending levels, respondents show support for increasing the funds allocated towards education.

Chapter 3 focuses on the allocation of increased public spending for education on different areas from preschool to university. Even though recent findings from the economic literature suggest that spending on early education can be very effective, results show that the majority of Germans does not support investing additional spending in this category. However, information about benefits of early education spending shifts majority support to spending on early and primary education from spending on secondary schools or later education stages. The effects persist over a two-week period, suggesting true information effects as channel. By contrast, results do not show that respondents with higher socioeconomic status are biased against early education investments, as would be predicted by models of special interest group influence. Therefore, the findings suggest that information is an important factor to understand the politics of education finance in Germany.

Chapter 4 investigates education policy that could contribute to increased equality of opportunities in the German education system. We test the feasibility of reform to tackle the issue of educational inequality with a series of survey experiments that show how information on educational inequality affects concerns about inequality and education policy preferences. The information treatment has a large effect on the general public's concerns about educational inequality, but only slightly shifts 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. The one policy with strong treatment effects is compulsory preschool, which entails commitment from disadvantaged families. We also show that information about the effectiveness of universal childcare increases support for compulsory preschool even further.

Chapter 5 shows that aspirations of students differ by parental background, a fact that is not mitigated by additional information on the returns and cost of education. Arguably, education outcomes depend both on institutional factors like those discussed in the third chapter, and on the effort and motivation of students (Hanushek, 1979). Therefore, we examine the role of information in determining demand for education. Prior research shows that children from highly educated families are much more likely to enroll in university, leading to a strong and persistent intergenerational correlation of education in many countries. We investigate whether ignorance about economic returns and costs of university education explains the gap in educational aspiration. Our survey shows that 74 percent of university graduates, but only 37 percent of those without a university degree aspire to university education for their children. It turns out that respondents without a university background are more likely to underestimate the earnings of university graduates as well as the available level of student aid. At the same time, they are more likely to overestimate the unemployment rate of university graduates and the level of tuition fees. However, correcting these beliefs through randomized information provision does not mitigate the gap in educational aspiration. Descriptive results furthermore suggest that the gap in aspiration would be equally large if respondents from different education background had similar information sets, suggesting that cost and benefits considerations are not the crucial factor for educational aspirations of parents. The findings of this chapter cast doubt on the hypothesis that misinformation explains educational inequality in Germany.

In general, the chapters in this thesis shed light both on the feasibility of education reform by documenting support for various policies and on the potential effects of information campaigns to shape the public preferences for education. For example, Chapter 4 shows that a number of education reforms have majority support among a representative sample of Germans, while the analysis in Chapter 3 suggests that information provision shifts the median preference. Similarly, Chapter 2 shows large effects of information provision with implications for the political economy of education spending. Yet, results on the effects of information treatment in general are nuanced: both the effects of information on the extent of educational inequality in Chapter 4 and of information on returns and cost of university education in Chapter 5 had limited effects on the public's preferences. Overall, this thesis paints an encouraging picture of public preferences for education, which seem to support a well-funded education system and meaningful reform of education policy—providing scope for policy-makers to work towards building an efficient and equitable German education system that is set up for a successful future.

2 How Information Affects Support for Education Spending[2]

Citizens in Europe and the United States differ widely in their views on various policies. Such transatlantic differences in policy preferences have been attributed to differences in culture, social beliefs, and political regulations and institutions (e.g., Alesina and Angeletos, 2005; Alesina et al., 2006). In this chapter, we focus on another potential source of differing preferences for government spending reforms: differences in current spending levels across countries and the extent to which citizens are informed about these spending levels. To shed light on how information on spending levels affects policy preferences, we conduct the first parallel randomized experiments within representative surveys of public opinion in the two largest industrialized nations in the western world, Germany and the United States.

We focus on government spending on education, which comprises a large share of public sector budgets around the world. On average across developed countries, more than one in eight dollars that governments spend goes to education—more than on defense, public order and safety, and environmental protection combined (OECD, 2014).[3] The quality of education systems is a cornerstone for future national prosperity (Hanushek and Woessmann, 2015), and citizens' support for education spending is a critical factor in shaping public budgets.[4]

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[2] This chapter is joint work with Philipp Lergetporer, ifo Institute at the University of Munich and CESifo; Guido Schwerdt, Department of Economics, University of Konstanz, CESifo, and IZA; Martin R. West, Harvard Graduate School of Education, NBER, and CESifo; and Ludger Woessmann, University of Munich, ifo Institute, CESifo, and IZA. Also see CESifo Working Paper No. 6192 and CESifo Working Paper No. 5938. For helpful comments, we would like to thank Eric Bettinger, Kenny Martens, Andrei Shleifer, Erik Snowberg, Alois Stutzer, and seminar participants at Harvard, Konstanz, Mainz, the CPB in The Hague, the ifo Center for the Economics of Education in Munich, the congresses of the European Economic Association in Geneva, the European Society for Population Economics in Berlin, the Economics of Education Association in Madrid, and the CESifo Area Conference on the Economics of Education. Michael B. Henderson and Paul E. Peterson made important contributions to the design of the survey instruments and experiments. We thank Franziska Kugler for her collaboration in designing and executing the German survey. Financial support through the Leibniz Competition (SAW-2014-ifo-2) is gratefully acknowledged.

[3] In 2011, public spending on education was 12.9 percent of total public expenditure on average across the member countries of the Organisation for Economic Co-operation and Development (OECD, 2014, p. 257). The figure is 11.0 percent in Germany and 13.6 percent in the United States.

[4] The importance for public budgets holds irrespective of the debate over whether higher spending levels are essential to improve school quality (e.g., Hanushek, 2003; Jackson et al., 2016).

Education finance in Germany and the U.S. differs in a number of ways that might be expected to give rise to differences in public support for higher education spending. Among others, the U.S. has higher overall spending levels, but lower teacher salaries, than Germany. If public preferences fully determine policies, we would expect that respondents in both countries are equally satisfied with current policies. But if preferences are not fully aligned with spending levels, the observed differences in current spending patterns might lead Americans to be less inclined to support overall spending increases, but more inclined to support higher teacher salaries. In addition, the majority of school funding comes from the local level in the U.S. but from the state level in Germany. If local control aligns actual spending levels more closely with citizens' preferences, one would expect smaller effects of informing about actual spending levels in the United States. Yet we know very little about the public's knowledge of current education spending levels, the extent of support for increased spending, and the role of information in shaping public views in the two countries.

To investigate these issues, we surveyed representative samples of the adult populations in Germany and the U.S. and implemented three randomized experiments on how the provision of information affects support for education spending. The German and American surveys included more than 4,000 and 2,600 respondents, respectively. Within each survey, randomly selected subgroups were given different types of information before answering the same questions about the level and allocation of public spending on education. Earlier in both surveys, we asked respondents to estimate current levels of education spending and teacher salaries.

We find that a vast majority of the public in both countries underestimates current levels of school spending and teacher salaries. Absent the provision of information, an absolute majority in both countries supports increased government spending on education, with somewhat higher levels of support among Germans than Americans (71 percent vs. 60 percent).

Our first survey experiment shows that citizens of both countries react similarly to two information treatments, with treatment effects (relative to the control mean) hardly differing. Informing respondents about the current level of annual public education spending per student reduces support for increased spending by more than one quarter (to 50 percent in Germany and 43 percent in the U.S.). Additionally stating that the spending increase would be financed through higher taxation reduces support by more than half compared to the control group (to 30 percent in Germany and 26 percent in the U.S.), with the shares in support no longer differing significantly between the two countries.

Our second survey experiment examines preferences for increases in teacher salaries. In both countries, staff compensation makes up 81 percent of total current school expenditure (OECD, 2014, p. 284), making salary levels a crucial component of overall education spending levels. When respondents are informed about current salary levels, the share who support increases in teacher salaries declines by about 40 percent (relative to the control mean) in both countries, although baseline support is much lower in Germany. The latter difference, the only notable divergence in preferences that we observe between the two countries, is consistent with the fact that current salaries are higher in Germany. In fact, based on an estimated negative relationship between salary levels and support for salary increases across U.S. states, we suggest that Germany's higher current salary levels could account for more than two thirds of the German-U.S. difference in support for salary increases.

Further analysis confirms that these treatment effects reflect actual information effects, rather than simply the effect of being primed to think about monetary values as opposed to, say, observable conditions in local schools before reporting support for spending increases (Iyengar et al., 1984; Krosnick and Kinder, 1990). In both countries, treatment effects are substantially larger for respondents who underestimated actual levels and are almost zero for respondents who had already been well informed prior to the information treatment. The fact that treatment effects vary with prior information levels indicates that they at least partly reflect genuine effects of being better informed, rather than just priming. The heterogeneity by prior information levels is more pronounced for teacher salaries than for school spending, in line with the hypothesis that people have a better anchor when estimating salaries compared to spending levels. The significant heterogeneity is also particularly noteworthy given that we find virtually no evidence of heterogeneous treatment effects across population subgroups defined by gender, age, education, income, employment, and parental and minority status.

Our third experiment examines how preferences for specific categories of education spending react to the provision of information on the tradeoffs involved when choosing among them. When asked how best to allocate new education spending, respondents' preferences shift away from class-size reductions towards alternative spending categories—teacher salaries (particularly in the U.S.) and materials such as new books and technology (particularly in Germany)—if they are informed about what could be achieved in each category with the same amount of additional spending. Additional experiments included only in the German survey show that information on spending requirements also reduces support for specific education reforms and that the basic findings for educational spending generalize to other categories of public spending.

The broad pattern of results across the three comparative survey experiments suggests that, despite numerous institutional and political differences between the two countries, citizens' knowledge of and preferences over education spending are quite similar in Germany and the U.S., as are reactions to the provision of information. The overall level of preferences for increases in government spending on education, their substantial reduction when mentioning current spending levels and tax financing requirements, and the shift away from preferences for class-size reductions towards other spending alternatives when informed about the quantitative tradeoffs involved are all comparable across the two countries. The lone exception is Germans' lower preference for teacher salary increases, which is consistent with existing salary differences between the two countries.

Our results contribute to at least three strands of literature. First, a substantial comparative literature examines transatlantic differences in the public's policy preferences in different areas. In particular, Alberto Alesina and coauthors have studied U.S.-European differences in preferences for inequality, redistribution, and working times, concluding that preference differences are not so much related to differences in utility functions or culture as to differences in social beliefs, political institutions, and regulations (see Alesina and Angeletos, 2005 and Alesina et al., 2004 on redistributive policies; Alesina et al., 2001 on the welfare state; and Alesina et al., 2006 on working times). Our results analogously suggest that the views of Americans and Germans about education spending may be quite similar.

Second, several recent papers use survey experiments in individual countries to study how policy preferences respond to the provision of different kinds of information. For example, Di Tella et al. (2012) study effects of information treatments on preferences for privatization reforms in Argentina, Cruces et al. (2013) and Kuziemko et al. (2015) for redistributive policies in Buenos Aires and the U.S., respectively, Elias et al. (2015) for payments for human organs in the U.S., Schueler and West (2016) for education spending in the U.S., and Bursztyn (2016) for the tradeoff between education spending and cash transfers in Brazil. We extend this literature by using parallel survey experiments to compare how the same information treatments affect public opinion on education spending in two countries.

Third, our results advance our understanding of the political economy of education policy. They provide empirical underpinning for the mostly theoretical literature on the political economy of education spending (see, e.g., Glomm et al., 2011) and call for an extension of these theories towards incorporating citizens' misperceptions. Furthermore, our findings provide guidance as to the political feasibility of policies such as increasing teacher salaries or reducing class sizes (e.g., Woessmann and West, 2006).

In what follows, Section 2.1 provides a theoretical framework to guide interpretation of the information experiments, as well as background on the U.S. and German education systems. Section 2.2 introduces the data and the experimental design. Section 2.3 reports our main results concerning the effect of information provision on support for education spending and teacher salaries in the two countries. Section 2.4 demonstrates that German levels of support are consistent with the relationship between current salary levels and support for salary increases across American states. Section 2.5 provides evidence of heterogeneous treatment effects by respondents' prior information. Section 2.6 examines the effects on preferences of highlighting the tradeoffs between different spending categories. Section 2.7 assesses the robustness of our main results concerning information effects for specific education reforms and other categories of public spending. Section 2.8 discusses our findings and concludes.

2.1 Background

We start by providing theoretical and institutional context for the survey experiments described below. We first propose a theoretical framework based on a standard preference model that illustrates how the provision of information could influence policy preferences. We then offer background information on the American and German education systems and the sources and levels of education spending in the two countries.

2.1.1 Theoretical Framework

As a framework for our analyses, we consider a standard model that represents preferences as indifference curves for bundles of two goods between which a consumer is indifferent. In our case, the two goods are policies: spending on public education and other categories of government spending (see Figure 2.1 for a graphical exposition and Appendix A for a formal derivation). A budget line depicts how much of one good must be sacrificed to receive more of the other. Our analysis refers to the preferences of one individual who represents the median voter in a given country. There will be a distribution of preferences around that median voter, so changes in the size of preferred changes for this representative agent will ultimately translate into changes in the share of people in the country supporting spending increases or decreases.

The Effect of Status Information on Support for Policy Change We start with two assumptions about the median individual's knowledge and preferences, which are supported below by evidence from our survey data for both countries. First, the individual underestimates current levels of spending and teacher salaries. Second, without further infor-

mation, what the person perceives to be the current levels of spending and salaries are below her personal optimal levels, so that she prefers spending and salaries to increase.

This simple framework allows us to analyze the effects of the two types of information treatments in our survey experiments. The first type is informing respondents about current levels of education spending.[5] Let E_u in Figure 2.1 represent the individual's estimate of the current level of education spending. Her optimal policy choice is E_u^*, indicating that she favors a large increase in education spending (Δ uninformed).

If the person now is informed that the actual level of education spending is in fact not E_u but rather the higher level E_i, her optimal policy choice would be E_i^*, and support for increasing education spending declines (Δ informed < Δ uninformed). Put differently, E_i^* lies to the left of E_u^* projected onto the new budget line with constant levels of other spending. The intuition behind this shift in preferences is straightforward: The individual in the informed state is equivalent to an uninformed individual who received an increase in her budget and spent it all on education. Since under standard preferences she would prefer to spend a portion of this increase on other items, the allocation after the increase reduces her relative demand for additional education spending. In a setting of underestimation of current levels of education spending and preferences for increased education spending, one would therefore expect that informing about current levels of education spending would reduce support for spending increases.

If we change the axes in Figure 2.1 to represent teacher salaries (rather than overall education spending) vs. other spending (inside or outside the education system) and maintain the same assumptions, the analysis can similarly be interpreted in terms of providing information about current levels of teacher salaries, which is predicted to reduce support for salary increases.

Analyzing the effects of providing information on a single budget item like spending for schools or teacher salaries is useful in illustrating the potential consequences of transparency policies focused on these areas. More generally, the analysis illustrates how access to better information can shape citizens' policy preferences with respect to public spending, a topic that has been the subject of surprisingly little scholarly research. One should be cautious, however, in using such analysis to draw conclusions about how spending levels would need to change to better align with citizens' preferences. To the extent that respondents are also uninformed about areas of public spending beyond education, the

[5] We assume for now that the information treatment does not affect the individual's estimate of other spending or of the relative price of education.

analysis does not necessarily reveal the optimality of policy choices, and providing information on education spending may also affect estimates of spending levels on other items (see Appendix A for a formal treatment and section 2.7 for evidence on several categories of public spending).

The Effect of Tradeoff Information on Support for Policy Change The second type of information treatment informs about the tradeoffs between different categories of education spending, rather than about current spending levels. Consider the choice between the number of teachers per student (the inverse of class size) and other education spending in Figure 2.2. The dashed line shows the individual's uninformed beliefs about the tradeoff between class-size reduction and other spending categories. Informing the person about the actual tradeoff makes her aware that the true budget line has a steeper slope (i.e., smaller reductions in class size for increases in alternative spending than the person had expected). As a result, the optimal policy choice is one where class sizes are not reduced by as much as in the uninformed case (Δ informed $<$ Δ uninformed). Support for class-size reduction decreases, while support for increasing spending in other categories increases.

If we redefine the axes in Figure 2.2 to capture overall education spending (rather than teachers per student) vs. non-education spending, the picture can also be used to analyze the additional treatment in our first survey experiment designed to raise the salience of taxation: mentioning the need to raise taxes heightens the person's awareness that increasing education spending requires taking money from other (in this case private) uses. Making this tradeoff salient is therefore expected to reduce support for spending increases.

2.1.2 Background on the U.S. and German Education Systems

Education is currently the third largest category of government expenditure (after social protection and health care) in both Germany and in the U.S. (OECD, 2015, p. 73). Each country's public school system enrols the vast majority of students. In the U.S., the share of students who attend a private school is 8 percent from primary through to upper secondary school (OECD, 2014, p. 416). In Germany, it is 4 percent in primary school, 9 percent in lower secondary, and 8 percent in upper secondary school. However, the American and

German education systems also differ in a number of important respects, including the sources of education funding, spending levels, and teacher compensation.[6]

In both the U.S. and Germany, the federal government plays a secondary role in education governance. The distribution of responsibility for public education across different levels of government is reflected in the sources of education funding. In 2011, the federal government contributed 14 percent of total funding in the U.S. and 11 percent in Germany (OECD, 2014, p. 259). While only 35 percent of funding for American schools came from the states, this share was 72 percent in Germany, underscoring the dominant role of the *Länder* in education policy. In the U.S., 51 percent of funding comes from the local level, compared to 17 percent in Germany, giving local school boards a larger role in education governance. If the local nature of funding aligns spending levels more closely with the local optimum or makes residents better informed, the theoretical framework presented above would predict smaller effects of information treatments on preferences for education spending in the United States.

In addition to funding sources, spending levels also differ between the countries. According to OECD estimates, spending for primary education is 45 percent higher in the U.S. than in Germany, and 24 percent higher for secondary education (OECD, 2014, p. 215). In dollar terms, while the U.S. spent $10,958 per student in primary education and $12,731 in secondary education in 2011, expenditures in Germany were at $7,579 and $10,275, respectively. These numbers correspond to 3.7 percent of GDP in the U.S. and 3.0 percent of GDP in Germany (OECD, 2014, p. 230). Public expenditure on non-tertiary education accounts for 9.2 percent of total public expenditure in the U.S. and 6.7 percent in Germany (OECD, 2014, p. 257). All else equal, the theoretical framework would thus lead to the expectation of lower support for spending increases, as well as smaller absolute information treatment effects, in the United States.

The higher overall spending level in the U.S. is not reflected in higher average teacher salaries. Quite to the contrary, teacher salaries are substantially higher in Germany than in the U.S., both in absolute terms and relative to other workers with a post-secondary degree (OECD, 2014, p. 467-471). In primary school, the average annual statutory salary of teachers (after 10 years of experience) is 33 percent higher in Germany ($59,795) than in the U.S. ($44,995). The difference is even more pronounced in secondary school, where German

[6] The education systems also differ in other aspects such as a reliance on comprehensive high schools versus a tracked secondary school system, the prevalence of central exit exams, methods of school finance, and the allocation of educational resources. See Henderson et al. (2015) for a more extensive description of the historical, institutional, and cultural contexts of the education systems in the two countries.

teachers earn 48 percent more than U.S. teachers at the lower secondary level and 55 percent more at the upper secondary level. Comparing teacher salaries to the earnings of other workers with a tertiary education yields a similar picture: At the primary, lower secondary, and upper secondary levels, German teachers earn 88, 97, and 105 percent, respectively, compared to other college-educated workers. In the U.S., relative wages are substantially lower at 67, 68, and 70 percent, respectively. Average class sizes in the two countries are nonetheless quite similar in primary school at 21 students and slightly smaller in Germany (24 students) than in the U.S. (27 students) in lower secondary school (OECD, 2014, p. 450). Moreover, as noted above, the total share of current education spending devoted to staff compensation is 81 percent in both countries. Together with the higher spending levels and lower teacher salaries in the U.S., this implies that the U.S. spends considerably more than Germany on non-teaching staff. All else equal, one would expect higher support for salary increases and larger absolute information treatment effects in the United States.

2.2 Data and Methods

Our analysis draws on three randomized survey experiments embedded in representative surveys of public opinion on education policy that we conducted in the two countries.

2.2.1 Comparative Opinion Surveys in Germany and the United States

In spring 2014, we surveyed representative samples of the adult population in both countries about their opinions on education policy.[7] The German sample includes more than 4,000 respondents, and the U.S. sample used in this chapter more than 2,600. Both surveys were conducted primarily online, with additional provisions undertaken in each country to ensure representativeness also for residents without internet access. As mentioned, this assures that our samples represent the American and German voting-age populations, a crucial aspect in the framework of political economy theories such as median voter models.

[7] For a descriptive depiction of general patterns of public opinion on education policy in Germany and the U.S., see Henderson et al., 2015.

The German Survey This chapter relies on data from the ifo Education Survey 2014, a nationally representative, stratified sample of the German population aged 18 years and older.[8] The survey was conducted in German between April and July 2014.

Overall, the ifo Education Survey 2014 comprised 39 questions on education policy. These included the questions on education spending used in this chapter, which were coordinated with the U.S. survey. They also included three additional survey experiments that we present in section 2.7. The exact wording of each question can be found at www.cesifo-group.de/ifo-bildungsbarometer. Within the survey experiments, the maximum number of different treatment conditions was four.

The total number of observations in the German survey is 4,171 adults. The specific number of respondents to each question varies for two reasons. The first reason is item nonresponse, which is very low at 1 percent on average. The second reason is that on the spending question and on the tradeoff question, subgroups of respondents were assigned to additional Germany-specific treatment conditions that are not discussed here. Survey weights are employed throughout to ensure the sample's representativeness with respect to the national adult population.

The U.S. Survey The American survey drew a nationally representative, stratified sample of the U.S. population aged 18 years and older and representative oversamples of the following subgroups: public school teachers, African Americans, and Hispanics.[9] The sample was drawn from a nationally representative panel of adults, obtained via address-based sampling and list-assisted random digit-dialing sampling techniques, who agreed to participate in a limited number of online surveys. Individuals who do not have internet access were provided access by the polling firm. The survey was conducted in May and June 2014. Respondents could elect to complete the survey in English or Spanish.

Overall, the U.S. survey comprised 36 questions on education policy, including the questions coordinated with the German survey used in this chapter. The exact wording of each question is displayed at www.educationnext.org/edfacts. Within the survey experiments, the maximum number of different treatment conditions was three.

[8] The survey was implemented by the polling firm TNS Infratest (now called Kantar Public Germany); see www.tns-infratest.com/sofo/index_EN.asp.

[9] The survey was implemented by the polling firm Knowledge Networks (KN), a GfK company. Detailed information about the maintenance of the KN panel, the protocols used to administer surveys, and the comparability of online and telephone surveys is available at www.knowledgenetworks.com/GANP.

The total number of observations from the U.S. survey used in this chapter is 2,669 adults.[10] The specific number of respondents varies from question to question due to item nonresponse and the fact that, in the cases of opinion about school spending and teacher salary, the sample was randomly divided into multiple groups in order to examine the effect of variations in the way questions were posed. Survey weights are employed throughout to account for nonresponse and the oversampling of teachers and other demographic subgroups.

2.2.2 Experimental Design

In order to identify the effect of the provision of information on public support for education spending in the two countries, we implemented the same three randomized survey experiments in both countries. Within each survey, randomly selected subgroups were given different types of information before answering the same questions about their views on specific aspects of public education spending. Randomization was independent across the three survey experiments. As outlined above, this design allows us to identify the causal effect of the provision of the specific information on the public's preferences for education spending in a survey setting.

The first experiment relates to a question on preferences for overall increases in education spending.[11] The control group (Uninformed) was simply asked about their support for increases or decreases in government funding for public schools in their school district (U.S.) or nationally (Germany). Respondents could choose one of the following five answer categories: greatly increase, increase, stay about the same, decrease, or greatly decrease. There are two treatment groups. One treatment group (Informed) was first informed about the actual level of spending per student in their district (U.S.: $12,400 on average across the sample) or nationally (Germany: €6,400) and then asked the same question as the control group. After receiving the same information as the first treatment group, a second treatment group (Informed+tax) was asked whether taxes to fund public schools should increase or decrease. As is evident from Table 2.1, the samples are well balanced across the treatment conditions in terms of observable characteristics, indicating that randomization was successful in both countries.

In the other two experiments, there is only one treatment group each. The second experiment asked about support for increases or decreases in the salaries of public school

[10] The full U.S. survey had a sample size of 5,266 adults and included an additional experiment designed to estimate the impact of the provision of information on student achievement on policy views; the sample used in this chapter comprises the control group from that experiment.

[11] The exact wording of the questions in the two countries can be found in Appendix B.

teachers. In contrast to the uninformed control group, the treatment group was informed about the current level of teacher salaries in their state (U.S.) or nationally (Germany). The third experiment examined preferences for different categories of education spending. The control group was simply asked whether a planned increase in government spending should go to class-size reductions, teacher salary increases, or new books and technologies. The treatment group was informed that in the U.S. (Germany), reducing average class sizes by 3 students would cost roughly the same amount as increasing teacher salaries by 13 (15) percent or buying $10,000 (€20,000) in new books and technologies for each class every year before choosing one of these three options. The balancing table (Appendix Table A2.1) indicates that observable characteristics are again well balanced across control and treatment groups in the second and third experiments.

In addition to the survey experiments on opinions, we asked respondents in both countries to estimate the current levels of education spending and teacher salaries, respectively. These questions were asked much earlier in the surveys and were separated from the related opinion questions by a series of questions on other education policy topics. This design is likely to reduce the possibility of backfire effects where individuals might respond defiantly to belief corrections by reinforcing their initial position (Nyhan and Reifler 2010).[12]

In our analyses, we report results both with and without the following set of control variables available in both countries: gender, parental and employment status, income,[13] age,[14] and education.[15] As is evident from Table 2.1, a larger share of U.S. than German respondents are parents and employed. The U.S. population is also somewhat younger. In addition, it has a larger share of people with college degrees, whereas the German population has a larger share of apprenticeship degrees. The available information on minority status differs between the two countries: in the U.S., it refers to the non-white population, whereas in Germany, it refers to having a migration background (i.e., both parents not

[12] Separating the belief elicitation from the information treatment makes the correction of false beliefs less immediate for the respondents and thus reduces the chance of such a behavioral response. In our surveys, respondents did not have the option to go back in the surveys to review or alter their responses to earlier questions.

[13] Income is measured as percentile rank within each country because income was surveyed in different ways in the two countries that correspond to the most common way of reporting income. In the U.S. survey, income refers to annual pre-tax household income. In the German survey, income refers to monthly household income net of taxes and social security contributions.

[14] To allow for nonlinearities in the relationship between age and preferences, we collapse our age variable into four categories: 18-34, 35-49, 50-64, and 65+.

[15] To make the different degrees in the two countries comparable, education is measured in three categories: less than high school, high school but no B.A., and B.A. degree or higher.

born in the country). We therefore report the minority variable in Table 2.1 but omit it as a control variable in our experimental analyses due to the lack of comparability across countries.

2.3 The Effect of Information Provision on Support for Education Spending and Teacher Salaries in Germany and the U.S.

Our first two experiments study the effect of information provision on support for increases in education spending and in teacher salaries, respectively. In both cases, respondents in the treatment and control conditions were asked to indicate their preferences on a five-point scale ranging from "greatly decrease" to "greatly increase," with the middle category indicating that spending or salary levels should "stay about the same." To simplify presentation, our main estimations of treatment effects are based on linear probability models where the top two and bottom three response options are collapsed to create a binary indicator of support for a spending or salary increase; we report conventional OLS standard errors. We also show results in which the outcome is defined as the probability of support for decreased spending or salaries.[16]

2.3.1 Preferences for Education Spending

In the control group that did not receive any specific information, 60 percent of the U.S. population and 71 percent of the German population favors government funding for public schools to increase or greatly increase (Figure 2.3). Note that the lower support for increased spending in the U.S. compared to Germany is consistent with the fact that spending levels are currently higher in the U.S. than in Germany, independent of whether they are measured per student, relative to GDP, or as a share of total public spending. A descriptive analysis of who supports higher spending in Table 2.2 shows that in the U.S., females and poorer people are more likely to be in favor of spending increases and in Germany, respondents with university degrees are more supportive of spending increases.

Table 2.3 reports how the two information treatments affect support for education spending (see also Figure 2.3). In each country, both treatments substantially reduce support for increased spending. The strong majority in favor of spending increases turns into a minority of less than one third among those who are informed about current spending levels and

[16] Supplemental analyses available upon request confirm that using ordered probit models with the dependent variable coded 1 for "greatly decrease" to 5 for "greatly increase" yields qualitatively identical results to those reported in Tables 2.2-2.6.

told that increased spending would be financed through a tax increase. In this treatment group, preferences for spending increases no longer differ significantly between the two countries. Being informed about current spending levels reduces support for increased spending by 17 percentage points in the U.S. (column 1) and by 20 percentage points in Germany (column 2). Treatment effects are even larger in the second treatment that also raises the salience of tax financing: This reduces support by 34 percentage points in the United States and by 41 percentage points in Germany. That is, in both countries the information treatment reduces support for increased spending by more than a quarter and the additional tax treatment reduces it by more than half.

Pooling the data from both countries (column 3) reveals that the effect of the information treatment does not differ significantly between the two countries, whereas the effect of the combined information and tax treatment is 7 percentage points larger in Germany than in the United States. If the willingness to accept marginal tax increases is negatively related to status quo tax levels, Germans' stronger reaction to the tax treatment could plausibly be related to the fact that the tax burden is higher in Germany than in the United States (OECD, 2016c). Results do not change qualitatively when controls are included (column 4), indicating that neither the difference in support levels nor the differences in the treatment effect are due to underlying demographic differences in the populations of the two countries. The same is true when the controls are additionally interacted with a country indicator (column 5). This model allows the associations of the control variables with support for spending increases to differ across countries. Thus, to the extent that differences exist between the two countries, they cannot be attributed to differences in background characteristics such as age, gender, education, income, employment status, parental status, and migration status or in their association with opinions.

Column 6 reports the same model with an indicator for support for spending decreases (rather than increases) as dependent variable (with the residual category being respondents who prefer spending levels to stay about the same). While the control mean is low in both countries (7 percent in the U.S. and 4 percent in Germany), the information treatment raises support for decreased spending by 3 percentage points on average in the two countries and the information-plus-tax treatment raises support for decreased spending by 11 percentage points. Neither of the two treatment effects differs statistically significantly between the two countries.

Replication Study As a robustness check, we administered a slightly revised version of the experiment to new representative samples of respondents in both countries in a follow-up survey in 2015. We randomly assigned respondents in both countries (1,986 in the U.S. and

1,984 in Germany) either to the control group or the information treatment group. In contrast to the 2014 survey, respondents in Germany were informed about the average annual spending levels per student in their respective *Land*, which varied between €5,800 (in North Rhine-Westphalia) and €8,700 (in Thuringia). The provision of state-level information is particularly relevant for Germany because of the *Länders*' dominant role for education policy and funding (see section 2.1.2).

Results of the follow-up survey are presented in Appendix Table A2.2. The control mean of support for increased school spending is 61 percent in the U.S. and 74 percent in Germany. As in the 2014 survey, being informed about current spending levels reduces support for spending increases substantially in both countries (columns 1 and 5). In the U.S., the information reduces support by 16 percentage points and in Germany by 15 percentage points. The fact that the information treatment effect for Germany, while still substantial and highly significant, is somewhat smaller in the follow-up survey than in the 2014 survey, might be due to the presentation of state information rather than national information. In any case, the 2015 survey results confirm the negative effect of informing respondents about current spending levels on preferences for higher spending in both countries.

2.3.2 Preferences for Teacher Salaries

The second survey experiment examines how the provision of information influences public support for one specific category of education spending: teacher salaries. In contrast to the relatively abstract concept of per-pupil spending levels, people are likely to have clearer benchmarks drawn from their own personal experience when asked to evaluate whether a given level of compensation is appropriate (an assertion we will return to in section 2.5 below).

In the uninformed control group, 62 percent of Americans support increases in teacher salaries, as compared to just 29 percent of Germans (Figure 2.4). This difference is consistent with the evidence presented above that teacher salaries are substantially higher in Germany than the U.S., both in absolute terms and relative to other college graduates. Note that the pattern is exactly the opposite from that observed for spending levels (which were lower in Germany, with Germans more supportive of spending increases), suggesting that the between-country differences in public preferences do not reflect overall differences in the level of education spending or in the extent of support for increased educational inputs. In describing supporters for salary increases, Table 2.2 shows that minorities and those with a college degree are more likely to support teacher salary increases in the U.S.; females and older people are less likely to support increases in Germany.

Table 2.4 reports the results of the survey experiment examining how informing respondents about current levels of teacher salaries affects support for salary increases (see also Figure 2.4). The information treatment reduces support for teacher salary increases by 24 percentage points in the U.S. and by 12 percentage points in Germany. The difference in the absolute magnitude of the treatment effects of 12 percentage points is statistically significant and unaffected by the inclusion of control variables (columns 3-5). Likewise, conditioning on the control variables does not qualitatively affect the German indicator, suggesting that the lower support for salary increases cannot be attributed to demographic differences between the two countries.

In the uninformed control group, relatively few people support decreases in teacher salaries (6 percent in the U.S. and 8 percent in Germany). In the U.S., this is unaffected by the information treatment (column 6). In Germany, however, support for decreasing teacher salaries grows by 8 percentage points when people are informed about current salary levels.

2.3.3 Heterogeneous Treatment Effects by Demographic Subgroups

Table 2.5 reports the two treatment effects of the school spending experiment and the treatment effect of the teacher salary experiment for subgroups defined based on the following characteristics: gender, parental status, employment status, minority status, income, age, and education. There are hardly any significant treatment heterogeneities present either in the U.S. or Germany. In particular, treatment effects are significant in all cases, and they rarely differ across the subgroups. For example, none of the treatment effects differs in a noteworthy way for people with different levels of income, age, or education. Neither do the information treatments act differently for different ethnic groups in the United States. The only cases where treatment effects differ statistically significantly across subgroups are the combined information-plus-tax treatment in Germany by parental and minority status and the treatment effect in the salary experiment in the U.S. by parental status and income level. We refrain from offering substantive interpretations of these differences due to the large number of subgroup comparisons and the associated risk of false conclusions.

In the 2015 follow-up survey in Germany, we surveyed two additional background characteristics that provide information on respondents' voting behavior and the importance of education topics for their personal voting decisions. For respondents who usually cast a vote at elections and those who consider education topics important for their vote choice, we find that treatment effects of the school spending experiment are smaller but remain

large and significant (not shown). This highlights the importance of our results for the overall political economy of majorities for public school spending.

2.4 Explaining Differences between Germany and the United States

Both in Germany and the United States, informing citizens about current levels of education spending and teacher salaries significantly reduces support for spending and salary increases. Likewise, making tax financing requirements salient further reduces support for increased spending in both countries. However, in absolute terms, the tax treatment effect is significantly larger in Germany and the effect of the salary information treatment is significantly smaller. Does this reflect differences in the composition of the populations, differences in the status quo policies in the two countries, or genuine differences in public preferences? We have already shown that treatment effects hardly budge when controlling for demographic differences between the populations. We have also noted that the direction of the differences across countries is consistent with citizens of the two countries reacting in similar ways to divergent policy environments.

Here, we use two approaches to provide additional evidence suggesting that the differences between the two countries reflect differences in current policy. As indicated, current spending levels are lower, but teacher salary levels are higher, in Germany than in the United States. Consistent with the notion that differences in current policies likely drive differences in public opinion, Germans in the control conditions are more likely to support spending increases, but less likely to support salary increases.

Thus, a first approach is to look at treatment effects relative to the extent of baseline support for increases in the two countries in the absence of any treatment. As is evident from the bottom parts of Tables 2.3 and 2.4, treatment effects relative to control means are quite similar in the two countries: Informing about spending levels reduces support for spending increases by 28 percent of the untreated support level in the U.S. and by 29 percent in Germany. The combined information plus tax treatment reduces support for spending increases by 57 percent of the control mean in the U.S. and by 58 percent in Germany. And relative to the respective control means, the information treatment reduces support for teacher salary increases by 39 percent in the U.S. and by 42 percent in Germany. This pattern indicates that differences in absolute treatment effects are directly related to differences in uninformed levels of support for policy changes, which may in turn relate to differences in current policies.

To examine this possibility more formally, we link the analysis directly to current differences in policy levels. To do so, we make use of the variation across U.S. states. We focus the analysis on opinions on teacher salaries, where differences are starkest between the two countries. The analysis proceeds in two steps: First, using our U.S. dataset, we estimate the association between current levels of teacher salaries and support for salary increases across U.S. states. Second, from that association we project the level of support we would expect at the German teacher salary level and compare this to Germans' actual support for salary increases.

Figure 2.5 illustrates this projection analysis. It confirms that higher average salary levels are systematically related to less support for salary increases across U.S. states. The underlying regression (which includes our standard set of control variables) indicates that an increase in average monthly teacher salaries by $1,000 is associated with a decrease in support for increased salaries by 0.8 percentage points (*p*-value 0.015). In units equivalent to the U.S. salary data, current teacher salaries in Germany are at $85,450.[17] Based on a linear projection from the association across U.S. states, at this salary level we would expect 39 percent support for salary increases, just 10 percentage points higher than the actual support of 29 percent. In other words, this exercise suggests that differences in current salary levels might account for as much as 69 percent of the difference between Germany and the U.S. in mean support for teacher salary increases.[18]

In this sense, even the German results on preferences for teacher salaries are consistent with the U.S. results: The lower uninformed support level for salary increases is in line with the higher current salary levels in Germany. And relative to this uninformed support level, the information treatment effect in Germany is very similar to the one in the United States. Differences in the absolute size of the treatment effects thus appear to be mostly attributable to differences in the policy status quo in the two countries.

[17] To translate German salary levels into the units available for U.S. states (drawn from the U.S. Department of Education's Digest of Education Statistics 2012), we take the relative salary level of Germany compared to the U.S. on an internationally comparable scale from the OECD, 2014, p. 467 (using the statutory salary for lower secondary teachers after 10 years), which stands at 148 percent of the U.S. level, and inflate the national average salary level in our U.S. data by this amount.

[18] Using salary levels at the upper rather than lower secondary level, where German salaries stand at 155 percent of the U.S. level, the expected support would be even closer to the true value at 36 percent, with salary differences accounting for 78 percent of the U.S-German support difference.

2.5 Information vs. Priming: Heterogeneity by Prior Knowledge

A potential concern with the evidence presented so far is that the apparent treatment effects of information could reflect respondents having been primed to think about the topic of education spending in terms of financial values, as opposed to respondents becoming better informed. To shed light on this issue, we estimate whether treatment effects differ by respondents' prior knowledge of spending and salary levels.[19]

To be able to do so, we asked respondents to report their best estimates of current levels of school spending and teacher salaries. In both countries, these estimation questions were asked early in the survey and separated from the respective opinion questions by a number of questions on other education policy topics.

Figure 2.6 shows distributions of the public's estimates of spending and salary levels in the two countries (expressed relative to the respective actual levels). Two patterns stand out. First, a majority of the public in both countries tends to underestimate actual levels of school spending and teacher salaries. In the U.S., the median estimate of spending per student is $3,000, which is 24 percent of the actual average spending level of $12,400. In Germany, the median estimate is €1,500, or 23 percent of the actual average spending level of €6,400.[20] Second, in both countries the distribution of estimates of salary levels comes close to a normal shape, whereas the distribution of estimates of spending levels, in addition to being further from the actual value, is more diffuse.[21] This might indicate that people do not have a clear anchor when estimating current levels of educational spending per student, whereas they have a better anchor (from knowing their own salary or salary levels in general) when estimating what teachers earn. Therefore, estimates of teacher salary levels may be more informative and include less noise than estimates of school spending levels.

Thus, focusing first on the teacher salary experiment, we analyze whether the information treatment effect differs by respondents' prior knowledge of actual teacher salary levels.

[19] This approach to distinguishing information and priming effects was developed in an observational setting by Lenz (2009) and has been applied in survey experiments by Cruces et al. (2013), Clinton and Grissom (2015), and Schueler and West (2016).

[20] In both countries, the leftmost bar of the histogram is the highest one, containing estimates below 8.8 percent of the actual value in the U.S. and below 8.3 percent in Germany.

[21] The spikes in the distribution of German estimates reflect that respondents tend to answer in multiples of 5000 (spending) or 500 (salary) and that the denominator (the actual levels) is a national constant in Germany (but not in the U.S.).

The key result is displayed in Figure 2.7, which plots a linear estimate of how the probability to support increased salaries depends on the prior guesses of teacher salary levels, separately for the control group and for the treatment group.[22] In the uninformed control group, there is a clear tendency for respondents who had initially underestimated actual salary levels to be more likely to support salary increases. In the treatment group that is informed about actual salary levels, this association turns around.

The difference between the two lines illustrates the dependence of the information treatment on prior guesses. In both countries, the treatment effect is largest for those who had initially underestimated actual salary levels the most: Once these people learn that teachers actually earn much more than they had thought, their willingness to support salary increases drops by a large amount. This treatment effect of information in terms of decreasing support for increased salaries decreases (in absolute terms) as the extent to which people underestimate actual salary levels decreases.

For people who had roughly the correct guess about teacher salary levels, the treatment effect is no longer statistically significantly different from zero. For them, the information provided was already known, and indeed there is no treatment effect. Finally, for people who had initially overestimated teacher salaries, the treatment effect turns in the other direction: Having thought that teachers earned more, now being informed about the actual (lower) levels makes these people more willing to support salary increases, and significantly more so in Germany.

The right panel of Table 2.6 shows these results in regression form. The treatment effect of informing respondents about actual levels of teacher salaries shows a strong positive interaction with the extent to which respondents had initially underestimated salary levels in both countries. For respondents with low estimates of teacher salary levels, the information treatment (revealing that teachers in fact earn more than they had thought) significantly reduces the probability of favoring salary increases. For respondents who had roughly had the correct knowledge of actual teacher salaries (with an estimate roughly equal to the actual level), this information treatment effect is close to zero. And for respondents whose initial estimate was much higher than actual teacher salary levels, the information treatment (revealing that teachers in fact earn less than they had thought) turns positive, so that they are now more likely to favor salary increases. The pooled model indicates that both the negative treatment effect for under-estimators and the positive

[22] So as not to be driven by extreme outliers, we trim observations whose guesses were below one third of and above three times the actual level in this analysis.

treatment effect for over-estimators are smaller (in absolute terms) in Germany than in the U.S. when under- and over-estimation is measured in percent of the actual salary level.

For the school spending experiment, results from a specification that includes a linear interaction between the information treatment variable and respondents' estimates of current spending as a percentage of actual levels are less clear. This is consistent with our suggestion that estimates of spending levels—for which there is no readily available anchor on which to fix estimates—contain more noise than estimates of salary levels. However, the first three columns of Table 2.6 confirm that a similar pattern of heterogeneity emerges from a simple specification that interacts the treatment effect with an indicator of whether respondents' initial guesses had been above the actual value. In both countries, the effect of information is strongly negative for the larger group of respondents who had initially underestimated spending levels, whereas it is significantly smaller (in absolute terms) and close to zero for those who had initially overestimated spending levels. In the pooled model, the treatment effect is 18 percentage points smaller (in absolute terms) for those who initially overestimated actual spending levels, and this difference by prior knowledge status does not differ significantly between the two countries.

A potential concern with asking respondents to guess spending levels is that doing so creates a situation in which the information treatment explicitly corrects the false initial guesses offered by most respondents. Since respondents might feel obliged to state spending preferences that are consistent with the new information, the reported treatment effects might be due to so-called experimenter demand effects rather than genuine information effects. Presumably, this reaction to the correction of false beliefs should be particularly prevalent among individuals who were very sure about the accuracy of their estimate as compared to those who were very unsure. We test this proposition in our 2015 follow-up surveys (see section 2.3.1), where we again asked respondents for their best estimate of current spending levels and, in addition, asked how sure they were about their guess.[23] This allows us to test whether the information treatment effect differs by the degree of initial uncertainty about the guess. Columns 2-4 and 6-8 of Appendix Table A2.2 show that, in both countries, the information treatment effect does not vary significantly with the uncertainty of the prior guess. This finding is inconsistent with an interpretation where demand effects triggered by the correction of false beliefs are a main driver of the results. The likely absence of experimenter demand in our setting is further corroborated by evidence showing that online surveys are less susceptible to experimenter demand ef-

[23] Respondents indicated their certainty on a seven-point scale from "very unsure" to "very sure".

fects than more traditional survey modes (see, for instance, Kreuter et al., 2008; van Gelder et al., 2010).

In sum, in both countries treatment effects of informing people about actual levels of school spending and teacher salaries are strongly heterogeneous by respondents' prior information status about spending and salary levels. If all that the treatment did was to prime respondents to think about spending and salaries in terms of dollar or euro values, treatment effects should not differ by prior knowledge. The fact that they do indicates that they do not solely capture priming effects, but in fact also reflect effects of people becoming better informed. The stark heterogeneities in treatment effects by prior knowledge of spending and salary levels are especially noteworthy in light of the fact that treatment effects for the same experiments hardly differ across demographic groups.

2.6 Evidence on Tradeoffs between Different Spending Categories

To study support for education spending in greater detail, our third survey experiment examines the effect of providing information on the tradeoff involved between different spending options. In particular, supposing that the government plans to increase spending in the school system, respondents are asked to choose one of the following three options: reducing class sizes, increasing teacher salaries, and purchasing new books and technologies.

In the uninformed control group, a plurality of respondents in each country favors class-size reductions over the other options: 46 percent in the U.S. and 64 percent in Germany (Figure 2.8). Consistent with the second experiment, fewer Germans (4 percent) than Americans (24 percent) favor teacher salary increases. Roughly one third of respondents in both countries favor the purchase of new books and technologies.

The treatment group is informed that in the U.S. (Germany), reducing average class sizes by 3 students would cost roughly the same amount as increasing teacher salaries by 13 (15) percent or buying $10,000 (€20,000) in new books and technologies for each class every year before choosing one of these three options.[24] This treatment reduces support for class-size reductions in both countries, to 35 percent in the U.S. and 48 percent in Germany. While in the U.S. support mainly shifts towards increasing teacher salaries (33 percent), in Germany it mainly shifts towards buying new books and technologies (46 percent).

[24] The differences in the calculated tradeoffs in the two countries are consistent with slightly smaller class sizes and substantially higher teacher salaries in Germany (see section 2.1.2 above).

Table 2.7 reports treatment effects on support for the different spending options as relative risk ratios estimated by multinomial logit models. In both countries, the tradeoff information significantly reduces the relative risk of choosing class-size reductions over teacher salary increases. While the treatment effect is larger in Germany, the difference between the two countries is not statistically significant. Given the difference in baseline support for teacher salary increases, the treatment effect of tradeoff information on the relative support for increased teacher salaries compared to new books and technologies also does not differ significantly between the two countries. Only the difference in the reduction in support for smaller classes over new books and technologies between the two countries reaches statistical significance, with the relative risk ratio being smaller in Germany.

In sum, support for education spending is affected not only by information on spending levels, but also by information on the tradeoffs involved between different categories of education spending. In particular, once aware of these tradeoffs, the public in both countries expresses far less enthusiasm for class-size reductions. In the informed group, support is rather evenly distributed over the three options in the U.S. and closely split between class-size reductions and purchase of new teaching material in Germany.[25]

2.7 Further Robustness Analyses

Evidence from three additional experiments implemented in the German survey provides further insight into the robustness of our main findings concerning the role of information in public support for education spending (see Appendix C for details on these experiments).[26] Two experiments test whether spending information also affects expressed preferences for specific education reforms. The third experiment investigates whether information effects generalize beyond education to other areas of public spending.

[25] Ongoing research investigates the extent to which the effect of the tradeoff information may reflect preference nonlinearities rather than genuine information effects in the German setting.

[26] Due to space constraints, these robustness experiments were not implemented in the U.S. survey. Additional details on these German experiments are provided in CESifo Working Paper 5938, one of the two working papers that are now combined into this chapter.

2.7.1 Preferences for Specific Education Reforms

As documented in the literature, survey responses can be sensitive to whether policy questions are posed in a general or specific way.[27] In two experiments, we therefore test whether information treatment effects are also evident when expenses are tied to well-defined education policy proposals. We investigate preferences for two reforms widely discussed in Germany: whole-day schooling and abolishing grade retention.

German students typically attend school only until lunchtime, generating frequent proposals to extend the school day into the afternoon. According to one study, implementing whole-day schooling would cost more than €9 billion a year. The left panel of Appendix Table A2.3 shows that providing this information reduces support for the introduction of a whole-day school system from 61 to 55 percent.

In contrast to whole-day schooling, grade retention has long been integral to the German education system, but there are regular proposals to end the practice. This provides an opportunity to test whether information affects not only situations where spending would increase (as in the case of the experiments on school spending, teacher salaries, and whole-day schooling), but also situations where a proposal would reduce spending. Respondents are asked whether they favor requiring low-performing students to repeat a grade. The treatment group is informed that grade repetitions have been estimated to cost the German school system almost €1 billion each year. This information treatment reduces support for the policy, albeit only slightly by 4 percentage points from the control mean of 78 percent who are in favor of grade repetition (right panel of Appendix Table A2.3).

Overall, we therefore find that cost information affects not only general preferences for spending on education, but also preferences for specific policy proposals with implications for spending.

2.7.2 Other Categories of Public Spending

Finally, in addition to investigating preferences for education spending, we also tested whether similar information treatment effects exist in other areas of public spending. In particular, our third robustness experiment provides information on current spending lev-

[27] For instance, in their seminal work, Free and Hadley (1967) show that when asking general questions about the appropriate scope of government, half of the American public can be classified as “conservatives” while only 16 percent are classified as “liberals”. In contrast, when asking survey questions about specific activities of the government, 65 percent are classified as “liberals” and only 14 percent are classified as “conservatives.”

els in each of five major areas of public expenditure: social security, education, public safety, defense, and culture. As is evident from Appendix Table A2.4, providing information on current spending levels significantly reduces support for increased spending in all five areas, corroborating our earlier findings.

Interestingly, baseline support for spending increases is higher in education than in the other areas. While the treatment effect is also largest for education spending in absolute terms, it is largest for defense spending when viewed relative to the control mean. Note that while spending information is provided in per-capita terms in our main analysis, here it is provided in absolute terms as total spending in the country, indicating that the treatment effect does not depend on the specific format in which the information is presented.

In sum, at least in Germany, the negative information treatment effects of our main analysis do not hinge on the level of abstraction of the policy question at hand or on the specific format in which the information is provided. Further, the basic result generalizes to other areas of public spending.

2.8 Discussion and Conclusions

In a series of survey experiments, we examine how various forms of information affect public opinion concerning education spending in Germany and the United States. Our results show that in both countries, providing the public with information on current spending levels and clarifying the tradeoffs involved in allocating educational resources changes preferences on education spending. In particular, information on current levels of education spending and teacher salaries reduces support for spending and salary increases for the public as a whole and within virtually all demographic subgroups. In both countries, these effects are confined largely to individuals who had underestimated actual spending and salary levels, indicating that they reflect genuine effects of becoming better informed. In addition, telling the public that increased education spending would be financed through higher taxes further reduces support. Finally, informing the public about the tradeoffs involved between different spending options reduces support for class-size reductions in favor of teacher salary increases (mostly in the U.S.) and purchase of new teaching material (mostly in Germany).

These experimental results are consistent with the preference model used to motivate our analysis. Our descriptive data indicates that, in both countries, the public on average underestimates current levels of spending and salaries and—in the absence of information—tends to favor spending and salary increases. The preference model therefore predicts

that providing information about current levels of spending and salaries will reduce support for increases; that making the implications of increasing education spending for resource uses outside education salient will also reduce support for increases; and that information on tradeoffs between spending options will affect their relative popularity.

In this general sense, the public in Germany and the U.S. responds quite similarly to the provision of information on education spending. Relative to the baseline support for spending and salary increases in the uninformed group, the information treatment effects are remarkably consistent across the two countries. The country with higher current levels differs for overall education spending and teacher salaries—and so do our results on which country exhibits greater support for increasing these levels. And while current teacher salary levels are substantially higher in Germany than in the U.S. on average, Germans' level of support for salary increases is very much in line with what would be predicted based on the negative relationship between salary levels and support for increases observed across American states.

These similarities in citizens' reactions to information treatments may be surprising in light of research emphasizing transatlantic differences. For example, the two countries we study have adopted very different institutional structures for their societies, with Germany considered to be the leading exemplar of the conservative welfare state and the U.S. the prototype of liberal capitalism (e.g., Esping-Andersen, 1990; Hall and Soskice, 2001). Other work has emphasized how differences between the U.S. and Europe in political institutions, market regulations, and social beliefs about matters such as the extent to which effort influences economic outcomes lead to sharp differences in policy preferences (see Alesina et al., 2001, Alesina et al., 2006; Alesina et al., 2004; Alesina and Angeletos, 2005; Linos and West, 2003). Despite these differences, our results suggest that German and American citizens' preferences with respect to education spending are quite similar, as are their responses to the provision of information.

The provision of more direct evidence for the notion that public preferences are the same across countries is of course complicated by the fact that preferences are likely to be influenced by the policies that are currently in place, which differ across countries. However, as mentioned earlier, our surveys indicate that Germans and Americans provide very similar responses when asked about the benefits of education: 97 percent of the public in both countries stated that the academic performance of high-school students is "very" or "somewhat" important for the country's future prosperity (on a four-point-scale ranging from "not important at all" to "very important"). This similarity in perspective on academic performance lends credibility to our suggestion that preferences are, in fact, similar

across countries and that the differences in documented policy preferences are largely due to differences in the policy status quo.

In terms of policy implications, our results first illustrate how information matters in shaping policy preferences. Citizens in both countries systematically underestimate what is currently spent on education and, once provided with accurate information, update their preferences accordingly. Our results therefore cast doubt on whether uninformed opinions accurately reflect public preferences concerning the size of the education budget. At the most basic level, this implies that public opinion polls are likely to be an unreliable guide for policymakers seeking information on citizens' true preferences with respect to education spending (Althaus, 1998). Surveys that incorporate information about status quo spending levels across multiple policy domains and clarify the implications of proposed changes for tax burdens are apt to produce a more reliable barometer of public sentiment.

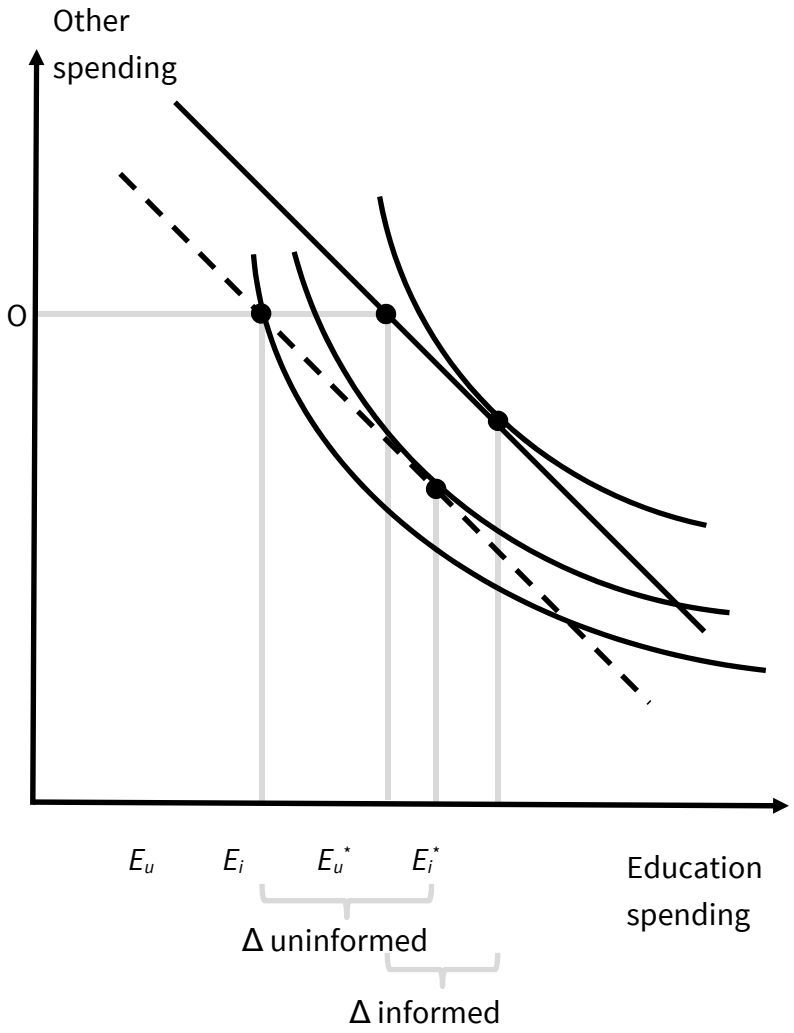
The results also shed light on the potential consequences of proposals prevalent in both countries to improve information levels by increasing transparency about education spending, for example by incorporating spending information into educational accountability systems (see, e.g., Boser, 2011). In the U.S., a new requirement to publish school-level data on per-pupil spending in annual school report cards was incorporated into the federal Elementary and Secondary Education Act in its 2015 reauthorization in an effort to promote efficiency in the use of public resources and expose inequities in the distribution of spending across schools. Providing citizens with more information on education policy is also high on the agenda in Germany. In 2006, the meeting of the federal and state ministers of education has instituted a biannual national education report that reports on leading indicators in different educational areas, including education spending, in increasing detail. Over recent years, several of the individual *Länder* have also started to publish their own education reports, such as Schleswig-Holstein in March 2017.

Our results suggest that such proposals to increase transparency may make it more difficult to sustain public support for increased education spending. The importance of information does not mean, however, that efforts to improve transparency about government budgets would necessarily undermine public support for education budgets. First, in no case do we see a majority emerge in favor of decreasing overall spending levels. Second, our robustness analyses suggest that citizens may be equally misinformed about spending levels in other policy domains. Providing them with similar information on how much the government spends for other purposes (e.g., health care or incarceration) might well change respondents' perspective towards education. Our results do indicate, however,

that equipping the public with better information about government budgets could alter their views and help align spending levels with citizen's preferences.

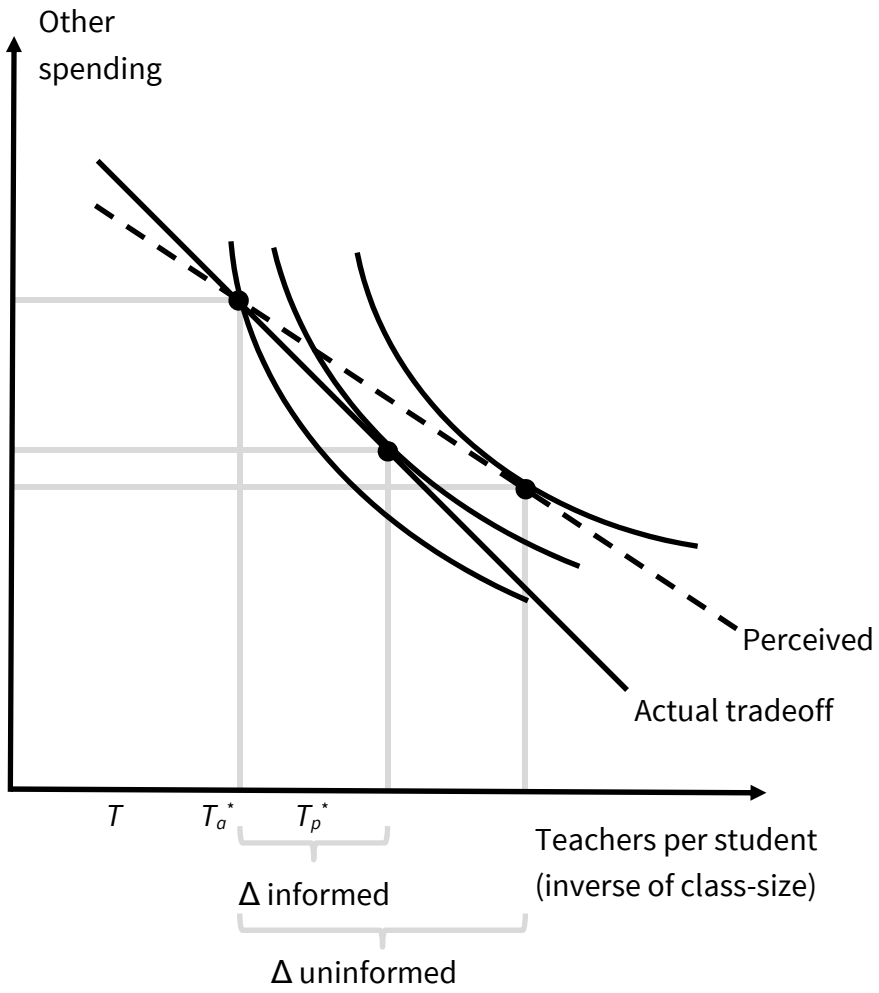
Our results also have a bearing on policies governing the allocation of education resources. Specifically, they suggest that in the absence of information on the relative cost of different spending options, people in both countries will support greater investment in class-size reduction than would be the case if they understood its opportunity costs. In that sense, informing the public about the tradeoffs between different forms of education spending may provide politicians with leverage to reallocate resources within the education system.

Figure 2.1: The effect of status information on support for policy change



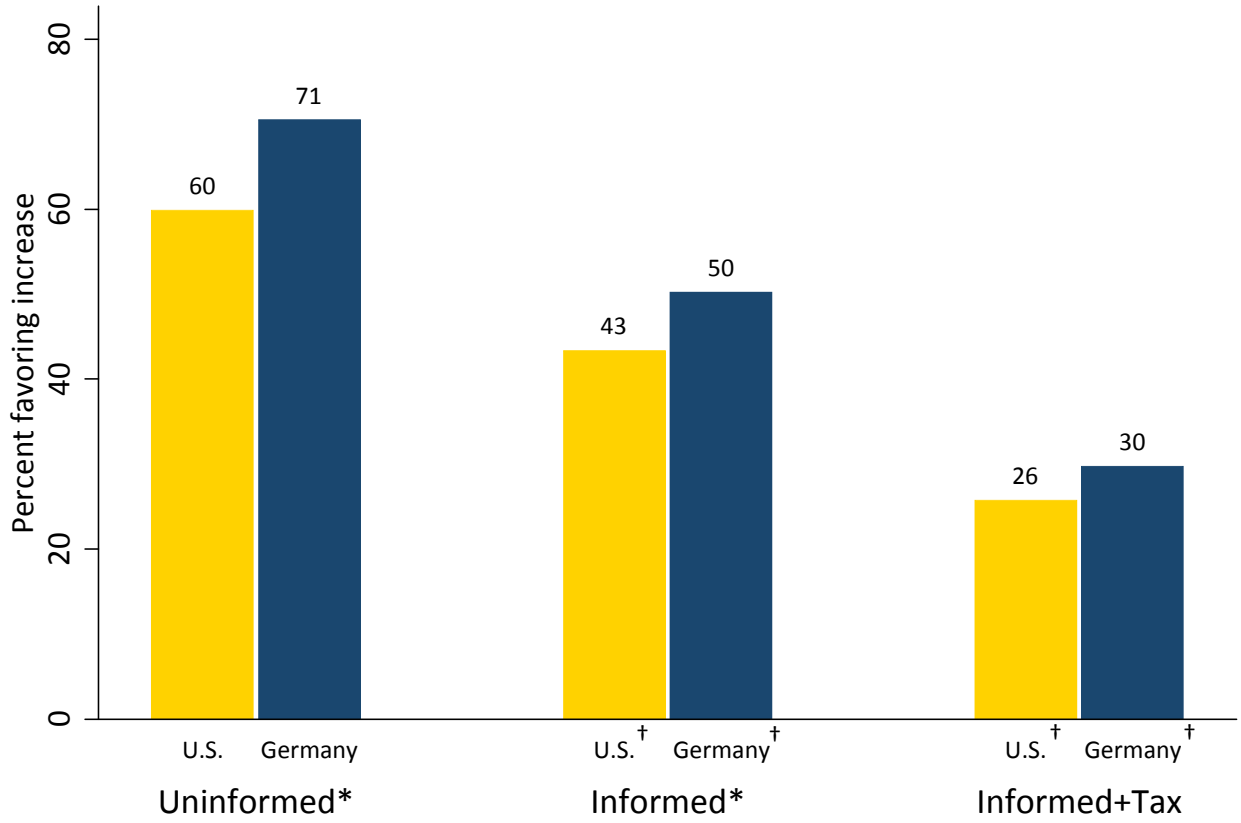
Notes: E_u = education spending (uninformed); E_i = education spending (informed); E_u^* = optimal education spending (uninformed); E_i^* = optimal education spending (informed); *O* = other spending.

Figure 2.2: The effect of tradeoff information on support for policy change



Notes: T = teachers per student (status); T_p = optimal teachers per student (perceived tradeoff); T_a = optimal teachers per student (actual tradeoff).

Figure 2.3: Support for higher spending: Information on spending and reference to taxes



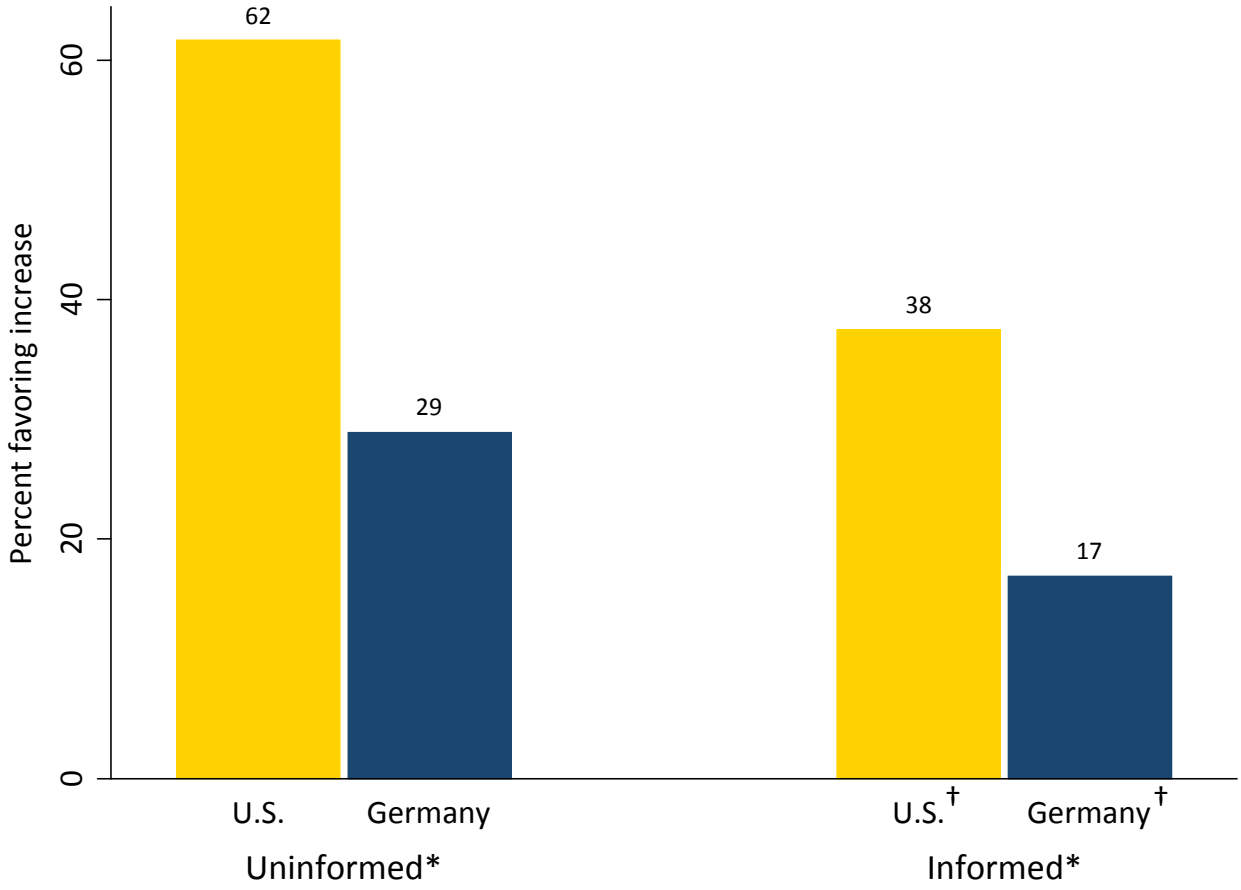
Notes: Share of respondents who favor government funding for public schools to either "greatly increase" or "increase"; other categories are "stay about the same," "decrease," and "greatly decrease". Three randomized experimental groups. Control group (Uninformed) did not receive further information. First treatment group (Informed) was informed about current spending levels. Second treatment group (Informed+tax) was additionally referred to tax financing requirements. See Appendix B for wording of the question in the two countries.

* Difference between the two countries is statistically significant at the 5 percent level.

† For the country, difference to the control group is statistically significant at the 5 percent level.

Source: The 2014 EdNext Survey and ifo Education Survey 2014.

Figure 2.4: Support for higher teacher salaries depending on current salary information



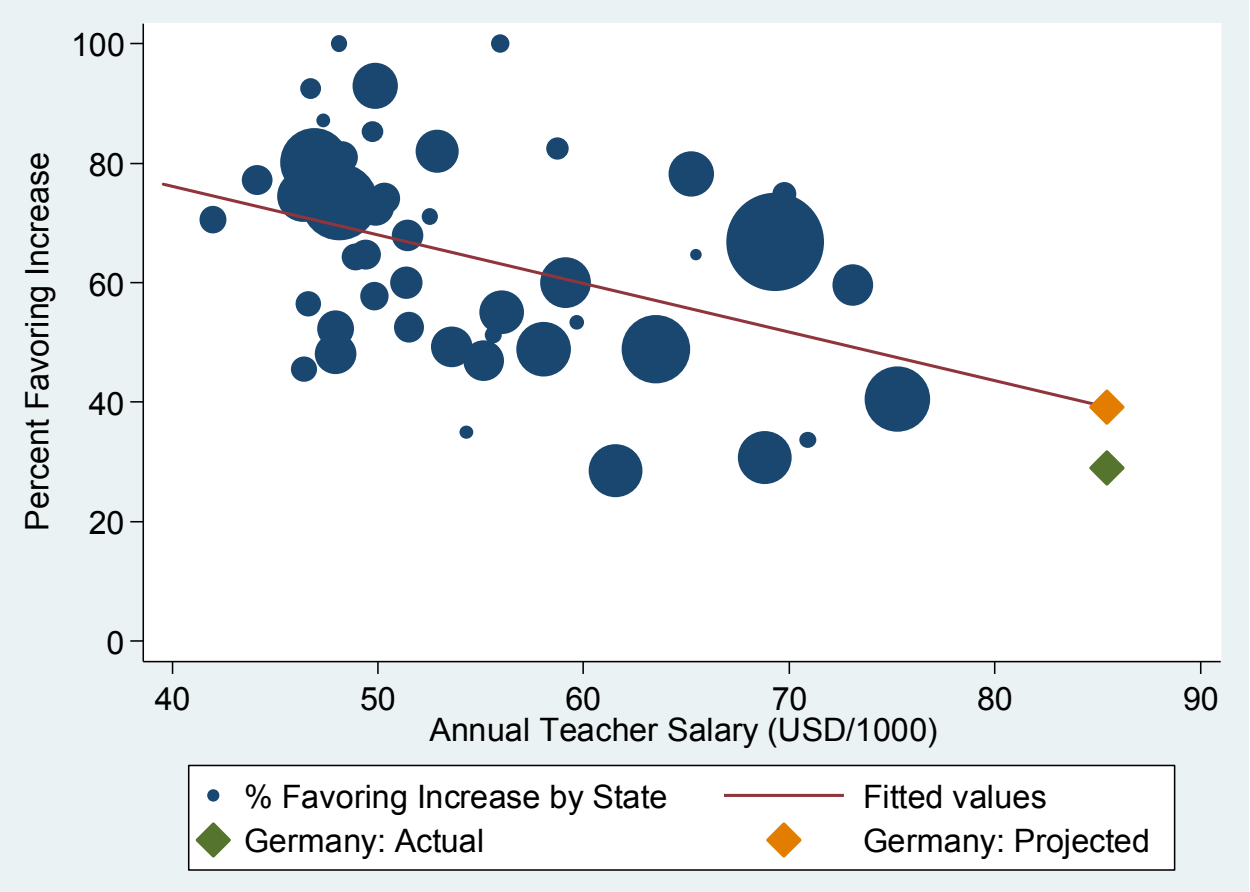
Notes: Share of respondents who favor teacher salaries to either "greatly increase" or "increase"; other categories are "stay about the same," "decrease," and "greatly decrease". Two randomized experimental groups. Control group (Uninformed) did not receive further information. Treatment group (Informed) was informed about current teacher salary levels. See Appendix B for wording of the question in the two countries.

* Difference between the two countries is statistically significant at the 5 percent level.

† For the country, difference to the control group is statistically significant at the 5 percent level.

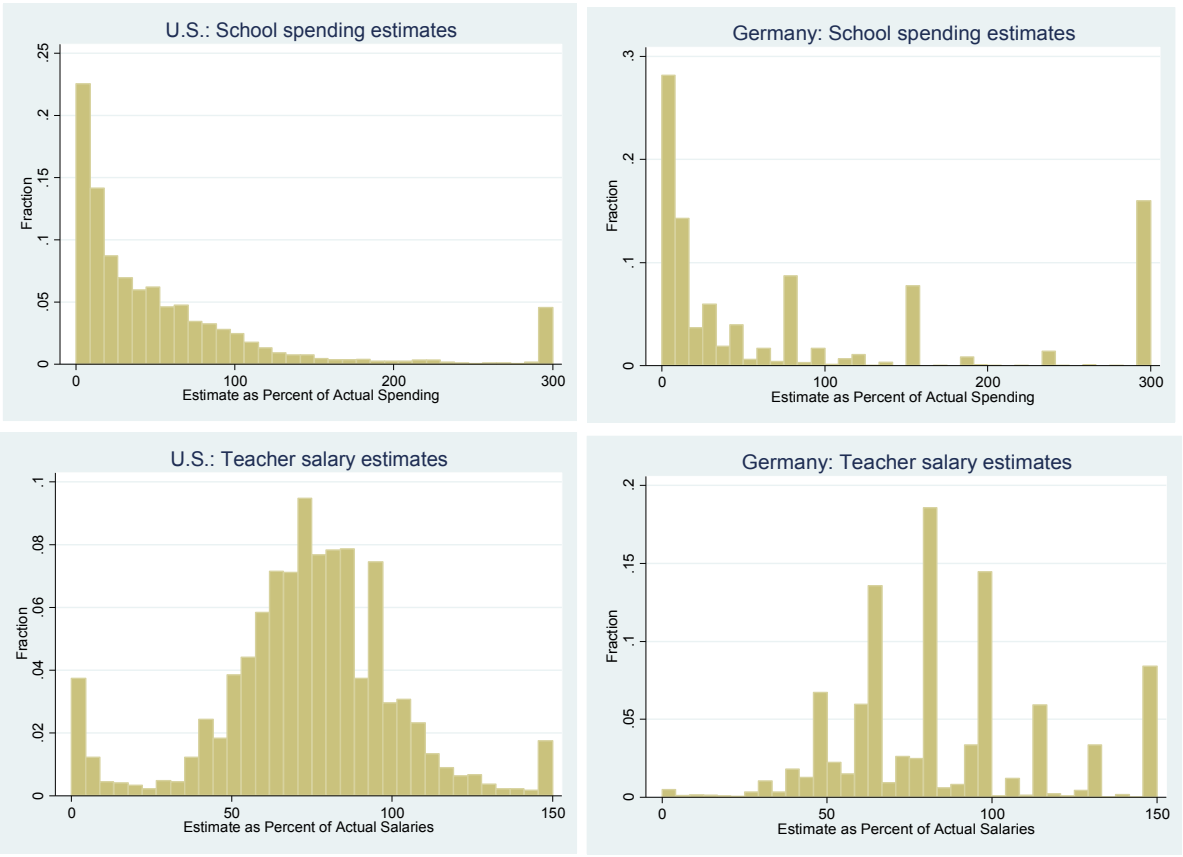
Source: The 2014 EdNext Survey and ifo Education Survey 2014.

Figure 2.5: Teacher salary levels and support for increases: Projecting from U.S. states to Germany



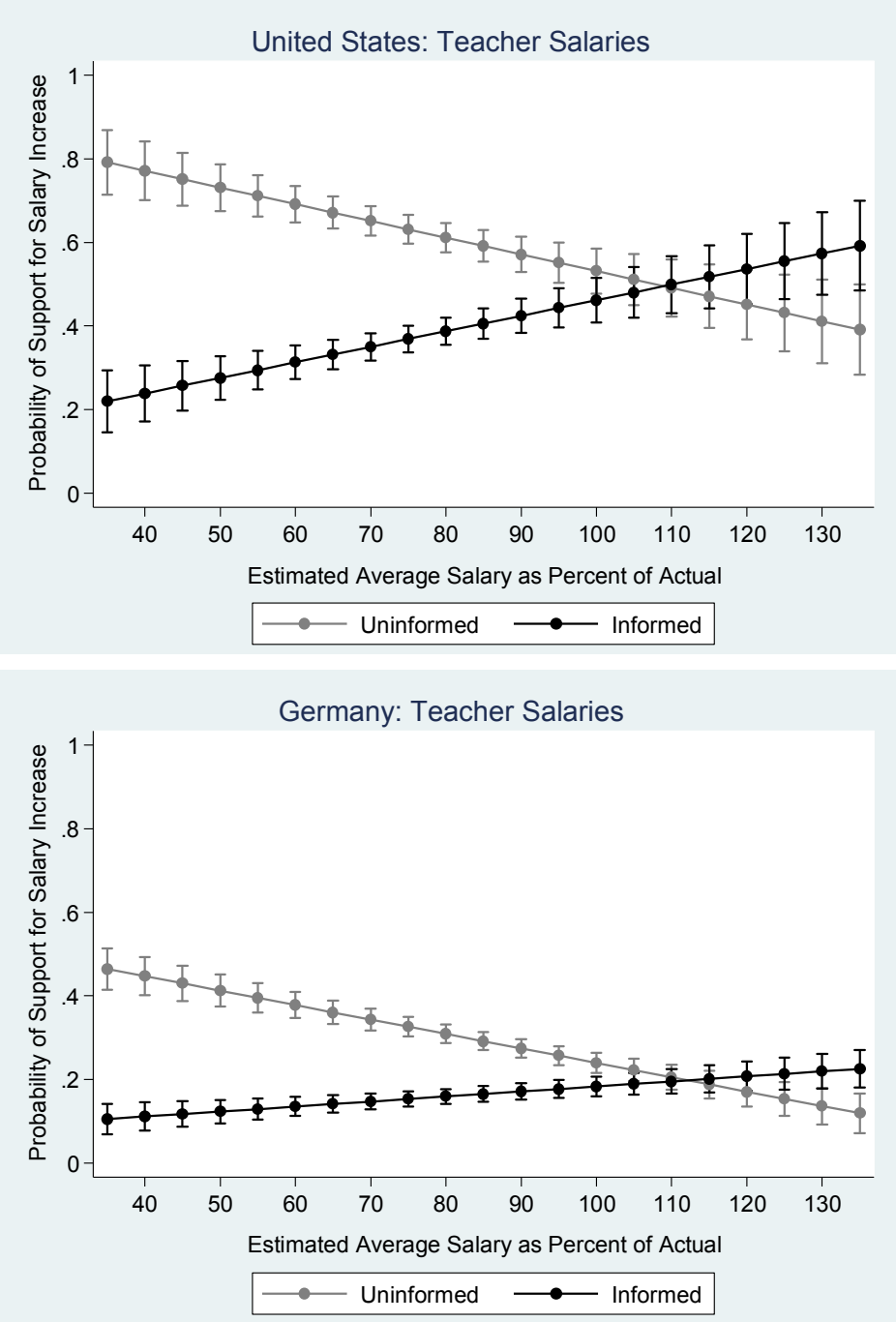
Notes: Scatterplot of mean support for increasing teacher salaries against average teacher salaries across U.S. states. State observations are weighted by the number of respondents. Three states with mean support for salary increases below 10 percent are omitted as outliers; results are substantively identical if they are included. Projection of German support is based on the (weighted) linear regression line between support and salary levels across U.S. states. Source: The 2014 EdNext Survey and ifo Education Survey 2014.

Figure 2.6: The public's estimates of spending and salary levels



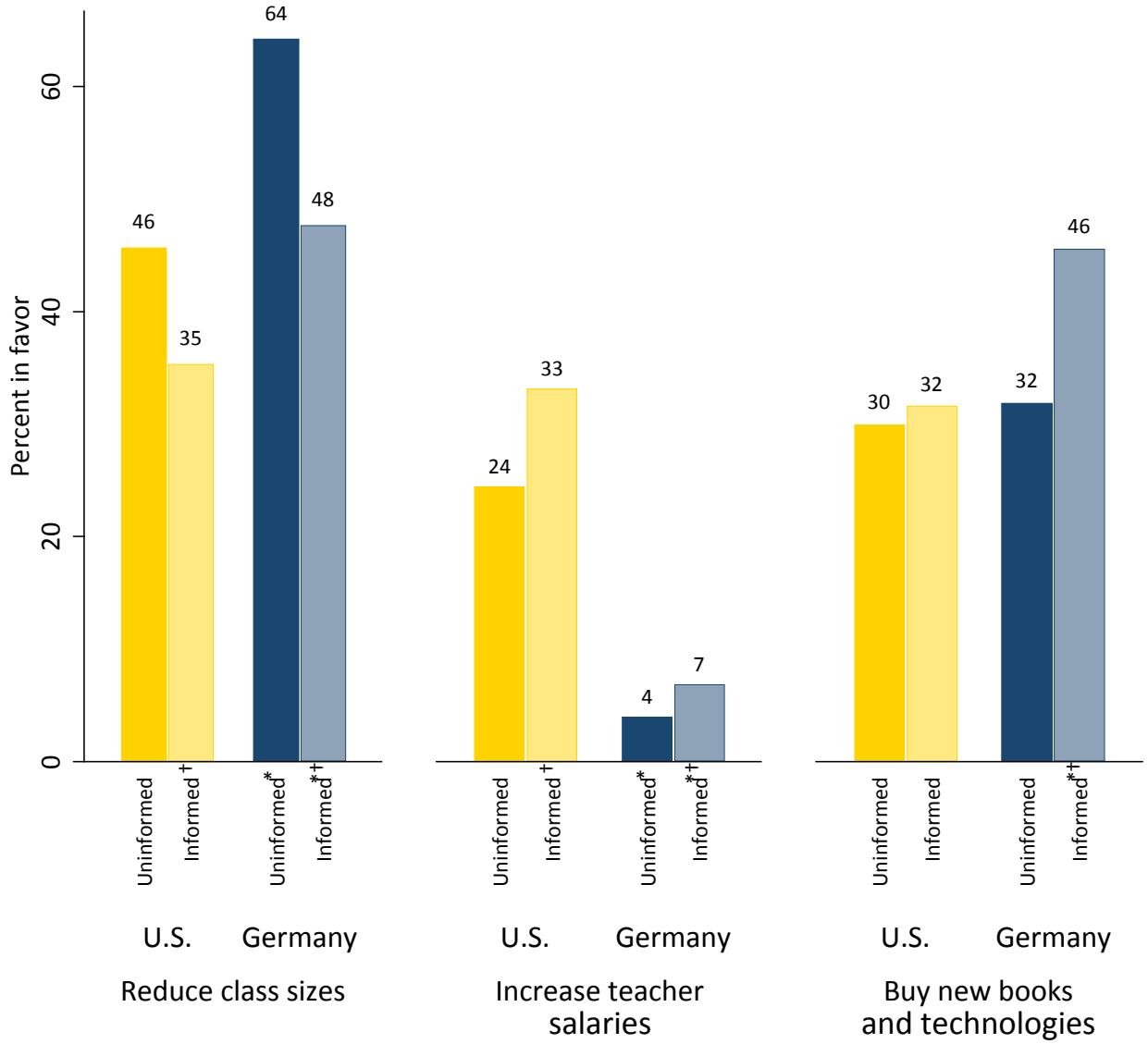
Notes: Histograms of respondents' best guesses of current levels of school spending and teacher salaries, respectively, relative to the respective actual levels. Spending (salary) estimates above 300 (150) percent of actual levels were first replaced by 300 (150). Source: The 2014 EdNext Survey and ifo Education Survey 2014.

Figure 2.7: Heterogeneity of information effects in teacher salary experiment with prior knowledge



Notes: Predicted support for increasing teacher salaries (and 95 percent confidence intervals) by experimental condition depending on respondents' prior estimates of current salaries (as a percentage of actual salaries) based on linear probability models reported in Table 2.6, columns 4 (U.S.) and 5 (Germany). Randomized experimental group: Informed: respondents informed about current teacher salary levels in their state (U.S.) or nation (Germany). Respondents with salary estimates greater than 300 percent or less than 33 percent of actual salaries are excluded from the estimation sample. Source: The 2014 EdNext Survey and ifo Education Survey 2014.

Figure 2.8: Support for spending on smaller classes, higher salaries, and teaching material



Notes: Share of respondents favoring the specific spending option over the other two options. Two randomized experimental groups. Control group (Uninformed) did not receive further information. Treatment group (Informed) was informed that reducing average class sizes by 3 students would cost roughly the same amount as increasing teacher salaries by 13 (15) percent or buying $10,000 (€20,000) in new books and technologies for each class every year in the U.S. (Germany). See Appendix B for wording of the question in the two countries.

* Difference to the same category in the U.S. is statistically significant at the 5 percent level.

† For the country, difference to the control group is statistically significant at the 5 percent level.

Source: The 2014 EdNext Survey and ifo Education Survey 2014.

Table 2.1: Descriptive statistics and balance across treatment conditions: School spending experiment

	U.S.			Germany		
	Uninformed	Informed	Informed+tax	Uninformed	Informed	Informed+tax
	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.502	0.551	0.545	0.522	0.504	0.516
Parent	0.297	0.318	0.288	0.185	0.175	0.187
Employed	0.577	0.581	0.576	0.473	0.496	0.505
Minority	0.336	0.339	0.365	0.056	0.083	0.069
Income (percentile)	54.25	53.98	53.12	53.24	53.50	52.88
Age						
18-34	0.276	0.309	0.304	0.237	0.241	0.255
35-49	0.217	0.228	0.225	0.241	0.269	0.266
50-64	0.335	0.270*	0.312	0.272	0.247	0.232**
65+	0.173	0.194	0.159	0.250	0.244	0.248
Education						
Less than high school	0.103	0.106	0.114	0.102	0.103	0.085
High school but no B.A.	0.612	0.581	0.601	0.745	0.767	0.744
B.A. degree or higher	0.285	0.312	0.285	0.153	0.129	0.171
Refused	0.008	0.013	0.015	0.012	0.011	0.015
Observations	897	869	903	1,010	1,092	1,066

Notes: Means. Significance tests indicate whether the mean for the informed or informed+tax treatment groups differs from the mean for the uninformed control group. Joint *F*-tests in regressions of each variable on both treatment dummies are statistically insignificant except for minority status for Germany (p=0.077); age 50-64 for the United States (p=0.037); and BA degree or higher for Germany (p=0.042). Observations weighted by survey weights to ensure national representativeness. Significance levels: ***p<0.01, **p<0.05, *p<0.10. Source: The 2014 EdNext Survey and ifo Education Survey 2014.

Table 2.2: Who supports school spending and teacher salary increases? Descriptive evidence

	School spending				Teacher salaries			
	U.S.		Germany		U.S.		Germany	
	(1)		(2)		(3)		(4)	
Female	0.070*	(0.040)	-0.026	(0.032)	0.016	(0.034)	-0.065***	(0.022)
Parent	0.073	(0.050)	0.019	(0.044)	0.041	(0.042)	-0.034	(0.030)
Employed	0.002	(0.048)	0.013	(0.037)	-0.033	(0.039)	-0.027	(0.026)
Minority	0.071	(0.043)	-0.098	(0.068)	0.137***	(0.036)	-0.029	(0.042)
Income (percentile)	-0.001*	(0.000)	0.001	(0.001)	0.000	(0.001)	-0.001	(0.000)
Age								
35-49	0.061	(0.063)	-0.010	(0.050)	-0.016	(0.050)	-0.019	(0.034)
50-64	0.015	(0.057)	0.018	(0.044)	-0.023	(0.049)	-0.022	(0.032)
65+	0.023	(0.069)	0.004	(0.052)	-0.001	(0.057)	-0.072*	(0.037)
Education								
High school but no B.A.	-0.054	(0.081)	0.095	(0.060)	0.018	(0.069)	-0.050	(0.039)
B.A. degree or higher	0.007	(0.088)	0.226***	(0.068)	0.148**	(0.074)	0.043	(0.049)
Constant	0.604***	(0.098)	0.551	(0.077)	0.053	(0.088)	0.436***	(0.051)
Observations	894		997		1,306		2,062	
R^2	0.042		0.034		0.047		0.020	

Notes: Linear probability models. Dependent variable: binary outcome of support for increased spending. Sample: control group. Regressions weighted by survey weights to ensure national representativeness. Standard errors in parentheses. Significance levels: ***p<0.01, **p<0.05, *p<0.10. Source: The 2014 EdNext Survey and ifo Education Survey 2014.

Table 2.3: Effects of information treatment on support for school spending

Support for school spending to:	Increase					Decrease
	U.S.	Germany	Pooled			Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
Informed	-0.165***	-0.203***	-0.165***	-0.167***	-0.169***	0.028*
	(0.029)	(0.023)	(0.029)	(0.029)	(0.029)	(0.016)
Informed+tax	-0.341***	-0.408***	-0.341***	-0.341***	-0.344***	0.111***
	(0.027)	(0.022)	(0.027)	(0.027)	(0.027)	(0.019)
Germany			0.107***	0.122***	Included	Included
			(0.026)	(0.026)		
Germany X Informed			-0.038	-0.034	-0.031	0.011
			(0.037)	(0.037)	(0.037)	(0.019)
Germany X Informed+tax			-0.067*	-0.068**	-0.065*	-0.010
			(0.035)	(0.035)	(0.035)	(0.023)
Controls	No	No	No	Yes	Yes	Yes
Controls interacted with country	No	No	No	No	Yes	Yes
Control mean	0.599	0.705	-	-	-	-
Information effect/Control mean	-0.276	-0.288	-	-	-	-
Information+tax effect/Control mean	-0.570	-0.578	-	-	-	-
Observations	2,646	3,128	5,774	5,774	5,774	5,774
R^2	0.081	0.110	0.100	0.106	0.116	0.116

Notes: Linear probability models. Dependent variable: binary outcome of support for increased (column 6: decreased) spending. Randomized experimental groups: Informed = respondents informed about current school spending levels. Informed+tax = respondents additionally referred to tax financing requirements. Controls include gender, parental and employment status, income, age (four categories), and education (three categories). Regressions weighted by survey weights to ensure national representativeness. Standard errors in parentheses. Significance levels: ***p<0.01, **p<0.05, *p<0.10. Source: The 2014 EdNext Survey and ifo Education Survey 2014.

Table 2.4: Effects of information treatment on support for teacher salary

Support for teacher salaries to:	Increase					Decrease
	U.S.	Germany	Pooled			Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
Informed	-0.242***	-0.120***	-0.242***	-0.246***	-0.247***	0.006
	(0.023)	(0.014)	(0.023)	(0.023)	(0.023)	(0.013)
Germany			-0.327***	-0.314***	Included	Included
			(0.020)	(0.021)		
Germany X Informed			0.122***	0.123***	0.122***	0.076***
			(0.027)	(0.027)	(0.027)	(0.017)
Controls	No	No	No	Yes	Yes	Yes
Controls interacted with country	No	No	No	No	Yes	Yes
Control mean	0.617	0.290	-	-	-	-
Information effect/Control mean	-0.392	-0.415	-	-	-	-
Observations	2,639	4,127	6,766	6,766	6,766	6,766
R^2	0.059	0.020	0.115	0.126	0.128	0.031

Notes: Linear probability models. Dependent variable: binary outcome of support for increased (column 6: decreased) teacher salaries. Randomized experimental group: Informed = respondents informed about current teacher salary levels. Controls include gender, parental and employment status, income, age (four categories), and education (three categories). Regressions weighted by survey weights to ensure national representativeness. Standard errors in parentheses. Significance levels: ***p<0.01, **p<0.05, *p<0.10. Source: The 2014 EdNext Survey and ifo Education Survey 2014.

Table 2.5: Heterogeneous treatment effects by demographic subgroups

	School spending experiment								Teacher salary experiment			
	Informed				Informed+tax				Informed			
	U.S.		Germany		U.S.		Germany		U.S.		Germany	
	No/Low	Yes/High	No/Low	Yes/High	No/Low	Yes/High	No/Low	Yes/High	No/Low	Yes/High	No/Low	Yes/High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Female	-0.156***	-0.179***	-0.191***	-0.217***	-0.302***	-0.380***	-0.416***	-0.401***	-0.230***	-0.253***	-0.110***	-0.134***
	(0.041)	(0.040)	(0.033)	(0.031)	(0.039)	(0.040)	(0.033)	(0.030)	(0.034)	(0.032)	(0.022)	(0.018)
Parent	-0.151***	-0.204***	-0.212***	-0.161***	-0.325***	-0.378***	-0.390***	-0.494***	-0.208***	-0.322***	-0.129***	-0.083***
	(0.034)	(0.054)	(0.026)	(0.052)	(0.032)	(0.050)	(0.025)	(0.048)	(0.028)	(0.042)	(0.016)	(0.031)
Employed	-0.177***	-0.157***	-0.206***	-0.202***	-0.360***	-0.338***	-0.377***	-0.441***	-0.260***	-0.229***	-0.123***	-0.117***
	(0.044)	(0.038)	(0.033)	(0.032)	(0.042)	(0.036)	(0.032)	(0.031)	(0.036)	(0.031)	(0.020)	(0.020)
Minority	-0.189***	-0.117***	-0.199***	-0.218**	-0.348***	-0.336***	-0.419***	-0.250***	-0.224***	-0.273***	-0.123***	-0.080
	(0.035)	(0.050)	(0.024)	(0.085)	(0.033)	(0.047)	(0.023)	(0.092)	(0.029)	(0.039)	(0.015)	(0.054)
Income	-0.160***	-0.169***	-0.169***	-0.229***	-0.367***	-0.317***	-0.371***	-0.436***	-0.280***	-0.203***	-0.124***	-0.117***
	(0.040)	(0.041)	(0.035)	(0.030)	(0.037)	(0.039)	(0.035)	(0.029)	(0.032)	(0.034)	(0.022)	(0.019)
Age	-0.200***	-0.134***	-0.186***	-0.222***	-0.349***	-0.337***	-0.420***	-0.394***	-0.279***	-0.206***	-0.125***	-0.115***
	(0.046)	(0.036)	(0.034)	(0.031)	(0.044)	(0.032)	(0.033)	(0.030)	(0.037)	(0.029)	(0.021)	(0.019)
Education	-0.154***	-0.191***	-0.186***	-0.282***	-0.354***	-0.308***	-0.406***	-0.436***	-0.246***	-0.245***	-0.128***	-0.090**
	(0.036)	(0.046)	(0.025)	(0.057)	(0.033)	(0.045)	(0.024)	(0.054)	(0.029)	(0.037)	(0.015)	(0.042)

Notes: Subgroup estimations by characteristic indicated in first column. Binary variables (female, parent, employed, minority): No = subgroup where the status is wrong; Yes = subgroup where the status is true. Income, age: Low = subgroup where variable is below sample mean; High = subgroup where variable is above sample mean. Education: Low = below B.A. degree; High = B.A. degree or higher. Linear probability models. Dependent variable: binary outcome of support for increased spending (columns 1-8) resp. increased teacher salaries (columns 9-12). Randomized experimental groups: Informed = respondents informed about current levels of school spending (columns 1-4) resp. teacher salaries (columns 9-12). Informed+tax = respondents additionally referred to tax financing requirements. Treatment effects do not differ significantly between the respective subgroups, with the exceptions that the Informed+tax treatment effect on support for spending increases differs significantly by parental (p=0.050) and minority (p=0.074) status in Germany and that the Informed treatment effect on support for salary increases differs significantly by parental status (p=0.023) and income level (p=0.098) in the United States. Regressions weighted by survey weights to ensure national representativeness. Standard errors in parentheses. Significance levels: ***p<0.01, **p<0.05, *p<0.10. Source: The 2014 EdNext Survey and ifo Education Survey 2014.

Table 2.6: Heterogeneity of information effects with respect to prior knowledge

	Spending			Salaries		
	U.S.	Germany	Pooled	U.S.	Germany	Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
Informed	-0.189***	-0.239***	-0.185***	-0.846***	-0.527***	-0.848***
	(0.077)	(0.051)	(0.031)	(0.094)	(0.050)	(0.094)
Estimate (Spending: >Actual;	-0.098*	-0.023	-0.106*	-0.401***	-0.346***	-0.401***
Salaries: % of Actual)	(0.058)	(0.042)	(0.059)	(0.088)	(0.044)	(0.087)
Informed X Estimate	0.175**	0.153***	0.176**	0.777***	0.471***	0.780***
	(0.083)	(0.057)	(0.085)	(0.121)	(0.057)	(0.121)
Germany			0.125***			-0.328***
			(0.029)			(0.078)
Germany X Informed			-0.057			0.316***
			(0.041)			(0.106)
Germany X Estimate			0.085			0.052
			(0.072)			(0.096)
Germany X Informed X Estimate			-0.014			-0.301**
			(0.103)			(0.133)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,722	1,904	3,626	2,426	3,833	6,259
R^2	0.052	0.070	0.055	0.103	0.068	0.154

Notes: Linear probability models. Dependent variable: binary outcome of support for increased spending (columns 1-3) and teacher salaries (columns 4-6). Estimate: binary variable indicating estimate of spending level is larger than actual spending level (columns 1-3) or continuous variable measuring salary estimate as a percentage of actual salaries (columns 4-6). Randomized experimental group: Informed = respondents informed about current spending (columns 1-3) or teacher salary (columns 4-6) levels. Controls include gender, parental and employment status, income, age (four categories), and education (three categories). Observations with teacher salary estimates greater than 300 percent and less than 33 percent of actual salaries excluded. Regressions weighted by survey weights to ensure national representativeness. Standard errors in parentheses. Significance levels: ***p<0.01, **p<0.05, *p<0.10. Source: The 2014 EdNext Survey and ifo Education Survey 2014.

Table 2.7: Effect of information treatment on preferences for spending options

	Class size vs. teacher salary			Class size vs. books/technologies			Teacher salary vs. books/technologies		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Informed	0.570***	0.559***	0.562***	0.734***	0.722***	0.722***	1.287***	1.292***	1.285***
	(0.048)	(0.048)	(0.048)	(0.060)	(0.060)	(0.060)	(0.115)	(0.117)	(0.116)
Germany	8.713***	8.918***	Included	1.323***	1.462***	Included	0.152***	0.164***	Included
	(1.603)	(1.654)		(0.126)	(0.142)		(0.029)	(0.032)	
Germany X Informed	0.754	0.755	0.718	0.706***	0.704***	0.704***	0.936	0.933	0.981
	(0.183)	(0.183)	(0.178)	(0.093)	(0.094)	(0.094)	(0.233)	(0.232)	(0.248)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls interacted with country	No	No	Yes	No	No	Yes	No	No	Yes
Observations	7,230	7,230	7,230	7,230	7,230	7,230	7,230	7,230	7,230
Information effect	0.430***	0.422***	0.403***	0.518***	0.508***	0.508***	1.206	1.205	1.260
for Germany	(0.098)	(0.096)	(0.094)	(0.053)	(0.053)	(0.053)	(0.280)	(0.279)	(0.298)

Notes: Relative risk ratios of multinomial logit models. Randomized experimental group: Informed = respondents informed about financial tradeoffs involved between options. Controls include gender, parental and employment status, income, age (four categories), and education (three categories). Regressions weighted by survey weights to ensure national representativeness. Standard errors in parentheses. Significance levels (of difference from 1): ***p<0.01, **p<0.05, *p<0.10. Source: The 2014 EdNext Survey and ifo Education Survey 2014.

Appendix

A Simple Model of the Effect of Information Provision on Preferences for Increased Education Spending

Consider a representative individual deciding between spending on education E and spending on other items O in order to maximize the following expression of her preferences subject to a budget constraint:

$$\max_{E,O} E^{\alpha} O^{1-\alpha} \qquad s.t.\ pE + O = B \tag{1}$$

where the parameter $\alpha \in (0,1)$ reflects the relative importance of education in preferences, p is the relative price of education, and B is the budget. Optimal spending levels on the two categories can be derived as

$$E^{*} = \alpha \frac{B}{p}, \qquad O^{*} = (1-\alpha)B \tag{2}$$

Subscripts u and i refer to uninformed and informed, respectively. Let us assume that in the uninformed state, the individual underestimates spending on education ($E_u < E_i$), whereas providing information on education spending does not affect the individual's estimates of spending on other items ($O_u = O_i = O$) or of the price of education (p). Optimal spending on education, E_u^{*} and E_i^{*}, is given by (2) for the respective budget, $B_u = pE_u + O$ and $B_i = pE_i + O$. Then, the difference in desired increases in spending on education between the informed and the uninformed status is given by:

$$\begin{aligned} \Delta_i - \Delta_u &= \left(E_i^{*} - E_i\right) - \left(E_u^{*} - E_u\right) \\ &= \left(\alpha \frac{pE_i + O}{p} - E_i\right) - \left(\alpha \frac{pE_u + O}{p} - E_u\right) \\ &= (\alpha - 1)\left(E_i - E_u\right) < 0 \end{aligned} \tag{3}$$

That is, the informed individual is closer to her optimal spending. Her support for increased spending is reduced by being informed about the actual spending, as depicted in Figure 2.1.

If the provision of information on education spending also affects the individual's estimates of spending on other items ($O_u \neq O_i$), $\Delta_i - \Delta_u$ is given by the following expression:

$$\Delta_i - \Delta_u = (\alpha - 1)(E_i - E_u) + \frac{\alpha}{p}(O_i - O_u) \tag{4}$$

and $\Delta_i - \Delta_u < 0$ holds as long as:

$$E_i - E_u > \frac{\alpha}{(1-\alpha)p}(O_i - O_u) \tag{5}$$

That is, all else equal, the likelihood that informing the individual about the true level of education spending will reduce her support for higher education spending increases with the relative extent to which the information affects spending estimates in education compared to other items, increases with the relative price of education, and decreases with the importance of education in her preferences.

B Wording of the Survey Questions

Estimate of spending level:

U.S.: Based on your best guess, what is the average amount of money spent each year for a child in public schools in your local school district?

Germany: Was schätzen Sie, wie viel wird durchschnittlich jedes Jahr pro Schülerin/Schüler an öffentlichen allgemeinbildenden Schulen in Deutschland ausgegeben?

Estimate of teacher salary level:

U.S.: Based on your best guess, what is the average yearly salary of a public school teacher in your state?

Germany: Was schätzen Sie, wie viel verdienen Lehrerinnen und Lehrer im Durchschnitt in Deutschland? Bitte schätzen Sie das Netto-Monatsgehalt einer Vollzeitstelle.

Support for higher education spending:

U.S.: *Uninformed [Informed]*: [As it turns out, according to the most recent information available $value is being spent each year per child attending public schools in your district.] Do you think that government funding for public schools in your district should increase, decrease, or stay about the same? *Informed+tax*: As it turns out, according to the most recent information available $value is being spent each year per child attending public schools in your district. Do you think that taxes to fund public schools in your district should increase, decrease, or stay about the same?

Germany: *Uninformed [Informed]*: [Die staatlichen Bildungsausgaben in Deutschland betragen im Durchschnitt jährlich 6400 Euro pro Schülerin/Schüler.] Sollten die staatlichen Ausgaben für Schulen in Deutschland Ihrer Meinung nach steigen, sinken oder unverändert bleiben? *Informed+tax*: Die staatlichen Bildungsausgaben in Deutschland betragen im Durchschnitt jährlich 6400 Euro pro Schülerin/Schüler. Sollten Steuern für die staatliche Finanzierung von Schulen in Deutschland Ihrer Meinung nach steigen, sinken oder unverändert bleiben?

Support for higher teacher salaries:

U.S.: *Uninformed [Informed]*: [As it turns out, public school teachers in your state are paid an average annual salary of $value.] Do you think that public school teacher salaries should increase, decrease, or stay about the same?

Germany: *Uninformed [Informed]*: [In Deutschland verdienen vollzeitbeschäftigte Lehrerinnen und Lehrer im Durchschnitt rund 3000 Euro netto im Monat.] Was meinen Sie, sollten die Gehälter von Lehrerinnen und Lehrern in Deutschland steigen, sinken oder unverändert bleiben?

Support for different spending categories:

U.S.: *Uninformed*: Suppose the government plans to increase spending in the school system. Which one of the following options do you favor? Reducing class sizes – Increasing teacher salaries – Purchasing new books and technologies. *Informed*: Suppose the government plans to increase spending in the school system. Reducing average class sizes by 3 students would cost roughly the same amount as increasing teacher salaries by 13 percent or buying $10,000 in new books and technologies for each class every year. Among these options, which do you favor? Reducing class sizes by 3 students – Increasing teacher salaries by 13 percent – Purchasing $10,000 in new books and technologies for each class every year.

Germany: *Uninformed*: Stellen Sie sich vor, die Regierung plant, die Ausgaben für das Schulsystem zu erhöhen. Für welche der folgenden Möglichkeiten sind Sie? Schulklassen verkleinern – Lehrergehälter erhöhen – Neue Schulbücher, Computer und andere Lehrmittel anschaffen. *Informed*: Stellen Sie sich vor, die Regierung plant, die Ausgaben für das Schulsystem zu erhöhen. Die Schulklassen um drei Schülerinnen/Schüler zu verkleinern, würde in etwa so viel kosten, wie die Lehrergehälter um 15 Prozent zu erhöhen oder neue Lehrmittel im Wert von jährlich 20000 Euro für jede Klasse anzuschaffen. Für welche dieser Möglichkeiten sind Sie? Schulklassen um 3 Schüler verkleinern – Lehrergehälter um 15

Prozent erhöhen – Neue Schulbücher, Computer und andere Lehrmittel im Wert von jährlich 20.000 Euro für jede Klasse anschaffen.

C Additional Experiments in the German Survey

Section 2.7 reports results on three additional experiments implemented in the German survey. As in the main survey experiments, these robustness experiments provided information to a randomly selected treatment group before eliciting preferences in the same way as in the uninformed control group. Again, randomization was independent across experiments.

Two robustness experiments analyze whether public support for specific reform proposals depends on information about the fiscal costs associated with their implementation. We chose two proposals that are currently under public debate in Germany: introducing a whole-day school system and abolishing grade retention. The former proposal implies the introduction of a new policy, whereas the latter implies the abolishment of a current practice. Respondents were randomly and independently assigned to a control group and a treatment group in both questions. The question on whole-day schooling is worded as follows: “Do you favor or oppose that Germany in general switches to a whole-day school system where all children are in school until 3 pm?” The question on grade retention is worded as follows: “Do you favor or oppose that low-performing students have to repeat the grade?” In both cases, respondents were asked to express their preferences on a five-point scale ranging from “strongly favor” to “strongly oppose”. The treatment groups were informed that introducing whole-day schooling would cost more than €9 billion per year and that grade retention costs almost €1 billion each year, respectively (cost estimates taken from Klemm, 2009, 2012).

The third robustness experiment extends the analysis to different areas of public spending. We follow a basic attitudinal approach (Ferris, 1983) by presenting respondents a list of government services and asking them if they favor spending more, the same, or less on each of these areas. We focus on the major areas of public expenditure: besides education, these include social security, public safety, defense, and culture.[28] The question is worded as follows: “In your opinion, how much should the government spend in the future in the following areas compared to today? Remember that increased public spending might have to be financed through an increase in taxes.” Members of the control group were

[28] To improve understanding of the formal terminology, we provided examples for selected areas of public spending, namely contributions to the public pension system and unemployment benefits for long-term unemployed as examples of spending on social security and the police as an example of spending on public safety.

asked to state their preferences for future public spending on a five-point scale ranging from “much less” to “much more” in each of the areas without any further information. All spending areas were presented simultaneously on one screen. The ordering of areas was randomized to balance potential order effects in answering behavior. Members of the treatment group were informed about the current levels of public spending per year in each area when answering the same question as the control group. Respondents were informed of the following annual spending levels (Statistisches Bundesamt, 2014b): €227 billion on social security, €95 billion on education, €38 billion on public safety, €27 billion on defense, and €10 billion on culture. The information was provided simultaneously for all spending areas.

D Appendix Tables

Table A2.1: Descriptive statistics and balance across treatment conditions: Teacher salary and spending options experiments

	Teacher salary experiment				Spending options experiment			
	U.S.		Germany		U.S.		Germany	
	Uninformed	Informed	Uninformed	Informed	Uninformed	Informed	Uninformed	Informed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	0.527	0.538	0.523	0.502	0.515	0.550	0.508	0.493
Parent	0.323	0.279**	0.185	0.186	0.289	0.312	0.181	0.187
Employed	0.585	0.572	0.483	0.499	0.588	0.568	0.498	0.482
Minority	0.350	0.344	0.077	0.074	0.340	0.354	0.077	0.071
Income (percentile)	52.86	54.65	53.81	52.77	53.32	54.24	53.64	54.28
Age								
18-34	0.287	0.304	0.235	0.248	0.295	0.296	0.233	0.241
35-49	0.246	0.202**	0.263	0.257	0.223	0.223	0.268	0.251
50-64	0.305	0.307	0.265	0.238**	0.312	0.300	0.263	0.253
65+	0.162	0.187	0.237	0.257	0.170	0.180	0.236	0.255
Education								
Less than high school	0.090	0.125**	0.098	0.09	0.109	0.107	0.090	0.110
High school but no B.A.	0.631	0.566***	0.765	0.754	0.601	0.596	0.771	0.753
B.A. degree or higher	0.277	0.310	0.137	0.156	0.290	0.298	0.140	0.147
Refused	0.012	0.016	0.011	0.010	0.013	0.018	0.009	0.013
Observations	1,322	1,347	2,083	2,088	1,331	1,338	986	1,068

Notes: Means. Significance tests indicate whether the mean for the informed treatment group differs from the mean for the uninformed control group. Observations weighted by survey weights to ensure national representativeness. Significance levels: ***p<0.01, **p<0.05, *p<0.10. Source: The 2014 EdNext Survey and ifo Education Survey 2014.

Table A2.2: Heterogeneity of information effect with respect to certainty of prior guess in the follow-up survey

	U.S.				Germany			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Informed	-0.159***	-0.185***	-0.181***	-0.196***	-0.149***	-0.197***	-0.163***	-0.212***
	(0.022)	(0.024)	(0.027)	(0.029)	(0.0282)	(0.033)	(0.034)	(0.039)
Estimate (Spending: >Actual)		-0.123***		-0.023		0.042		0.018
		(0.045)		(0.064)		(0.045)		(0.056)
Informed X Estimate		0.148**		0.121		0.180***		0.217***
		(0.064)		(0.085)		(0.065)		(0.076)
Certain			-0.107***	-0.058			-0.006	-0.0260
			(0.034)	(0.038)			(0.044)	(0.052)
Certain X Informed			0.066	0.042			0.023	0.053
			(0.047)	(0.052)			(0.064)	(0.074)
Certain X Estimate				-0.155*				0.076
				(0.092)				(0.095)
Certain X Informed X Estimate				-0.059				-0.124
				(0.132)				(0.148)
Control mean	0.577				0.740			
Observations	2,010	1,986	1,986	1,972	2,078	1,984	1,984	1,984
R^2	0.025	0.031	0.31	0.038	0.025	0.048	0.028	0.049
F (Certain X Informed, Certain X Informed X Estimate): *p* value				0.715				0.665

Notes: Linear probability models. Dependent variable: binary outcome of support for increased school spending. Estimate: binary variable indicating estimate of spending level is larger than actual spending level. Certain: binary variable indicating respondent was reasonably sure about his/her estimate of the spending level, as indicated by the top four points on a seven-point scale ranging from very unsure to very sure. Randomized experimental group: Informed = respondents informed about current spending level. Regressions weighted by survey weights to ensure national representativeness. Standard errors in parentheses. Significance levels: ***p<0.01, **p<0.05, *p<0.10. Source: The 2015 EdNext Survey and ifo Education Survey 2015.

Table A2.3: Effects of cost information on support for specific policy reform proposals in Germany

	Whole-day schooling		Grade retention	
	(1)	(2)	(3)	(4)
Informed	-0.054**	-0.051**	-0.038*	-0.029
	(0.024)	(0.023)	(0.021)	(0.021)
Controls	No	Yes	No	Yes
Control mean	0.605		0.775	
Estimated fiscal costs	€ 9 billion		€ 1 billion	
Observations	2,042	1,940	2,071	1,965
R^2	0.003	0.071	0.002	0.028

Notes: Linear probability models. Dependent variable: binary outcomes of support for policy/proposal. Informed = respondents informed about estimated fiscal cost of policy/proposal. Controls include age, gender, born in Germany, living with partner, education, employment status, working in education sector, parent status, household income, West Germany, living in large city, parental education level, patience, altruism, and political attitude. Standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Source: ifo Education Survey 2014.

Table A2.4: Effects of information treatment on support for public spending in different areas in Germany

	Support for public spending to increase									
	Social security		Education		Public safety		Defense		Culture	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Informed	-0.051**	-0.048**	-0.137***	-0.147***	-0.061***	-0.057***	-0.029***	-0.032***	-0.041**	-0.042***
	(0.021)	(0.021)	(0.019)	(0.019)	(0.021)	(0.020)	(0.011)	(0.011)	(0.016)	(0.016)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Control mean	0.516		0.721		0.498		0.091		0.216	
Relative effect	-0.098	-0.093	-0.191	-0.204	-0.122	-0.114	-0.322	-0.352	-0.192	-0.194
Spending level	€ 227 billion		€ 95 billion		€ 38 billion		€ 27 billion		€ 10 billion	
Observations	2,773	2,641	2,770	2,639	2,772	2,639	2,773	2,640	2,772	2,640
R^2	0.003	0.052	0.021	0.085	0.004	0.073	0.003	0.029	0.003	0.077

Notes: Linear probability models. Dependent variable: binary outcome of support for increased spending in area indicated in column header. Randomized experimental group: Informed = respondents informed about current annual spending levels for each area. Controls include age, gender, born in Germany, living with partner, education, employment status, working in education sector, parent status, household income, West Germany, living in large city, parental education level, patience, altruism, and political attitude. Relative effect: Information effect/Control mean. Spending level: current annual spending in respective area Regressions weighted by survey weights to ensure representativeness. Standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Source: ifo Education Survey 2014

3 Obstacles to Efficient Allocations of Public Education Spending[29]

Education systems are inherently hierarchical: Successful participation in any education stage depends on the dynamic of skill formation at earlier education stages. Economic theory shows that complementarities of skill acquisition over the life-cycle increase the returns to investments in earlier education areas (see, e.g., Cunha et al., 2006). At the same time, empirical studies suggest that spending on early education is generally more beneficial for fostering education and other life outcomes than spending on later education areas (Heckman, 2008). This implies that rates of return are not equalized across the different education stages at the current allocation of spending, favoring spending increases for early education areas. In light of this debate, the allocation of public spending in Germany has come under particular scrutiny—in particular since the share of public spending on early education in Germany is far lower than the share of public spending in tertiary education (OECD, 2016b).

This chapter investigates whether spending preferences of the population allow for inefficiencies in the allocation of education spending across different education levels. The possibility of the non-optimal allocation of government resources has commanded substantial public and scientific interest. The existence of such inefficiencies is well documented for a variety of contexts (Caplan, 2007; Romer, 2003). Inefficiencies in democratic processes are possible if special interest groups influence elections in their personal best interest (Su, 2006; Karabarbounis, 2011; Grossman and Helpman, 2001; Gradstein et al., 2005) or if voters hold misconceptions about the likely effects of policies (Gilens, 2001; Romer, 2003), as the theoretical literature on the political economy of resource allocation shows. Starting from these observations, I conduct a survey experiment to provide a better understanding of the political economy of education spending allocation.

[29] For helpful comments, I thank Martin West, Lori Taylor, Magne Mogstad, Noam Yuchtman, Ludger Woessmann, Philipp Lergetporer, and participants at the EALE 2017 in St. Gallen, the ESPE Conference 2017 in Glasgow, the EEA-ESEM 2017 in Lisbon, the AEFP Meeting 2017 in Washington, DC, the Harvard Education Policy Reading Group, the EBE Summer Meeting 2016, the Seminar at the ifo Center for the Economics of Education in Munich, and the ifo Christmas Conference. I am also most grateful to Ludger Woessmann, Philipp Lergetporer, Franziska Kugler, and Laura Oestreich for their help in preparing the survey. Financial support by Deutsche Forschungsgemeinschaft through SFB-TRR 190 and the Leibniz Competition (SAW-2014-ifo-2) is gratefully acknowledged.

To this end, I look at preferences for increases of public education spending for different education areas from early education to university in a representative sample of the voting-age population in Germany. The sample contains more than 4,000 respondents and is randomly split into a control and a treatment group. The treatment group of respondents receives information that, according to numerous studies, investments in earlier education yield greater benefits for the future prosperity of society than investments in later education levels. Then, respondents are asked to state their preference for what education level should benefit from additional government spending on education. The control group answers the same question without receiving any information.

Analysis shows no evidence for the special interest groups model. Public preferences for education spending do not differ between respondents with high or low incomes, contrary to the model's predictions (Su, 2006). In addition, preferences in the control group are consistent with the current allocation. Only 45 percent of respondents favor additional spending on early education levels: 15 percent for early childhood education, and 30 percent for elementary school. In contrast, 41 percent of respondents favor additional spending for secondary schools: 9 percent for vocational schools and 6 percent for universities. Hence, consistent with the status quo, the majority of respondents (55 percent) favor additional spending for later education.[30]

The survey experiment reveals that misconceptions among the public are consistent with inefficient allocations of education spending: Information on the efficacy of early education spending shifts the majority's preference toward spending on earlier education levels. In the treatment group, 66 percent of respondents favor allocating additional spending to early education or primary schools, an increase of 21 percentage points compared to the control group. The largest increase is for early education (16 percentage points), with a smaller increase for elementary schools (5 percentage points). For later education levels, support drops by 14 percentage points for secondary schools, 4 percentage points for vocational schools, and 2 percentage points for universities. Furthermore, the preferences for additional education spending correlate with respondents' beliefs (elicited earlier in the survey) at what education level additional public spending would have the greatest benefit for the country's future prosperity. This corroborates the earlier conclusion that perceived benefits of additional spending are an important determinant of public preferences.

[30] The sum of numbers can deviate from those reported in the text due to rounding.

Further analysis confirms the robustness of this interpretation. In a subgroup analysis, I find that this treatment effect is also present in an oversample of parents with school-aged children, who are a particularly relevant group in education policy. In a separate experiment, I additionally confirm that the effects of information persist over time in a sample of university students. Two weeks after treatment, students who received the information are 29 percentage points more likely to think that additional spending on early education would be most beneficial than the uninformed control group, and are 21 percentage points more likely to favor additional spending in this area. This suggests that treatment effects are due to genuine belief updating rather than artifacts of the survey design.

My results contribute to several strands of the literature. Most importantly, they add to the political economy literature on the consequences of misconceptions (Romer, 2003) by providing empirical evidence on public preferences for the allocation of education spending across levels (Su, 2006; Gradstein, 2003, Bursztyn, 2016; Glomm et al., 2011). Moreover, this chapter contributes to the literature on the potential of information treatments to mitigate the effects of imperfect information (see chapter 2, as well as for example Schueler and West, 2016; Cruces et al., 2013; Hastings et al., 2016; Elias et al., 2015; Kuziemko et al., 2015; Hoxby and Turner, 2015; Di Tella et al., 2012; Gilens, 2001). While this literature documents imperfect information in various domains, to my knowledge this research is the first to study misconceptions in preferences for the allocation of education spending. The analysis also relates to experimental studies that inform survey respondents about scientific findings (e.g. Elias et al., 2015; Alesina et al., 2018; Lergetporer et al., 2017), and extends this literature by studying preferences for education policy.

The chapter proceeds as follows. Section 3.1 provides background information on the inefficiencies of education spending in Germany and develops the theoretical framework for allocation of public spending across education levels. Section 3.2 describes the opinion survey and the experimental design in more detail. Section 3.3 reports evidence for the special interest group model. Section 3.4 presents evidence for the misconceptions model, including results of the survey experiment. Section 3.5 discusses the robustness of the information effects over time. Section 3.6 offers a discussion of the findings.

3.1 Institutional Background and Theoretical Framework

To start, this section provides institutional background and a theoretical framework for the survey experiment described below. First, section 3.1.1 offers background information on education spending in Germany. Then, section 3.1.2 proposes a theoretical framework of inefficient voting outcomes.

3.1.1 Inefficiencies in Education Spending in Germany

Economic theory of education spending suggests that there are dynamic synergies in the acquisition of skills over the life cycle. Skill attainment in earlier periods increases the productivity of learning and hence skill attainment in later periods, leading to the conclusion that skill begets skill (Cunha et al., 2006). This literature concludes that average effects of both private and public investments in human capital are greatest in early childhood and tend to decline with the age (Heckman, 2008). One important implication for the allocation of public spending is that early investments can be both efficient and equity-enhancing, while later human capital investments are generally more effective for those who have a higher baseline level of skill (Woessmann, 2008a). From an empirical perspective, a consensus is emerging that high-quality early childhood education can have substantial returns, especially for children from disadvantaged backgrounds (Elango et al., 2015; Garces et al., 2002; Ludwig and Miller, 2007). While many of the early findings in this literature rely on U.S. data, more recent work draws similar conclusions for Germany (Cornelissen et al., 2018; Felfe and Lalive, 2014), Norway (Havnes and Mogstad, 2011), and Denmark (Gupta and Simonsen, 2016). Overall, the literature suggests that public investments in early childhood education are a promising avenue for equity-enhancing policy.[31]

In light of this evidence, the German government has been criticized for investing too little in early education compared to high spending on tertiary education (OECD, 2016a). Proponents of increased spending for early childhood education argue that the German education system is characterized by costly high-quality childcare (for a discussion of the preschool education system in Germany, see Felfe and Lalive, 2014). At the same time, the German public university system is tax-funded and generally does not charge tuition fees (for an overview of this political debate, see Lergetporer and Woessmann, 2018).[32] The current figures for public expenditures per student as a share of GDP per capita are illustrative. While Germany spends 16 percent of per capita GDP on preprimary education, it

[31] However, even if an intervention is successful overall, there might be heterogeneities in impact. For example, preschool education seems to have no or even negative effects if state-funded preschools crowd out high-quality parental investment (Heckman et al., 2016; Fort et al., 2016). Furthermore, it is unclear whether preschool improves children's outcomes directly or through other channels, for example, changes in parental investment (Elango et al., 2015). Also, interventions are usually evaluated by their effect on labor-market or life outcomes, not necessarily allowing conclusions on the effects of interventions on individuals' utility.

[32] As students from advantaged backgrounds are overrepresented among university students, public higher education funding in Germany is highly regressive (Middendorff et al., 2013)

spends 31 percent on tertiary education (own calculations; OECD, 2016b).[33] Thus, in Germany, spending on early education is low compared to spending on tertiary education.[34] The next section introduces a theoretical approach to potential obstacles inherent in the political economy of education spending.

3.1.2 Theories of Inefficient Voting Outcomes

The puzzle of whether inefficient policies can persist in democratic settings has received considerable attention in the literature. This chapter tests the relevance of two models of inefficiency. The first model focuses on the role of special interest groups in voting outcomes. A second approach, the misconceptions model, shows that inefficient outcomes can also occur in case of imperfect information (Caplan, 2007; Romer, 2003). This section develops both arguments in the context of public spending on education.

A Model of Special Interests The first explanation for how the political process can lead to inefficient policies is that not all citizens have equal representation in voting outcomes. Instead, it is possible that special interest groups acting in their own best interest are able to manipulate voting outcomes in their favor (Acemoglu, 2003). For example, it is commonly assumed that individuals with higher income have higher weight in the political process (Karabarbounis, 2011; Campante, 2011). Starting from this assumption, Su (2006) shows that the hierarchical structure of the education system leads to conflicts of interest within society: Since children from wealthy family backgrounds are more likely to attend higher education, it can be optimal for high-income voters to favor spending increases for tertiary education at the expense of spending on early general education to maintain the exclusivity of their own children's education.

The special interest group model generates two clear predictions for preferences on public education spending: First, individuals with high incomes will be more likely to favor additional spending for later education than low-income individuals. Second, the observed policy outcome will not correspond to the preferences of the median voter, but be skewed

[33] The share of public spending on early education in comparison to spending on tertiary education is also smaller in Germany than in the average OECD country. In Germany, public expenditure per student on preprimary education is 15.57 percent of GDP per capita, on primary 17.07 percent, for lower secondary 20.78 percent, for general upper secondary 22.40 percent, for vocational upper secondary 19.79 percent, for postsecondary non-tertiary education 11.02 percent, and for tertiary 31.24 percent. The corresponding numbers for the OECD average are 17.12, 20.14, 23.72, 20.62, 21.88, 13.76, and 28.21. Calculations are based on indicators B1.1, B3.1a and b, C2.3, and X2.1 from the OECD (2016b).

[34] Recent international tests results have drawn renewed attention to the issue of early education funding, as they show that the correlation between academic achievement of primary school students and their socioeconomic background has significantly increased in Germany over the past decade (Hussmann et al., 2017).

toward the preferences of the more influential group. The data from a representative sample of the German population allow me to test the validity of these predictions.

A Model of Misconceptions A second possible explanation of inefficient political outcomes focuses on voters' misconceptions. While the evidence for widespread ignorance in various domains of public policy is vast (e.g., Gilens, 2001), imperfect information among the electorate is not a sufficient condition for inefficient voting outcomes (Wittman, 1995). For example, biased preferences of individual voters need not lead to inefficient voting outcomes if laws of large numbers apply, or if the decision to vote is itself endogenous, with better-informed voters turning out at higher rates. However, Romer (2003) proves that if misconceptions exist, inefficient outcomes are possible even if the electorate is large and voter participation is endogenous—as individual voters are likely to draw the same fallacious conclusions, there is systematic bias to society's ignorance.[35] The model shows that if individuals' errors are correlated, the population votes against a policy reform that would be beneficial for every member of society with positive probability.

There is reason to assume that system bias, i.e. misconceptions, could also play a role in the political economy of education policy.[36] If, for instance, the electorate on average underestimate the benefits of early education spending or overestimates the benefits of later education spending, voting outcomes might be inefficient as a result of these misconceptions. The survey experiment tests whether information changes the public's spending preferences, as would be expected if initial beliefs are biased. I complement the experiment by evidence on respondents' beliefs on the beneficial effects of spending at different education levels (see section 3.1.3 for details).

3.2 Opinion Survey and Experimental Design

3.2.1 Data

The data used in this chapter are from the ifo Education Survey 2015, and was again sampled and weighted to be representative of the German voting-age population.[37] The sam-

[35] For example, among the people who do not correctly answer that a cannon ball and a feather will fall equally fast in vacuum, almost everyone will incorrectly assume the cannon ball is faster, while very few will incorrectly assume the feather falls faster (Romer, 2003).

[36] For example, the return to investment in education is inherently hard to observe given that benefits materialize only in the very long run. Also, the role of education institutions, and hence their financing needs, are directly affected by changes in the composition of families, which might be underestimated by the general population.

[37] As in chapter 2, respondents answered the survey electronically, either online (80 percent) or as part of a face-to-face interview that employed a handheld tablet device (remaining 20 percent).

ple of 4,204 respondents includes an oversample of 1,744 parents of school-aged children, which allows detailed analysis of an important special interest group in the political economy of education policy. The survey was conducted in May 2015 and comprised a total of 34 opinion questions, as well as a wide range of socioeconomic background variables.[38]

If respondents chose not to answer a question, a pop-up notification encouraged them to do so; if they still preferred to skip the question, they were taken to the next question. Overall, item non-response is very low, 1 percent on average, across all questions used in this chapter. Appendix Table A3.1 (column 1) shows descriptive statistics for the samples' socio-economic background characteristics.

3.2.2 Experimental Design

The survey was designed to answer the following questions. First, how should increases in education spending be allocated according to public preferences in Germany? Second, does information on the benefits of education spending across education stages change public preferences?

To address these questions, respondents were asked to choose which level of education should benefit from additional public education spending. The question was worded as follows (see Appendix Table A3.2 for a summary of all question wordings): "Numerous studies show that education is important for the future prosperity of society. Suppose the government plans an increase in education spending. If only one level of education can benefit from this increase, which area should it be in your opinion?" Respondents were asked to choose one of the following answer categories: "early education," "elementary schools," "secondary schools," "vocational schools," or "universities and universities of applied sciences."[39]

[38] A particularly important background variable in this chapter is monthly net household income, which was recorded in 18 bins from "below 400 Euro," …, to "5,000 Euro and more." Following the convention in the literature, the first bin is assigned the border value multiplied by a factor of 0.75, each following bin is assigned a value equal to the average of the upper and lower border, and the last bin is assigned the border value multiplied by a factor of 1.5 (Katz and Autor, 1999). Throughout the chapter, income is reported in 1,000 Euro.

[39] The wording for the answer category "early education" included all types of childcare institutions that typically enroll children between the ages of 1 and 6 years old. "Secondary school" in Germany commonly refers to a school of the tracked system, for example, Gymnasium. Vocational schools are an integral part of Germany's apprenticeship system, which combines formal schooling with in-compary training. These schools are typically specialized in a few professions and provide additional academic training to apprentices, for example, lessons in optical physics for optometrists. Throughout the rest of this chapter, I refer to "universities and universities of applied sciences" as universities.

The experiment implemented in the survey was designed to establish whether information provision can shift preferences for public education spending. To this end, a randomly selected group of respondents were randomly assigned to a treatment condition. The treatment was designed to provide information reflecting current findings from economic theory (see section 3.1.1) in an easily accessible way. Thus, the introductory sentence in the treatment condition was changed to: "Numerous studies show that spending for early childhood education has a more beneficial effect on the future prosperity of society than spending in later areas of education." Otherwise, the question was presented exactly the same as the question in the control condition.[40] Respondent-level randomization of treatment status allows me to cleanly identify the causal effect of information provision.[41]

Lastly, the survey elicits respondents' prior beliefs by asking them to guess at what level of the education system addition education spending would have the greatest benefit. The question, which was asked before the survey experiment, was worded in the following way:[42] "What is your best guess, in which one of the following areas would additional public spending have the most beneficial effect on the future prosperity of society?"[43] Again, respondents choose one of the five answer categories from early education to university. Respondents also indicate how sure they were that their answer was close to correct on a seven-point scale ranging from "very unsure" to "very sure."

[40] Survey experiments can be susceptible to priming effects, where the use of specific words might change responses momentarily in a subconscious reaction to the treatment (Bargh and Chartrand, 2000). In order minimize the possibility that such unintended priming effects arise, the question wording for the control and treatment groups differed only in the information content of the introductory sentence. Both experimental groups read that numerous studies find that education spending is important for the future prosperity of society. The only difference in wording was that respondents in the treatment group were told that research supports spending for early education. Therefore, all changes in responses caused by other elements of the question wording, for example, the focus on the future prosperity of society or the mention of scientific studies, are present in both the control and treatment groups and hence do not bias the estimation of treatment effects.

[41] A subset of respondents in the treatment group additionally received the information that Germany spends less than the EU average on early childhood education, but more than the EU average on tertiary education. Since there are no significant differences in answers between treatment groups, results are pooled for the purpose of this chapter.

[42] This question was asked at the beginning of the survey (15 questions earlier than the main outcome question) to avoid behavioral responses that might arise from providing information in a way that is perceived as a correction of previously stated beliefs.

[43] If we assume that respondents' only concern is to maximize future prosperity of society, we would expect to see a perfect correlation between the answers to the question of greatest benefits and the question of spending preferences. Different answers should therefore be interpreted as both driven by classical measurement error and by respondents whose answers are not determined by maximization of the future prosperity of society.

3.2.3 Empirical Framework

This section describes the empirical strategy I use to test the predictions of the special interest group model and the misconceptions model. Since the outcome of interest is categorical, I use multinomial logit models for estimation.[44]

First, I assess the relevance of the special interest group model by regressing spending preferences on income:

$$P(y_i = j) = F(\alpha_0 + \alpha_1 Income_i + \alpha_2{}' X_i + \varepsilon_i) \qquad j = 1, \ldots, 5 \qquad (1)$$

where y_i is an indicator equal to *j* if individual *i* favors increased spending for education level *j*, $Income_i$ is a measure of individual *i*'s net household income, X_i is a vector of control variables, and ε_i is an error term. The coefficient of interest is α_1, the estimated effect of income on the probability of choosing category *j*.

Second, to assess the relationship between respondents' baseline beliefs and public preferences for education spending, I estimate the following regression model:

$$P(y_i = j) = F(\beta_0 + \textstyle\sum_k \beta_1^k x_{ik} + \beta_2{}' X_i + \vartheta_i) \qquad j, k = 1, \ldots, 5 \qquad (2)$$

where again y_i is an indicator equal to *j* if individual *i* favors increased spending for education level *j*, x_{ik} is an indicator that equals 1 if respondent *i* estimated that benefits of additional public spending would be greatest for education level *k*, X_i is the vector of control variables, and ϑ_i is an error term. In this specification, the coefficients $\beta_1^1, \ldots, \beta_1^5$ describe the relationship between prior information and spending preferences. For example, β_j^j represents the difference in the probability to favor spending on education level *j* for respondents who do and do not think that benefits are largest for spending on *j*.

Finally, I test the impact of the information treatment on spending preferences by estimating the following regression model:

$$\mathrm{P}(y_i = j) = F(\gamma_0 + \gamma_1 Treatment_i + \gamma_2{}' X_i + \tau_i) \qquad j = 1, \ldots, 5 \qquad (3)$$

[44] An alternative approach would be to use the hierarchical structure of the education levels to estimate an ordered regression model. Results do not change under this specification. However, due to the early tracking between general and vocational education in Germany, there is no natural progression for the categories "secondary schools", "vocational schools", and "university". Since LR and Wald specification tests reject the null hypothesis of parallel regressions for an ordered probit model. I estimate the multinomial model as my preferred specification.

where $Treatment_i$ is an indicator equal to 1 if individual *i* received the information treatment, and τ_i is an error term. The parameter of interest, γ_1, captures the effect of information provision on the probability of choosing spending category *j*.

The treatment effect γ_1 in equation (3) is identified by random assignment to information provision. Therefore, the inclusion of covariates, X_i, should not affect the magnitude of the estimated causal effect, but may increase precision. In contrast, estimates of α_1 and β_1 in equations (1) and (2) do not have a causal interpretation due to potential endogeneity and might be sensitive to the inclusion of covariates. Throughout the chapter, I present estimation results with and without additional covariates.

3.3 Evidence on the Special Interest Group Model

This section provides results for the special interest group model introduced in section 3.1.2. As outlined above, a first prediction of the model is that policy outcomes should generally be at odds with majority opinion of the electorate. In contrast to this prediction, the bottom row of Table 3.1 shows that the German public's preferences for education spending are consistent with low levels of investment in early education. More spending for early childhood education is preferred by 14 percent of respondents and more spending for elementary schools by 30 percent. Allocating increased spending to secondary schools is the most favored option for 41 percent, to vocational schools for 9 percent, and to universities for 5 percent of respondents. Thus, the majority of respondents do not favor an expansion of spending on the early education levels.

Estimating equation (1) also shows very limited support for the prediction of the special interest group model. According to the theory of special interests, we would expect a negative correlation of higher income with spending for early education, which disproportionately benefits poorer families, and a positive correlation with spending on universities, which mostly benefits children from more advantaged backgrounds. The two panels of Table 3.1 report average marginal effects of a 1,000 Euro increase in monthly household income on spending for each of the five education levels. As it turns out, effects are very small at 1 percentage point or less, indicating that respondents with higher household income are neither more nor less likely to support spending on any level of education. Note that because of potential endogeneity concerns, the estimated coefficients do not necessarily capture the causal effect of an income increase. When controls for age, education, and other sociodemographic characteristics are included, the correlation of income and spending on universities gains significance but remains very small in magnitude. These results do not support the hypothesis that high-income respondents purposely re-

strict funding to early education levels as predicted by the special interest group model. Instead findings show that the preferred allocation of spending across education levels does not differ by socioeconomic status.[45]

3.4 Evidence on the Misconceptions Model

This section presents three sets of evidence on the misconceptions model. First, I provide descriptive results on respondents' beliefs about where additional education spending would have the greatest benefits, and assess how these beliefs relate to spending preferences. Second, I discuss the experimental evidence for the effect of information provision on spending preferences. A concern for the interpretation of the survey experiment might be that the allocation of education spending across different levels is neither a particularly salient issue nor directly relevant for a majority of the German public. Therefore, the survey experiment might overestimate the malleability of preferences in response to information compared to the effects for stake-holders (Benabou and Tirole, 2016). To test this possibility, the third set of results tests the robustness of results for the subgroup of parents with children below the age of 25. Finally, the section concludes with further evidence of heterogeneities of the experimental estimates and a discussion of the role of interviewer demand effects.

3.4.1 Descriptive Evidence on Prior Beliefs and Spending Preferences

Respondents' beliefs about where additional spending would have the greatest benefits are consistent with the notion that respondents misconceive the benefits of additional spending in the different areas. The bottom row of Table 3.2 depicts the shares of respondents who believe that additional education spending would be most beneficial at the respective spending level. The distribution of beliefs is similar to spending preferences described above: 25 and 26 percent of respondents estimate that increased public spending would be most beneficial at the early education and elementary school level, respectively. The largest share of 35 percent thinks that benefits are greatest for additional spending on secondary schools, while 8 percent and 6 percent of respondents believe benefits are greatest for spending on vocational school and university.

[45] This conclusion equally holds if I compare the spending preferences of respondents with different education attainment, an alternative measure of socioeconomic status. Results by education (not reported) show that respondents who hold a university entrance qualification are (marginally) less likely to favor additional spending for elementary schools and more likely to favor additional spending for university (not robust to the inclusion of controls). The theoretical prediction of negative effects of higher status on early education spending is again not confirmed in the data.

The majority of respondents (61 percent) prefer spending increases in the category which they believe would yield the largest benefits.[46] For each education level, the two panels of Table 3.2 report the coefficients β_j^j from estimating equation (2).[47] Results show that for all five education levels, there is a strong link between the expected benefits of additional spending for the future of society and spending preferences. The probability to favor investments in a given category is between 40 and 49 percentage points higher for respondents who think investments will be most beneficial in this category (32 percentage points and 47 percentage points, respectively, in a specification with controls). Overall, estimated benefits for investment in different education levels closely mirror preferences for the allocation of public education spending. While the strong correlation suggests that beliefs on the greatest benefits for society might drive spending preferences, it does not necessarily reflect a causal effect if respondents' with different beliefs also differ along other unobserved dimensions. The next section addresses this question by reporting findings from the randomized survey experiment.

3.4.2 Experimental Evidence

The misconceptions model developed in section 3.1.2 predicts that if misconceptions of the electorate are a concern in the political economy of education spending, information provision should affect preferences for education spending. I test this prediction empirically by informing respondents about findings from studies that estimate that the benefits of additional education spending are generally higher for young children. This experiment allows us to assess to what extent the correlation between estimated benefits and preferences for education spending discussed above represents a causal relationship between beliefs and preferences.

The results of the survey experiment show that respondents change their answers substantially when they receive information, which is consistent with the prediction of the misconceptions model (see Figure 3.1). Table 3.3 reports average marginal effects of treatment based on equation (3) and shows that the treatment increases the share of respondents who favor additional spending on early education by 16 percentage points to a

[46] The joint distribution of answers, with a correlation of 0.55, is summarized in Appendix Figure A3.1. Further analysis shows that respondents are less likely to answer the question on the highest benefits differently than the question on spending preferences when they have higher educational attainment or when they are sure about their guess (see Appendix Table A3.3).

[47] For ease of exposition, I report only the diagonal entries (β_i^j with $i = j$) for each of the five education levels.

total of 31 percent (see Panel I).[48] For elementary schools, spending support increases by 5 percentage points to 35 percent. In contrast, the treatment significantly decreases support for additional spending on all later education levels. For secondary schools, the share that favors additional spending falls by 14 percentage points to 26 percent; for vocational schools, it falls by 4 percentage points to 5 percent; and for university by 2 percentage points to 3 percent. Taking into account that support for vocational schools and university started from low baseline levels, this implies a reduction by more than a third for each of these education levels. As expected given randomization, the inclusion of control variables does not change these results.

Overall, the experimental results show that information on the benefits of education spending shifts the majority in favor of increased investment in early education levels. A strict interpretation of the treatment information would imply that additional funds should be invested in early childhood education. Indeed, we observe the strongest gain in support for this category. In a weaker sense, the treatment information implies that investments are more effective the earlier they are made. The positive treatment effect for elementary schools indicates that respondents see spending in this area similarly as investment in early education. Looking at the joint support for spending on early childhood education and elementary schools might therefore be insightful. For the education levels prior to secondary schools, the information treatment turns a minority of 45 percent in support for increased spending into a majority of 66 percent (both shares are different from the simple majority of 50 percent at the 1 percent level of significance). In this sense, providing information to correct potential misconceptions changes the efficiency of the majority opinion.

The heterogeneity of treatment effects with respect to the certainty with which respondents hold their beliefs about the benefits of education spending provides evidence that the observed treatment effects are due to belief updating. The upper (lower) part of Panel II in

[48] In a large sample, randomization yields unbiased estimates of treatment effects. To test whether randomization successfully balanced respondents' observable characteristics between treatment and control groups, Appendix Table A3.1 reports estimates from the following regressions:

$$Treatment_i = \alpha + \beta\, Covariate_i + \varepsilon_i \quad (4)$$

where $Treatment_i$ is individual i's treatment status and $Covariate_i$ is individual i's value for each control variable. With only three exceptions, the assignment to treatment and control group balances the observable characteristics well (one marginally significant, two at the 5 percent level). When all controls are included as regressors, the null hypothesis of joint significance is rejected with an F-test p-value of 0.1208. Overall, these findings suggest that randomization was successful.

Table 3.3 shows estimates of equation (3) separately for sure (unsure) respondents.[49] As we would expect from models of belief updating (Benabou and Tirole, 2016), treatment effects of information provision tend to be larger for respondents who are less sure about their prior beliefs.[50] For instance, the treatment effect of information provision on preferences for spending on early childhood education is 18 percentage points for unsure respondents, but only 12 percentage points for sure respondents (difference marginally significant). The additional increase in support for early education is mirrored by a decrease in support for secondary schools and universities, where treatment decreases by 15 percentage points and 3 percentage points for unsure respondents compared to 13 percentage points and null percentage points for sure respondents. The treatment effects for spending on elementary schools and vocational schools are of the same magnitude for both groups. Overall, treatment effects are still very large in the group of respondents who reported high certainty, suggesting high malleability of spending preferences in general. Thus, both the evidence of the previous section and the results of the experiment suggest that imperfect information is an important piece of the puzzle for understanding preferences for education spending.

3.4.3 Education Spending Preferences of Stake-Holders: Parents

The allocation of public education spending is likely to be a particularly salient issue for parents. As long as their children are still in school, parents are likely to have considerable insight into the strengths and weaknesses of the education system. Arguably, they hold stronger prior opinions on the optimal allocation of resources to schools, not least because they are directly affected by education policy. If the results so far are driven by respondents who react strongly to information because they do not have a prior opinion on the issue, we would expect that treatment effects are lower for stake-holders. The following section investigates this hypothesis with data from an oversample of parents with children of school age.

Results show that findings from the previous sections hold equally well for this subgroup. Table 3.4 replicates the findings from the general population for the oversample of parents. In the main specification, parents include all respondents who report having a child

[49] I define sure respondents as those who indicated a value of 5 or higher on a seven-point scale of how sure they are their answers are correct, unsure respondents those with a value of 1 to 4. As almost a third of respondents chose the middle category of certainty, the arbitrary assignment of these respondents to the unsure group might be a concern. However, results are robust to an alternative specification that includes the middle category in the definition of sure respondents (not shown).

[50] Further analysis suggests that this difference is not driven by experimenter demand effects (see section 3.4.5 for details).

below the age of 25.[51] The bottom row shows that spending preferences are very similar to those of the general population. Parents are significantly less likely (by 5 percentage points) to favor spending for early education and significantly more likely (by 10 percentage points) to favor spending on secondary schools;[52] however, the magnitudes of these differences are such that they do not change the conclusion that the majority favors spending for later education areas. As the first panel of Table 3.4 shows, there is no evidence that parents with higher household income favor spending for early education less than parents with lower household income. Only support for additional spending on secondary schools decreases slightly by 3 percentage points per 1,000 Euro of household income (marginally significant). As for the general population, this analysis suggests that the prediction of the special interest model does not describe parents' preferences.

The second panel of Table 3.4 shows the estimated treatment effects for the subsample of parents with children below the age of 25. Parents in the treatment group are 19 percentage points more likely to favor increased spending on early education compared to parents in the control group.[53] Similarly, the treatment increases support for spending on elementary schools by 6 percentage points and decreases support for spending on secondary schools (20 percentage points) and vocational schools (5 percentage points), while leaving support for spending on universities unaffected. Thus, for spending on early education (marginally significant) and secondary schools, the treatment effects of the subgroup of parents are even larger than for respondents without children below the age of 25 (see Appendix Table A3.5). This is contrary to what would be expected if the effects of the information treatment were driven by respondents who do not have strong opinions on issues of education spending. Finally, the third panel of Table 3.4 highlights that the relationship between prior beliefs about the benefits of additional spending and spending preferences are as aligned for parents as for the general population. These additional tests suggest that even if we limit the analysis to those directly affected, we find little support for the special interest group model and strong support for the misconceptions model.

Another advantage of looking at parents is that it allows insight into the importance of self-interested answering behavior by respondents. For example, parents of two-year-old children might support additional funds for early education, while parents of 20-year-olds

[51] The effects of information provision hold equally for alternative definitions of parents, namely, parents who state that their children currently live in their household, as well as parents whose children are still in the education system.

[52] See Appendix Table A3.4 for details. Parents are also significantly less likely to guess that further spending for university is most beneficial; answers are not significantly different for the other education areas.

[53] The difference in treatment effects between parents and respondents without children below the age of 25 is marginally significant (see Appendix Table A3.5).

might be more likely to favor additional funds for vocational schools or universities. I test this hypothesis by regressing a dummy equal to 1 if any of the respondent's children attend the respective education level on the spending preference indicated by the parent. For this analysis, I limit the sample to respondents in the control group so as not to confound level differences with differences in treatment effects.[54] The average marginal effects are reported in the first panel of Table 3.5.[55] A parent whose child attends a certain education level is generally more likely to answer that spending should benefit this education level. For example, the probability to favor additional spending on elementary schools is 10 percentage points higher among parents who have a child attending elementary school than among other parents. The association is also positive for early education (3 percentage points, not significant) and secondary school (18 percentage points), and close to zero for vocational school and university. This pattern is even stronger for the estimate as to which area spending would be most beneficial, where the correlations are sizeable and significant for early education (13 percentage points), elementary school (12 percentage points), and secondary school (10 percentage points). A possible explanation consistent with this observation is that having children in the education system increases the salience of funding needs at their institution, and hence raises the perceived return to investment for that education level.[56] In conclusion, it seems that while self-interested answering behavior is observed for parents, this effect is stronger for the estimate of benefits than for preferences on spending increases. In addition, the results on parents' preferences show evidence that misconceptions are also widespread among this interest group, and hence suggest that a lack of concern for education spending is not driving the large effects of information provision.

3.4.4 Heterogeneities of Treatment Effects

In this section, I present an explorative analysis of treatment effect heterogeneities across regional and sociodemographic subgroups of the German population.

[54] In fact, there is sizeable heterogeneity in treatment effects among different parents: the treatment effect for parents of small children, for whom self-interest and common interest as suggested by the information treatment run in the same direction, is twice as large as the treatment effect for parents of children between primary school age and 25 years old (not shown).

[55] These results hold in specifications with and without further socioeconomic controls.

[56] If parents' answers are self-serving and forward-looking, they might favor additional spending for education levels that their child will attend in the future, for example, they might favor spending on secondary schools while their child is in elementary school. However, regressions that allow for parents to favor any education level that their child attends now or is likely to attend in the future estimate correlations of a similar magnitude (not shown). Furthermore, I find no evidence that parents are more likely to support spending for the next higher education level when their children are approaching the typical age of transition (not shown).

Local Conditions While the wording of questions is concerned with education spending in Germany as a whole, respondents might still intuitively answer in light of local circumstances, for example, depending on the spending needs they observe in their region. In this section, I focus on early education spending, where I observe the largest treatment effects. To test whether experimental results vary by current policy, I use administrative data on the share of children below the age of six who are enrolled in formal daycare (Statistisches Bundesamt, 2014a; 2015). This dataset is matched to my survey data at the municipal level, allowing a close approximation of local conditions. Appendix Figure A3.2 shows the correlation between enrollment share and support for additional spending on early education (aggregated to the state level for ease of exposition). Additional estimates show that in the control group, respondents in municipalities with a higher than median share of children in early education are not more or less likely to favor additional spending for early education. The experimental results show that the treatment effect of information provision for spending on early education is 8 percentage points larger in the group of high-attendance municipalities than in municipalities with below median attendance (not shown). It is well documented that individuals typically prefer information that is in line with their preferences (Benabou and Tirole, 2016). The finding of higher treatment effects in high-attendance municipalities is therefore consistent with higher unconditional preference for early education spending in high-attendance municipalities and higher willingness to take into account information consistent with one's preferences. I conduct a similar exercise using data on the public spending on early education in each state (Textor, 2015). The relationship between current government spending and support for additional spending is shown in Appendix Figure A3.3. The results show that control-group respondents in states that spend above median on early education are neither more nor less likely to favor further spending on early education, and have the same treatment effect as respondents from states that spend below the median.

Sociodemographic Characteristics In addition to our detailed analysis of parents, the rich set of demographic characteristics allows me to estimate treatment effects for other relevant subgroups of the population. Figure 3.2 provides an overview of the treatment effects for different subgroups, showing that effects are very homogeneous. For ease of exposition, the figure reports the effects of the treatment on the likelihood of favoring increased spending on early education areas, that is, early education and elementary schools combined.

For instance, respondents who are employed in the education sector are another important special interest group with vested interests in questions of education spending. For this group, treatment effects do not differ significantly from treatment effects for the

general population. The same is true for respondents who report they vote regularly and those who consider education topics a priority when casting their vote. Similarly, treatment effects are of the same magnitude for respondents in West Germany, respondents who have above-average news media exposure, and respondents who vote for the Social Democratic Party (rather than the Christian Democratic Union). I also estimate the effect of information for grandparents, and again find results no different than those for the overall population.[57] Only the treatment effect for parents of very young children is significantly larger than the results reported for the German population overall. These findings again show no evidence that influential groups of stakeholders hinder reform of education spending allocation in Germany. Overall, the results of this section show that misconceptions are not confined to certain groups, but are both wide-spread and malleable across the German population.

3.4.5 Interviewer Demand Effects

A potential concern with the findings reported so far is the possibility of interviewer demand effects. The wording of the treatment information might send a signal to respondents about what the experimenter considers to be the "right" answer, which can leave respondents reluctant to express disagreement (Cavallo et al., 2014).

To investigate whether treatment effects are driven by these demand effects, I make use of the different survey modes for our offline and online sample. Although respondents in both groups answered the same computerized survey, for the offline respondents an interviewer was present at the time of the interview. A model of interviewer demand effects would predict that the loss of privacy compared to the online sample will increase demand effects for respondents in the offline sample (Rosnow and Rosenthal, 1997). As Appendix Table A3.6 shows, this is not the case in my sample.[58] The interaction effect between the treatment effect and the interview mode is small and insignificant, meaning that respondents in the offline mode do not react differently to the treatment than respondents in the online mode. This suggests that respondents do not react differently to the information treatment in the presence of an interviewer. Furthermore, including controls for age and background characteristics yields an insignificant estimate of the coefficient on the dummy indicating offline status. The absence of mode effects indicates that, conditionally, offline respondents answer questions on education spending across areas

[57] I also include the results for parents of children below the age of 25 (see section 3.4.3), respondents in counties with a high share of children attending early childhood education, and respondents in states with above average spending on early education for completeness.

[58] This result holds both in a sample including all offline respondents and for the sample of offline respondents who asked the interviewer to fill out the survey for them, implying a standard face-to-face interview.

similarly to online respondents, strengthening the validity of the above test. Therefore, it seems unlikely that interviewer demand effects explain the large effects of information provision.

3.5 Persistence of Effects: Evidence from a Convenience Sample

The experimental design of this chapter allows estimating the causal effect of providing respondents with information on studies in favor of investments in early childhood education. However, interpretation of the effects crucially depends on whether the results presented so far are due to true information updating or merely artifacts of the survey design. The persistence of treatment effects supports the notion that these effects are due to genuine belief updating, because experimenter demand and priming effects are very unlikely to yield persistent shifts in respondents' policy preferences (see Grigorieff et al., 2016).

3.5.1 Convenience Sample

To test whether information provision has lasting effects, I collect additional evidence from a convenience sample that allows me to follow participants over a two-week period. The sample consists of 262 students at a German-speaking university. The study design allows me to observe 75 students for two sessions.[59] Students were matched across time through an anonymized code that precludes identification of individual students' answers by the researcher.

In the first session, students were randomly allocated to treatment and control group and, like the main sample, were asked what education level—from early education to university—should benefit from additional spending. As before, students in the treatment group were informed that, according to studies, spending on earlier areas of education had greater benefits for society than spending on later education. In the second session two weeks later, all students answered the question of where they think additional spending would have the greatest benefits for society. Then, all students again answered the question on spending preferences from the first session but without any information provision for either group. Overall, 16 percent of students favored additional spending for early education, 32 percent for elementary schools, 25 percent for secondary schools, 3 percent for vocational schools, and 24 percent for universities (see Appendix Table A3.7). Results show

[59] All students were invited to an experimental lab and paid a standard compensation for their participation. For reasons of logistics, students answered the survey questions via pen and paper rather than on a computer.

that university students are 20 percentage points more likely to prefer spending for university than the general population. They are also less likely to support additional spending on secondary schools (17 percentage points) and vocational schools (5 percentage points). They do not differ in support for early education and elementary schools.

For the student sample, I also conduct an additional analysis to test the prevalence of interviewer demand effects. As suggested in the literature, the extent of interviewer demand can be approximated by asking questions that make the "desirable" answer obvious (Quidt et al., 2017). I use a six-item scale of such questions to generate a measure of social desirability.[60] If demand effects are a major driver of the treatment group's answers, we would expect that students with high social desirability scores are less likely to deviate from presumed interviewer demand and thus exhibit stronger treatment effects than those with low desirability scores. As in the main sample, I find no evidence that demand effects drive the observed treatment effects (see Appendix Table A3.8). Students with high social desirability scores do not react to the treatment more strongly than students with low social desirability scores. To the contrary, as the second panel shows, treatment effects are muted and not significantly different from zero for this group. This suggests that the increase in support for increased spending on early education areas is not driven by students mainly choosing these answers because of demand effects. Interestingly, the largest difference in effects is that students with high desirability scores increase, rather than decrease, their support for increased spending for universities in the treatment condition compared to the control condition (marginally significant). As a result, the effect of treatment on support for university spending is larger for students with low social desirability scores. This suggests an alternative interpretation of the desirability of answers. Given that the convenience sample was surveyed at an experimental lab by university staff, it is possible that students assumed that increased spending for university would be the experimenters' desired answer. In this sense, students with high social desirability might have been more reluctant to react to the treatment information because it contradicted presumed interviewer demand. Overall, the additional evidence strengthens our confidence that demand effects are not the main explanatory factor for the observed treatment effects of information provision.

[60] For example, "My first impression of people is often correct" or "I am always honest towards others," with answers on a seven-point Likert-scale from "not correct at all" to "applies completely." I use questions suggested by Winkler et al. (2006), who develop a scale by choosing questions with the best predictive power from a larger set of commonly used questions related to the Marlowe-Crowne scale. Students are assigned a higher social desirability score if they answer more positively.

3.5.2 Experimental Results

As can be seen from Table 3.6, the immediate treatment effects in the convenience sample mirror the results for the main sample. Students in the treatment group are 22 percentage points more likely to favor increased spending for early education compared to the control group. Support for additional spending on elementary schools also increased by 3 percentage points, although the effect does not reach statistical significance. By construction, the increases in support for early education spending imply less support for spending on later education. While in the main sample the largest reduction is observed for secondary schools, in the convenience sample, support decreases most for spending on university (19 percentage points).

If the treatment merely primes respondents or changes the salience of information already known by the individual, these effects will dissipate quickly after the end of the survey (Kuziemko et al., 2015; Cavallo et al., 2014). However, if respondents are able to recall the treatment information at a later point in time, this implies that they must have read, understood, and processed the information. The second and third panel of Table 3.6 show results two weeks after information provision. Students who received the treatment information in the first session are 29 percentage points (significant at the 1 percent level) more likely to guess that the benefits of education spending are highest for early education than are respondents in the control group. At the same time, they are 14 percentage points less likely to guess that benefits are highest for university (marginally significant). As before, there is no significant effect of treatment on beliefs about the benefits of spending on elementary schools and secondary schools. Consistent with the change in beliefs on the benefits of education spending, students in the treatment group remain 21 percentage points more likely to prefer additional spending for early education. Support for higher spending on elementary schools also increases by 11 percentage points (not significant), while support for spending on secondary schools and university is again lower by 18 percentage points and 14 percentage points, respectively, in the initially treated group (both marginally significant). These results strongly suggest that the estimated treatment effects are indeed due to new information that contrasts with previously held misconceptions, and cannot be explained by priming effects of the information treatment.

It is not clear ex ante to what extent the findings from the student population in the convenience sample generalize to more diverse samples. However, as the bottom rows of Appendix Table A3.7 show, the magnitude of treatment effects is not significantly different for students and the general population with regard to spending on early education. For the later education areas, treatment effects are smaller for spending on secondary schools

and vocational schools in the convenience sample, but larger for spending on university. This is likely due to ceiling effects given the low level of support for increased university spending in the general population and the low level of support for increased spending on vocational schools in the convenience sample. Overall, these additional tests suggest that the preferences for spending on early education of the student sample approximate the preferences of the general public surprisingly well.

3.6 Conclusions

This chapter suggests that the predictions of a special interest group model of political power are not borne out in the context of a survey on education spending. Instead, the findings of the survey experiment support the theory that widely held misconceptions on the benefits of education investments play a major role in explaining the current inefficiencies in spending allocation. Informing respondents about the higher benefits of investments in early childhood education significantly shifts the median respondent to support increased spending in this area. Further tests corroborate both the robustness of this result and that it is, indeed, driven by true information updating.

As in the previous chapter, the findings presented here suggest that the German public is open to changes in the status quo of education spending. However, it is worth noting that the evidence in the education literature on the relationship between increased spending and improved outcomes is all but conclusive (Jackson et al., 2016; Hanushek, 2003). In practice, a well-designed strategy for successful implementation will be crucial to secure the potential benefits associated with any change in education spending. Even though sufficient funding is a necessary condition for satisfactory education outcomes, how the money is spent is likely to matter just as much.

While results suggest that differences in preferences across socioeconomic groups do not contribute to our understanding of education spending, they do not disprove the existence of political pressure groups along other dimensions not measured in this survey. Similarly, if power is concentrated not only in certain groups but lies with certain individuals, a survey sampling approach is unlikely to capture these dynamics. At the very least, the findings of this chapter highlight that misconceptions play a role in explaining preferences in education spending.

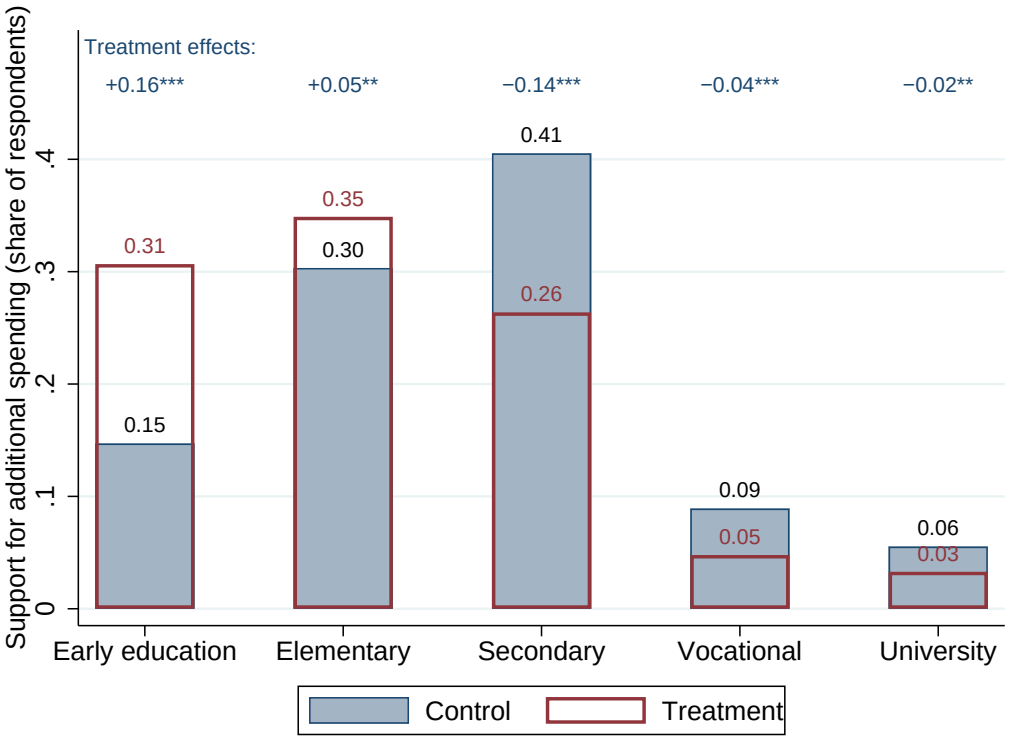
Also, my data does not speak to the origins of the misconceptions held by the German public. The evidence of parents overestimating returns to investments in education areas currently serving their children suggests myopia or salience effects as potential mecha-

nisms. Further research would be necessary to explore these hypotheses and provide more direct tests of possible channels.

Survey experiments necessarily suffer from a certain degree of artificiality. Most respondents will collect relevant information from a variety of sources over extended periods of time rather than reading this information immediately before casting a vote. Also, respondents might still lack relevant information even in the treatment condition. A number of alternative information treatments might also influence public preferences. For example, previous research has found that providing respondents with information on the current spending levels per child shifts preferences in the direction of equalized spending per child across different education levels (Lergetporer et al., 2016). The finding of significant information effects hence raises the broader question of what types of information are relevant for voters' decisions and how different information sets might interact. Further research in this area will contribute important insights into the vulnerability of the political process to misleading or one-sided information.

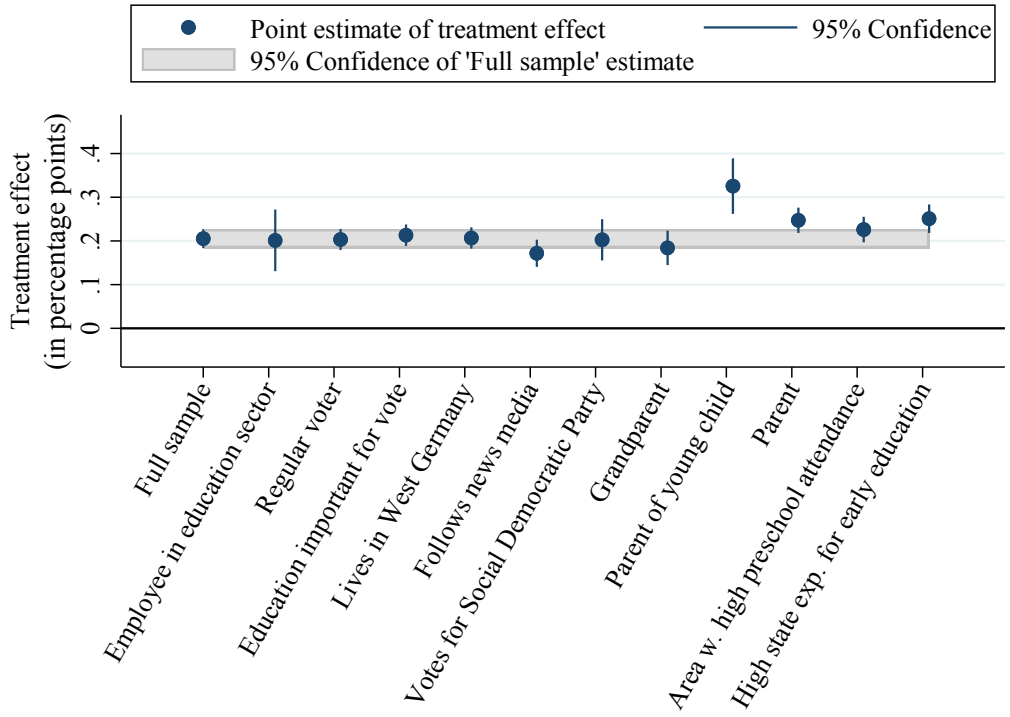
Overall, this chapter shows that information about scientific evidence can have large effects on the preferences of the electorate, suggesting substantial scope for information campaigns to initiate meaningful reforms of the education system. In particular, given the evidence on the benefits of childcare in Germany (see Cornelissen et al., 2018), increased focus on early education could provide one potential avenue to improve the equity of education outcomes. The following two chapters investigate the issue of educational inequality in more detail.

Figure 3.1: Experimental results of information treatment on spending preferences



Notes: Share of respondents who favor each category for additional spending. Treatment: "Numerous studies show that early investments in education have greater positive benefits on the future prosperity of society than investments at later ages." Control: No information. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2015.

Figure 3.2: Differences in treatment effects across subgroups



Notes: Results from 12 separate regressions Dependent variable is a dummy equal to 1 if the respondent favors early education or elementary school for increases in spending. Each point estimate of the treatment effect is from an OLS regression of preferences for additional spending on treatment status in the respective subgroup. As shown by the confidence intervals, all treatment effects are different from zero at the 5 percent level. Regular voter is a dummy coded 1 if the respondent votes "always" or "usually," 0 if respondent votes "sometimes" or "never." Follows news media user is a dummy coded 1 if the respondent reports consuming more news media than the median, 0 otherwise. Votes for Social Democratic Party is a dummy coded 1 if the respondent tends to vote for the Social Democratic Party, 0 if she votes for the Christian Democratic Union. Parent of young child is coded 1 if the respondent has children who do not yet attend elementary school. Parent is a dummy coded 1 if the respondent has children below the age of 25 years. Area with high pre-school attendance is a dummy coded equal to 1 if the respondent lives in a municipality where the share of children who attend preschool is above the median, 0 else. High state expenditure for early education is a dummy coded 1 if the respondent lives in a state that spends more than median on early childhood. Regressions weighted by survey weights. Data source: ifo Education Survey 2015.

Table 3.1: Heterogeneities of spending preferences by income

	Additional spending for				
	Early education	Elementary	Secondary	Vocational	University
(I) Without controls					
Household income	-0.007	0.014	-0.008	-0.008	0.009
	(0.008)	(0.012)	(0.012)	(0.013)	(0.008)
Observations			1,403		
(II) With controls					
Household income	-0.005	0.018	-0.019	-0.009	0.014**
	(0.010)	(0.014)	(0.015)	(0.017)	(0.007)
Observations			1,372		
Control mean	0.145	0.30	0.412	0.087	0.056

Notes: Results from a multinomial logit model. The table reports the average marginal effects of an increase in monthly household income by 1,000 Euro. Dependent variable is the answer to the question: Which education level, given an increase in public education spending, should benefit from the increase in spending? Controls include age, gender, migration status, parental education, municipality size, living with a partner, education, region of residence, having children below the age of 25, employment status, and working in the education sector. Control mean: share of respondents choosing each category in the control group. Regression is weighted by survey weights. Standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2015.

Table 3.2: Spending preferences and subjective benefits for society

	Additional spending for				
	Early education	Elementary	Secondary	Vocational	University
(I) Without controls					
Category has greatest subjective benefit	0.440***	0.469***	0.486***	0.448***	0.404***
	(0.039)	(0.038)	(0.034)	(0.062)	(0.077)
Observations			1,437		
(II) With controls					
Category has greatest subjective benefit	0.409***	0.444***	0.470***	0.441***	0.317***
	(0.041)	(0.038)	(0.034)	(0.067)	(0.082)
Observations			1,370		
Share: category has greatest benefit	0.251	0.257	0.352	0.078	0.062

Notes: Results from a multinomial logit model. The table reports the average marginal effects of beliefs that spending has the largest benefits for education level *j* on preferences for spending on education level *j*. Dependent variable is the answer to the question: Which education level, given an increase in public education spending, should benefit from the increase in spending? Controls include age, gender, migration status, parental education, municipality size, living with a partner, education, income, region of residence, having children below the age of 25, employment status, and working in the education sector. Last row: share of respondents choosing each category. Regression is weighted by survey weights. Standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2015.

Table 3.3: Effects of information on benefits of spending for earlier education levels

Panel I: Full sample

	Additional spending for				
	Early education	Elementary	Secondary	Vocational	University
(I) Without controls					
Treatment	0.160***	0.046**	-0.142***	-0.041***	-0.022**
	(0.016)	(0.020)	(0.021)	(0.012)	(0.011)
Observations	4,223				
(II) With controls					
Treatment	0.154***	0.057***	-0.143***	-0.044***	-0.023**
	(0.016)	(0.02)	(0.021)	(0.013)	(0.011)
Observations	4,013				

Panel II: Results by certainty

	Additional spending for				
	Early education	Elementary	Secondary	Vocational	University
(I) High certainty of prior estimate					
Treatment	0.122***	0.049	-0.125***	-0.037*	-0.009
	(0.028)	(0.032)	(0.031)	(0.019)	(0.014)
Observations	1,723				
(II) Low certainty of prior estimate					
Treatment	0.184***	0.047*	-0.153***	-0.044***	-0.034**
	(0.020)	(0.025)	(0.028)	(0.016)	(0.015)
Observations	2,467				
Difference in treatment effects	0.062*	-0.002	-0.028	-0.007	-0.025
Control mean	0.145	0.30	0.412	0.087	0.056

Notes: Results from multinomial logit models. The table reports the average marginal effects of being assigned to the treatment group. Treatment: Information that, according to studies, investments in early education have larger benefits for the future prosperity of society than investments in later education. Control: No information. Dependent variable is the answer to the question: Which education level, given an increase in public education spending, should benefit from the increase in spending? Controls include age, gender, migration status, parental education, municipality size, living with a partner, education, income, region of residence, having children below the age of 25, employment status, and working in the education sector. High certainty is a dummy equal to 1 if respondents indicated a value of 5 or above on a seven-point Likert scale indicating how sure they were of their previous belief. Low certainty of estimate is a dummy equal to 1 if respondents indicated a value of 4 or below. Regressions are weighted by survey weights. Standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2015.

Table 3.4: Effects for subsample of parents

	Additional spending for				
	Early education	Elementary	Secondary	Vocational	University
(I) Without controls					
Household income	0.004	0.019	-0.032*	0.011	-0.002
	(0.007)	(0.014)	(0.017)	(0.015)	(0.013)
Observations			775		
(II) Without controls					
Treatment	0.188***	0.059**	-0.195***	-0.048***	-0.003
	(0.021)	(0.027)	(0.029)	(0.018)	(0.017)
Observations			2,288		
(III) Without controls					
Category has greatest subjective benefits	0.347***	0.541***	0.518***	0.443***	0.341**
	(0.053)	(0.049)	(0.048)	(0.096)	(0.172)
Observations			780		
Control mean	0.144	0.261	0.462	0.087	0.045

Notes: Sample restricted to parents with children below the age of 25. Results from multinomial logit models. The table reports results on three separate regressions. Dependent variable is the answer to the question: Which education level, given an increase in public education spending, should benefit from the increase in spending? The first panel reports the average marginal effects of an increase in monthly household income by 1,000 Euro. The second panel shows the average marginal effects of being assigned to the treatment group. Treatment: Information that, according to studies, investments in early education have larger benefits for the future prosperity of society than investments in later education. Control: No information. The third panel reports the average marginal effects of beliefs that spending has the largest benefits for education level *j* on preferences for spending on education level *j*. Control mean: share of respondents choosing each category in the control group. Regressions are weighted by survey weights. Standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2015.

Table 3.5: Role of self-interest for subsample of parents

Panel I: Spending preferences

	Additional spending for				
	Early education	Elementary	Secondary	Vocational	University
Child attends education category	0.031	0.098**	0.183***	0.001	-0.000
	(0.038)	(0.044)	(0.050)	(0.041)	(0.033)
Observations			803		
Control mean	0.144	0.261	0.462	0.087	0.045

Panel II: Beliefs on benefits of additional spending

	Greatest benefits for spending on				
	Early education	Elementary	Secondary	Vocational	University
Child attends education category	0.134***	0.118***	0.103***	0.036	0.009
	(0.034)	(0.026)	(0.027)	(0.022)	(0.021)
Observations			2,269		
Control mean	0.258	0.248	0.373	0.071	0.050

Notes: Sample restricted to parents with children still in education. Results from multinomial logit models. The table reports results on two separate regressions. First panel: average marginal effects of having a child attending education level *j* on the preferences for spending on education level *j*. Dependent variable is the answer to the question: Which education level, given an increase in public education spending, should benefit from the increase in spending? Second panel: average marginal effects of having a child attending education level *j* on the belief that spending on education level *j* has the highest benefit. Dependent variable is the answer to the question: For what education level would additional spending have the largest positive effects on the future prosperity of society? Control mean: share of respondents choosing each category. Regressions are weighted by survey weights. Standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2015.

Table 3.6: Persistence of treatment effects

Panel I: Spending preferences at first session

	Additional spending for			
	Early education	Elementary	Secondary	University
Treatment effect	0.222**	0.031	-0.067	-0.185**
	(0.107)	(0.107)	(0.094)	(0.078)
Observations	75			
Control mean	0.220	0.293	0.244	0.244

Panel II: Beliefs on benefits of additional spending two weeks later

	Greatest benefits for spending on			
	Early education	Elementary	Secondary	University
Treatment effect	0.290***	0.001	-0.155	-0.136*
	(0.099)	(0.106)	(0.105)	(0.074)
Observations	75			
Control mean	0.122	0.293	0.390	0.195

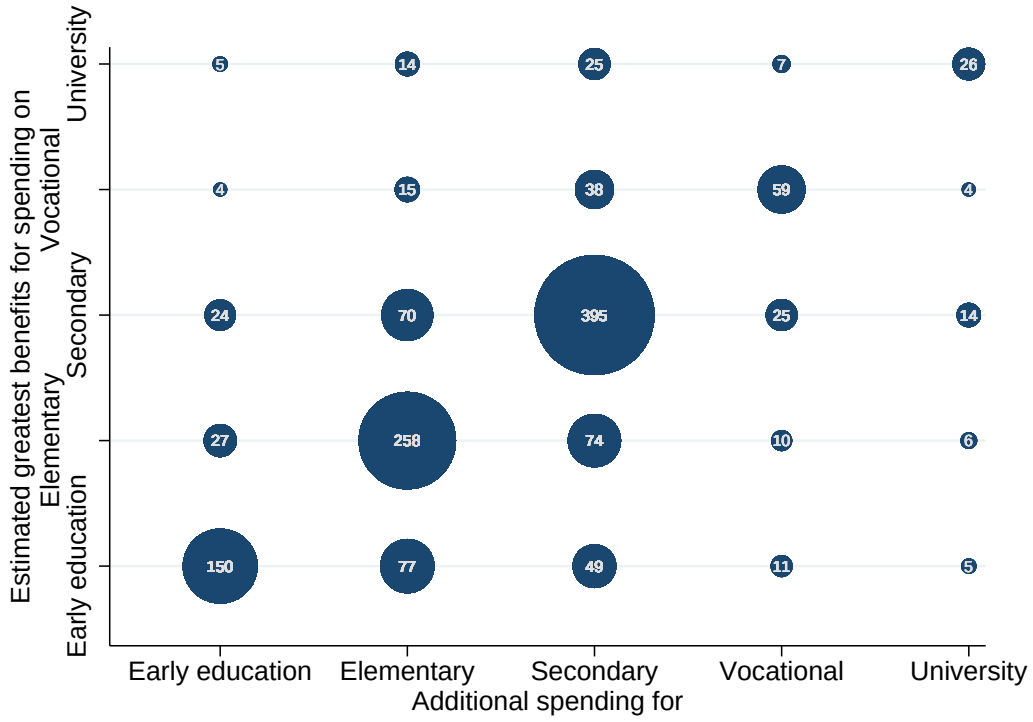
Panel III: Spending preferences two weeks later

	Additional spending for			
	Early education	Elementary	Secondary	University
Treatment effect	0.212**	0.109	-0.184*	-0.136*
	(0.102)	(0.106)	(0.103)	(0.074)
Observations	75			
Control mean	0.171	0.244	0.390	0.195

Notes: Sample of university students. Results from multinomial logit models. The category vocational training is omitted because of zero observations. The table reports the average marginal effects of being assigned to the treatment group. Treatment: Information that, according to studies, investments in early education have larger benefits for the future prosperity of society than investments in later education. Control: No information. First and third panel: Dependent variable is the answer to the question: Which education level, given an increase in public education spending, should benefit from the increase in spending? Second panel: Dependent variable is the answer to the question: For what education level would additional spending have the largest positive effects on the future prosperity of society? Control mean: share of respondents choosing each category in the control group. Standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

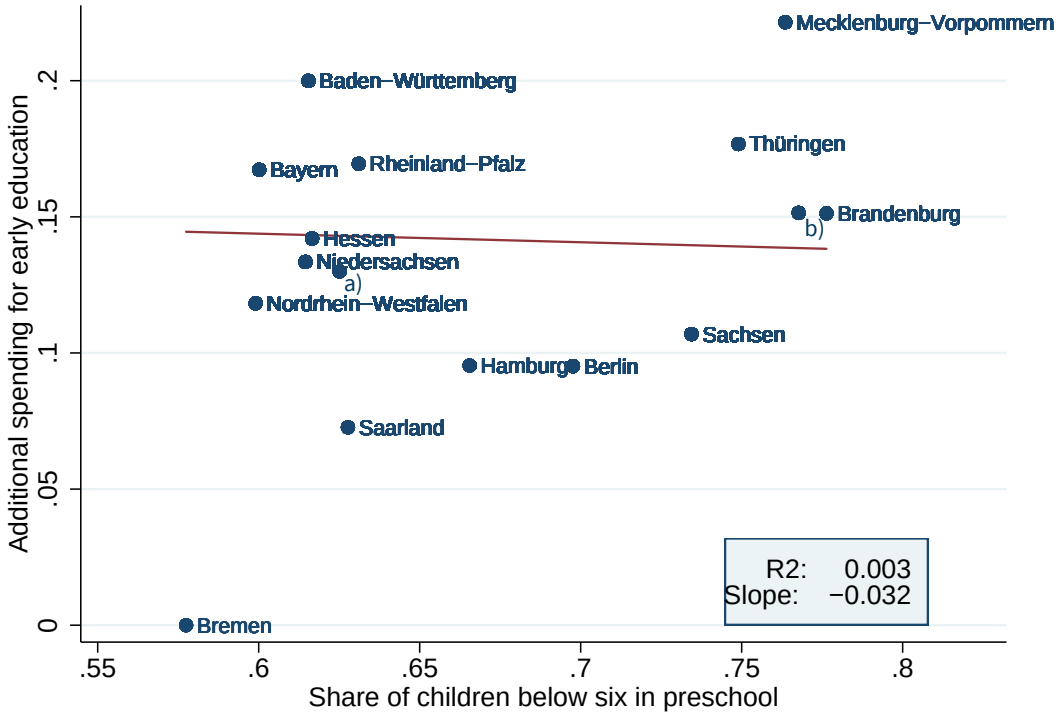
Appendix

Figure A3.1: Joint distribution of beliefs on highest benefit and spending preferences



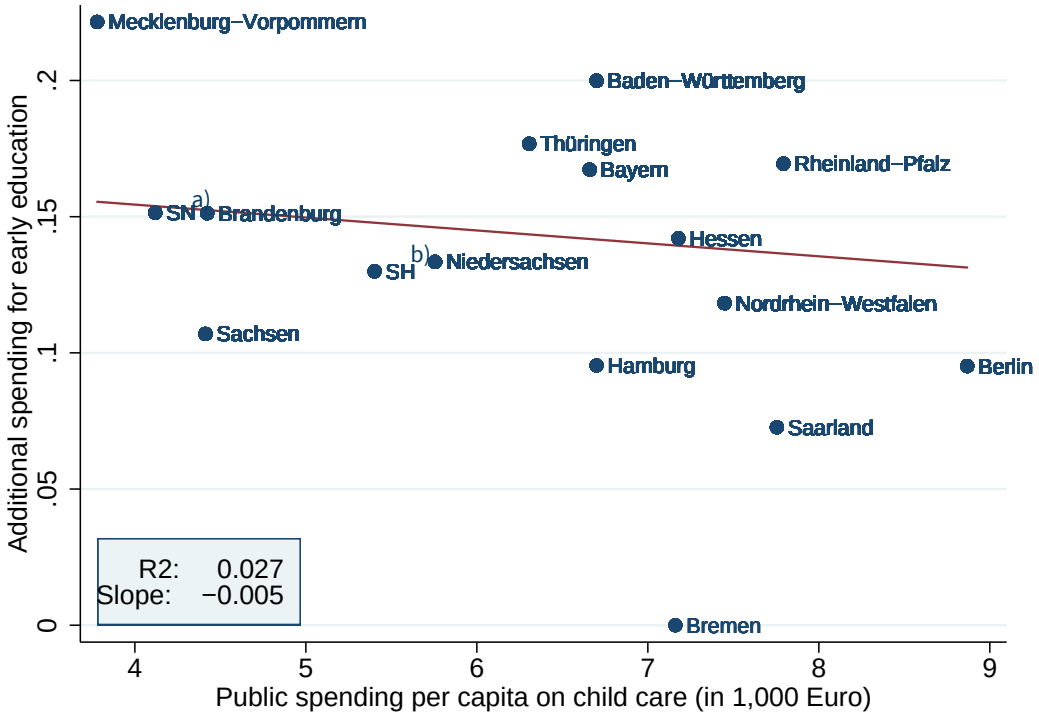
Notes: Variable on the horizontal axis is the answer to the question: Which education level, given an increase in public education spending, should benefit from the increase in spending? Variable on the vertical axis is the answer to the question: For what education level would additional spending have the largest positive effects on the future prosperity of society? Data source: ifo Education Survey 2015.

Figure A3.2: Correlation of preference for early education spending and attendance



Notes: Data on the shares of children in early education are aggregated to state level for the purpose of this graph. [a)]Label for Schleswig-Holstein and [b)] Sachsen-Anhalt omitted for expositional reasons. R-squared and slope are based on a simple OLS regression. Data source: ifo Education Survey 2015 and Statistisches Bundesamt 2014/2015.

Figure A3.3: Correlation of preference for early education spending and expenditures



Notes: Label for [a] Sachsen-Anhalt (SN) and [b] Schleswig-Holstein (SH) shortened for expositional reasons. R-squared and slope are based on a simple OLS regression. Data source: ifo Education Survey 2015 and Textor (2015).

Table A3.1: Summary statistics and balancing

	Mean	Std. deviation	Treatment status	p-value
	(1)	(2)	(3)	(4)
Age	50.798	17.903	0.000	0.748
Female	0.515		-0.023	0.223
Born in Germany	0.948		0.059	0.218
Parent holds university degree	0.277		0.040*	0.059
City size ≥ 100,000	0.314		0.014	0.497
Partner in household	0.612		0.013	0.533
Highest school degree				
No degree/basic degree	0.400		0.014	0.489
Middle school degree or equivalent	0.301		-0.041**	0.041
University entrance qualification	0.299		0.025	0.229
Lives in former West Germany	0.798		-0.007	0.746
Household income	2.278	1.402	-0.003	0.708
Parent	0.358		-0.011	0.555
Employment status				
Student	0.054		0.047	0.398
Employed	0.531		-0.022	0.251
Non-employed	0.415		0.013	0.509
Job in education sector	0.106		0.064**	0.032
Observations	4,206		4,203	

Notes: First column: sample means. Second column: standard deviation in brackets (for non-dummy variables). Third column: each row reports the coefficients from regressions of equation (4) for the survey experiment. Fourth column: p-values from coefficients in column (3). Regressions weighted by survey weights. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2015.

Table A3.2: Overview of question wordings

No.	Group	Wording	Type of question
15a	All	What is your best guess, in which one of the following areas would additional public spending have the most beneficial effect on the future prosperity of society?	5 answer categories: “Early education”; “Elementary schools”; “Secondary schools”; “Vocational schools”; “Universities and universities of applied sciences”
15b	All	How sure are you that your answer is close to correct?	7-point scale from “very unsure” to “very sure”
32	Control	Numerous studies show that education is important for the future prosperity of society. Suppose the government plans an increase in education spending. If only one level of education can benefit from this increase, which area should it be in your opinion?	5 answer categories: “Early education”; “Elementary schools”; “Secondary schools”; “Vocational schools”; “Universities and universities of applied sciences”
32	Treatment	Numerous studies show that spending for early childhood education has a more beneficial effect on the future prosperity of society than spending in later areas of education. Suppose the government plans an increase in education spending. If only one level of education can benefit from this increase, which area should it be in your opinion?	5 answer categories: “Early education”; “Elementary schools”; “Secondary schools”; “Vocational schools”; “Universities and universities of applied sciences”

Table A3.3: Additional spending other for than area with greatest benefit

	Characteristics of respondents	
	(1)	(2)
Age	-0.003*	-0.002
	(0.002)	(0.002)
Female	0.036	0.025
	(0.037)	(0.037)
Born in Germany	0.042	0.035
	(0.083)	(0.081)
Parent holds university degree	0.098**	0.093**
	(0.042)	(0.041)
City size ≥ 100,000	0.010	0.006
	(0.039)	(0.038)
Partner in household	0.066	0.064
	(0.042)	(0.042)
Education (baseline: no degree)		
Middle school degree or equivalent	-0.115***	-0.113**
	(0.044)	(0.044)
University entrance qualification	-0.190***	-0.178***
	(0.050)	(0.050)
Lives in former West Germany	-0.071	-0.071*
	(0.043)	(0.043)
Household income	0.000	0.002
	(0.015)	(0.015)
Parent	-0.071*	-0.071*
	(0.040)	(0.039)
Employment (baseline: employed)		
Student	0.065	0.048
	(0.131)	(0.128)
Non-employed	0.024	0.016
	(0.041)	(0.040)
Job in education sector	0.098*	0.110*
	(0.060)	(0.060)
Certainty (baseline: unsure)		
Undecided		0.007
		(0.047)
Sure		-0.108**
		(0.043)
Constant	0.554	0.601
Observations	1,329	1,329
R-squared	0.0376	0.0499

Notes: OLS regressions. Control group only. Dependent variable is dummy equal to 1 if respondent estimate that benefits are highest to spending for education area *j* but prefers additional spending for education level *i*. Regressions weighted by survey weights. Standard errors reported in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2015.

Table A3.4: Differences between full sample and parents

Panel I: Spending preferences

	Additional spending for				
	Early education	Elementary	Secondary	Vocational	University
Parent	-0.051**	-0.023	0.104***	-0.011	-0.019
	(0.024)	(0.031)	(0.033)	(0.023)	(0.021)
Observations	1,413				
Control mean	0.144	0.299	0.413	0.09	0.054

Panel II: Beliefs on benefits of additional spending

	Greatest benefits for spending on				
	Early education	Elementary	Secondary	Vocational	University
Parent	0.008	-0.010	0.028	0.001	-0.026**
	(0.018)	(0.017)	(0.019)	(0.012)	(0.011)
Observations	4,126				
Control mean	0.246	0.259	0.357	0.077	0.061

Notes: Results from multinomial logit models. The table reports the average marginal effects of a dummy equal to 1 if the respondent has children below the age of 25. First panel: Dependent variable is the answer to the question: Which education level, given an increase in public education spending, should benefit from the increase in spending? Second panel: Dependent variable is the answer to the question: For what education level would additional spending have the largest positive effects on the future prosperity of society? Control mean: share of respondents choosing each category. Regressions are weighted by survey weights. Standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2015.

Table A3.5: Treatment effect heterogeneities for parents

Panel I: Full sample

	Additional spending for				
	Early education	Elementary	Secondary	Vocational	University
Treatment effect	0.160***	0.046**	-0.142***	-0.041***	-0.022**
	(0.016)	(0.020)	(0.021)	(0.012)	(0.011)
Observations	4,223				

Panel II: Results by parental status

	Additional spending for				
	Early education	Elementary	Secondary	Vocational	University
(I) Parents					
Treatment effect	0.188***	0.059**	-0.195***	-0.048***	-0.003
	(0.021)	(0.027)	(0.029)	(0.018)	(0.017)
Observations	2,288				
(II) Nonparents					
Treatment effect	0.137***	0.045	-0.108***	-0.042**	-0.032**
	(0.023)	(0.028)	(0.029)	(0.017)	(0.014)
Observations	1,821				
Difference in treatment effects	-0.051*	-0.014	0.087**	0.007	-0.028
Control mean	0.164	0.312	0.368	0.095	0.060

Notes: Results from multinomial logit models. The table reports the average marginal effects of being assigned to the treatment group. Treatment: Information that, according to studies, investments in early education have larger benefits for the future prosperity of society than investments in later outcomes. Control: No information. Dependent variable is the answer to the question: Which education level, given an increase in public education spending, should benefit from the increase in spending? Parents is a dummy equal to 1 if respondents have a child below the age of 25. Nonparents is a dummy equal to 1 if respondent does not have children or all children are older than 25 years. Control mean: share of respondents choosing each category. Regressions are weighted by survey weights. Standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2015.

Table A3.6: Interviewer demand effects

	Additional spending for early education	
	(1)	(2)
Treatment	0.165***	0.164***
	(0.017)	(0.017)
Offline interview	0.090**	0.045
	(0.037)	(0.042)
Treatment × Offline interview	-0.024	-0.039
	(0.049)	(0.051)
Female		0.033*
		(0.017)
Age group (baseline: 18 to 45)		
45 to 64		0.050***
		(0.018)
65+		0.084***
		(0.030)
Born in Germany		0.016
		(0.039)
Parent holds university degree		0.010
		(0.019)
Number of books at home		0.035*
		(0.020)
Constant	0.130	0.056
Observations	4,223	4,177
R-squared	0.0341	0.0394

Notes: OLS regressions. Coefficient estimates show effect of treatment and survey mode on the probability to favor spending for early education. Treatment: Information that, according to studies, investments in early education have larger benefits for the future prosperity of society than investments in later education. Control: No information. Offline interview is equal to 1 if respondents were interviewed as part of a personal interview, 0 if they responded online. Dependent variable is dummy equal to 1 if respondent favors additional spending for early education. Regressions weighted by survey weights. Standard errors reported in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2015.

Table A3.7: Differences between main sample and convenience sample

Panel I: Differences in control group

	Additional spending for				
	Early education	Elementary	Secondary	Vocational	University
Sampled in convenience sample	0.010	0.013	-0.167***	-0.051**	0.196***
	(0.046)	(0.058)	(0.054)	(0.022)	(0.052)
Observations	1,467				

Panel II: Differences in treatment effects

	Additional spending for				
	Early education	Elementary	Secondary	Vocational	University
(I) Main sample					
Treatment effect	0.157***	0.048***	-0.153***	-0.041***	-0.011*
	(0.013)	(0.015)	(0.016)	(0.008)	(0.006)
Observations	4,218				
Control mean	0.152	0.311	0.417	0.080	0.039
(II) Convenience sample					
Treatment effect	0.120*	0.049	-0.041	0.007	-0.135**
	(0.062)	(0.073)	(0.065)	(0.027)	(0.059)
Observations	178				
Difference in treatment effects	-0.037	0.001	0.112*	0.048*	-0.124**
Control mean	0.162	0.324	0.250	0.029	0.235

Notes: Results from multinomial logit models. The table reports results from three separate regressions. The first panel reports the average marginal effects of a dummy equal to 1 if the respondent was sampled as part of the follow-up sample of university students. The second and third panel show the average marginal effects of being assigned to the treatment group. Treatment: Information that, according to studies, investments in early education have larger benefits for the future prosperity of society than investments in later education. Control: No information. Dependent variable is the answer to the question: Which education level, given an increase in public education spending, should benefit from the increase in spending? Control mean: share of respondents choosing each category in the control group. Standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2015.

Table A3.8: Treatment effect heterogeneities by students' social desirability score

Panel I: Full sample

	Additional spending for				
	Early education	Elementary	Secondary	Vocational	University
Treatment effect	0.120*	0.049	-0.041	0.007	-0.135**
	(0.062)	(0.073)	(0.065)	(0.027)	(0.059)
Observations	178				
Control mean	0.162	0.324	0.250	0.029	0.235

Panel II: Results by social desirability score

	Additional spending for				
	Early education	Elementary	Secondary	Vocational	University
(I) High social desirability					
Treatment effect	0.050	-0.054	-0.058	0.028	0.034
	(0.108)	(0.123)	(0.113)	(0.027)	(0.099)
Observations	61				
Control mean	0.200	0.360	0.280	0.000	0.160
(II) Low social desirability					
Treatment effect	0.158**	0.103	-0.030	-0.006	-0.225***
	(0.075)	(0.090)	(0.080)	(0.039)	(0.073)
Observations	117				
Difference in treatment effects	0.078	0.125	-0.000	-0.020	-0.182*
Control mean	0.140	0.302	0.233	0.047	0.279

Notes: Sample of university students. Results from multinomial logit models. The table reports the average marginal effects of being assigned to the treatment group. Treatment: Information that, according to studies, investments in early education have larger benefits for the future prosperity of society than investments in later education. Control: No information. Dependent variable is the answer to the question: Which education level, given an increase in public education spending, should benefit from the increase in spending? High social desirability is a dummy equal to 1 if respondents scored 18 or higher on one of two social desirability scales. Low social desirability is a dummy equal to 1 if respondents scored below 18 on both scales. Control mean: share of respondents choosing each category. Regressions are weighted by survey weights. Standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

4 Educational Inequality and Public Policy Preferences [61]

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, 2016a). 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 on educational achievement have taken center stage in the political debate.

This chapter 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. Redistributive policies, such as progressive taxation or minimum wages, are designed to equalize *outcomes*, but 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 aim at detaching the opportunity to turn effort into economic success from individual circumstances such as family background.[62] 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

[61] This chapter is joint work with Philipp Lergetporer and Ludger Woessmann, both at the ifo Institute in Munich. For helpful comments, we would like to thank Peter Bergman, Elisabeth Bublitz, Jonathan Davis, Emmanuel Saez, Stefanie Stantcheva, and seminar participants at Harvard, the CESifo education meeting in Munich, the European Society for Population Economics in Glasgow, the German Economic Association in Vienna, and its economics of education group in Hannover. We are also most grateful to Franziska Kugler and Elisabeth Grewenig for their help in preparing the surveys. Financial support by the Leibniz Competition (SAW-2014-ifo-2) and the German Science Foundation (CRC TRR 190) is gratefully acknowledged.

[62] 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 (see 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).

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.

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 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 percentage points to 68 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. Re-surveying respondents in a follow-up survey, we find that the information effect on respondents' beliefs and concerns about educational inequality persists about two weeks after the experiment, 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 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 percentage points (from a baseline support of 63 percent). While reaching statistical significance for the policy index in particular when exploiting the full range of preferences for the policy variable (from strong opposition to strong favoring), 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 6 percentage points (baseline 65 percent). Interestingly, making preschool compulsory is the one policy option that demands effort from the disadvantaged group—i.e., to attend preschool—rather than just offering additional financial 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 effects 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 on a broader scale, 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 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), information about recent scientific findings that preschool decreases 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 and 5 percentage points, respectively. Importantly, providing both pieces of information increases support by 13 percentage points, roughly the sum of the separate effects. The additivity of treatment effects implies that the information on the effectiveness of the policy, while affecting 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 seems 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 have greater trust in the education system than the general population. In a complementary dataset, we show that these subgroups are in fact 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 and in 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 to 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 increase. The only exception is that being informed about educational inequality raises support for introducing compulsory preschool, a policy initiative that would commit parents from disadvantaged backgrounds to send their children to preschool. The fact that no such effects are found for policy initiatives in different areas that would simply increase funding for disadvantaged groups might suggest that respondents do not favor unconditional financial support that is not tied to additional ef-

fort from the disadvantaged groups. This explanation is also consistent with the finding that treatment effects on preferences for compulsory preschool are restricted to those who do *not* have a general preference for increased government spending on schooling.

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 other factors, historical experience, culture, prospects of upward mobility, and individuals' socioeconomic background have been identified to shape redistributive preferences (e.g., Alesina and La Ferrara, 2005; Alesina and Guiliano, 2011; Luttmer and Singhal, 2011). 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.[63] 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 study 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, 2008b; 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.

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

The remainder of the chapter is structured as follows. Section 4.1 introduces the opinion survey and the experimental design. Section 4.2 presents and discusses the results. Section 4.3 concludes.

4.1 Data and Empirical Strategy

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

4.1.1 The Opinion Survey

The research in this chapter is based on two waves of the ifo Education Survey, that were fielded 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 again representative for the German voting-age population.[64] 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). 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 4.1.4).

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.

Columns 1 and 4 of Table 4.1 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.[65]

[64] We again conducted a mixed mode survey, where the part of the population that uses the internet is sampled from an online panel while individuals who report not to use the internet (13 percent in 2016; 9 percent in 2017) are polled at their homes by trained interviewers. We again employ survey weights throughout this chapter 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 presented in this chapter.

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

4.1.2 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 education policies aimed at reducing educational inequality.

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.[66] Throughout the survey, respondents in the control 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 A4.1). 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 information text remained visible to the treatment groups 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-

[66] 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, 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 (see chapter 5). 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 The 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 4.2, we provide evidence that re-spondents indeed process and remember the information as intended.

year equivalents.[67] We also asked how confident respondents were about the accuracy of their beliefs (from "1 very unsure" to "7 very sure"). These measures of respondents' prior beliefs allow us to investigate the channels through which the information treatment operates.

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).

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 are aimed at increasing equality of opportunities by reducing the influence of family background on student achievement.[68]

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 chil-

[67] 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.)"

[68] 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 inequality 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).

dren from low-income families and (ii) introducing compulsory preschool.[69] 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,[70] (v) providing bonuses for teachers who teach in schools with many disadvantaged students, (vi) introducing whole-day schooling until 4 pm for all students,[71] 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.[72]

While the evidence base for the equality implications of these different policies varies, all of them have been proposed as political responses to educational inequality. 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.

Additional Experiments We test the hypothesis that increased concerns about educational inequality lead to higher support for equity-oriented education policies. However, earlier evidence suggests that treatment effects on concerns might not be sufficient for shifting policy preferences for several reasons. 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., Bartels, 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 in-

[69] As discussed in chapter 3, 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 (2014) provide recent evidence for the equity-enhancing effects of universal childcare in Germany.

[70] 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.

[71] 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).

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

crease 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.[73]

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 doing so. In particular, such doubts might exist if the mechanisms through which education policies affect inequality of educational opportunity are 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 simultaneously. Comparing preferences for compulsory preschool across treatments reveals the complementarity of information on educational inequality and on policy effectiveness in shaping public policy preferences.[74]

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

[74] 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.

4.1.3 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:

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

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.

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

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

where $Subgroup_i$ equals 1 if respondent *i* is member of the respective subgroup and 0 otherwise. In this specification, the effect of information provision for the baseline group is given by β_1; β_3 measures the additional effect for the respective subgroup.

4.1.4 Test of Randomization

We test whether covariates differ across control and treatment groups to investigate if randomization successfully balanced respondents' observable characteristics. Columns 2 and 3 of Table 4.1 report differences between the control group and the treatment groups in the 2016 survey, and columns 5 to 7 in 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 8 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 4.1, 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.[75]

4.2 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.

4.2.1 Information Provision and Concerns about Educational Inequality

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

Experimental Results Table 4.2 reports our estimates of 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.[76] Odd-numbered columns show the unconditional regressions, even-numbered columns include a set of covariates.[77] For comparison, the reported control mean refers to the mean of the outcome variable in the uninformed control group.

As it turns out, the majority of respondents in the control group—55 percent—perceives unequal educational opportunity for children from different social backgrounds as a serious or very serious problem. Only 14 percent think it is no problem or a small problem

[75] For ease of exposition, Table 4.1 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 4.1.2 for details). Covariates are also balanced in the concern experiment: only 6 out of 60 pairwise comparisons between the control group and information treatment group are significant at the 5 percent level (results available upon request).

[76] 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).

[77] 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.

(with the remaining category of the five-point scale referring to a medium problem).[78] 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 A4.1).[79] Partisans of the conservative party (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 4.2 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 A4.2). The insignificant coefficient on the interaction term between the information treatment indicator and a dummy for the 2017 survey wave in Appendix Table A4.3 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

[78] 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.

[79] Appendix Table A4.1 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.

of the information treatment effect does not differ significantly between respondents with left-leaning and right-leaning political preferences.[80] 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.[81]

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.

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 A4.2). The vast majority of respondents (84 percent) underestimate the extent of 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

[80] 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?" Right-leaning political preferences are measured as indicating a preference for CDU/CSU, FDP or AFD.

[81] In fact, the treatment effect on viewing educational inequality as a serious problem is marginally significantly larger (by 5 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.

that the average respondent was informed by the treatment that educational inequality is more pronounced than she had previously believed.

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 4.3 and Figure 4.1 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 4.3). The positive slope of both lines in Figure 4.1 reflects the intuitive result 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 it is 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 A4.3 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 4.3).[82] 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.[83]

[82] 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).

[83] 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).

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 A4.4 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 Appendix Table A4.5). Thus, non-random selection into follow-up survey participation does not bias our estimates of treatment effect persistence.

Table 4.4 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 6 percentage points rather than 12 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 that are unlikely to persist over two weeks.

4.2.2 Information Provision and Public Policy Preferences

Next, we investigate whether the information provision, which increased concerns about educational inequality, also has a causal impact on public preferences for education poli-

cies 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.

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 4.5). 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 (Alesina et al., 2018).

The preferences for equity-oriented education policies are closely associated with concerns about educational inequality. Table 4.5 shows regressions of policy preferences on concerns in the control group. The dependent variables in columns 2 to 9 are dummies coded 1 if the respondent (strongly) favors the respective policy, and 0 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 be due to the fact that increases in teacher salary are generally unpopular with the German public (as discussed in chapter 2), but also due to a dislike of bonus policies among respondents who are concerned about inequality.

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 policy preferences. Table 4.6 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 percentage points (from a baseline support of 63 percent, see 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 percentage points (baseline 64 percent, see column 3). While estimates for all other policies are also positive, none reaches statistical significance, and most are very small.

We can exploit variation beyond the shares that support the respective policies by measuring policy preferences on a continuous five-point scale. As shown in Table 4.7, 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. 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.

We re-elicited policy preferences for compulsory preschool (but not for the other policies) in the 2017 wave. The first four columns of Table 4.8 indicate that the significant effect of informing about educational inequality on support for compulsory preschool found in the 2016 wave is replicated in the 2017 wave. The effect is slightly larger in the replication (which might reflect that the question was presented on its own in the 2017 survey, whereas it was part of a list of policy proposals in the 2016 survey), but the difference between survey waves does not reach statistical significance. Thus, in the pooled sample, the information treatment increases support for compulsory preschool by a highly significant 6 percentage points (column 4).

One feature that distinguishes the introduction of compulsory preschool from the other policy proposals is that it requires commitment from the disadvantaged families, namely

requiring all of them to send their children to preschool. As can be seen from Tables 4.6 and 4.7, there are basically no treatment effects 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 learning disability, does in fact target a different dimension of inequality of opportunity (disability) than the one addressed by the information treatment (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),[84] 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, while 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), information treatment effects do not differ significantly by political preferences.[85]

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

4.2.3 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. Sec-

[84] 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.

[85] 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.

ond, 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' trust in educational institutions or in the government.[86]

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 disconnect hypothesis 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 4.6 and 4.7 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.

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 implement anoth-

[86] 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 α=0.05 and power=0.80).

er 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 4.8 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 percentage points.[87] The estimates of the two experimental treatments do not differ significantly from one another.

The combination of 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 13 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 A4.6 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 received the combined information.[88]

Overall, we find that while respondents' doubts about whether education policies effectively mitigate educational inequality might be an important determinant of policy prefer-

[87] 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 the information treatment discussed in chapter 3).

[88] See Appendix Table A4.4 (column 2) and Appendix Table A4.5 for evidence that non-random selection into the follow-up survey does not drive this result.

ences, they do not seem to be a reason for the limited information treatment effects on policy preferences in the previous section.

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.[89] While we do not have 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 validity of the assumption that these subgroups are more trusting that public schools can alleviate educational inequality.

Table 4.9 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 I and II, 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

[89] As discussed above, the government in Germany is heavily engaged in the education sector (see chapter 2 for details).

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 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.[90]

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 III of Table 4.9 presents estimates of heterogeneous treatment effects for respondents who do and do not support increases in public school spending.[91] 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 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.

4.3 Conclusions

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*.

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

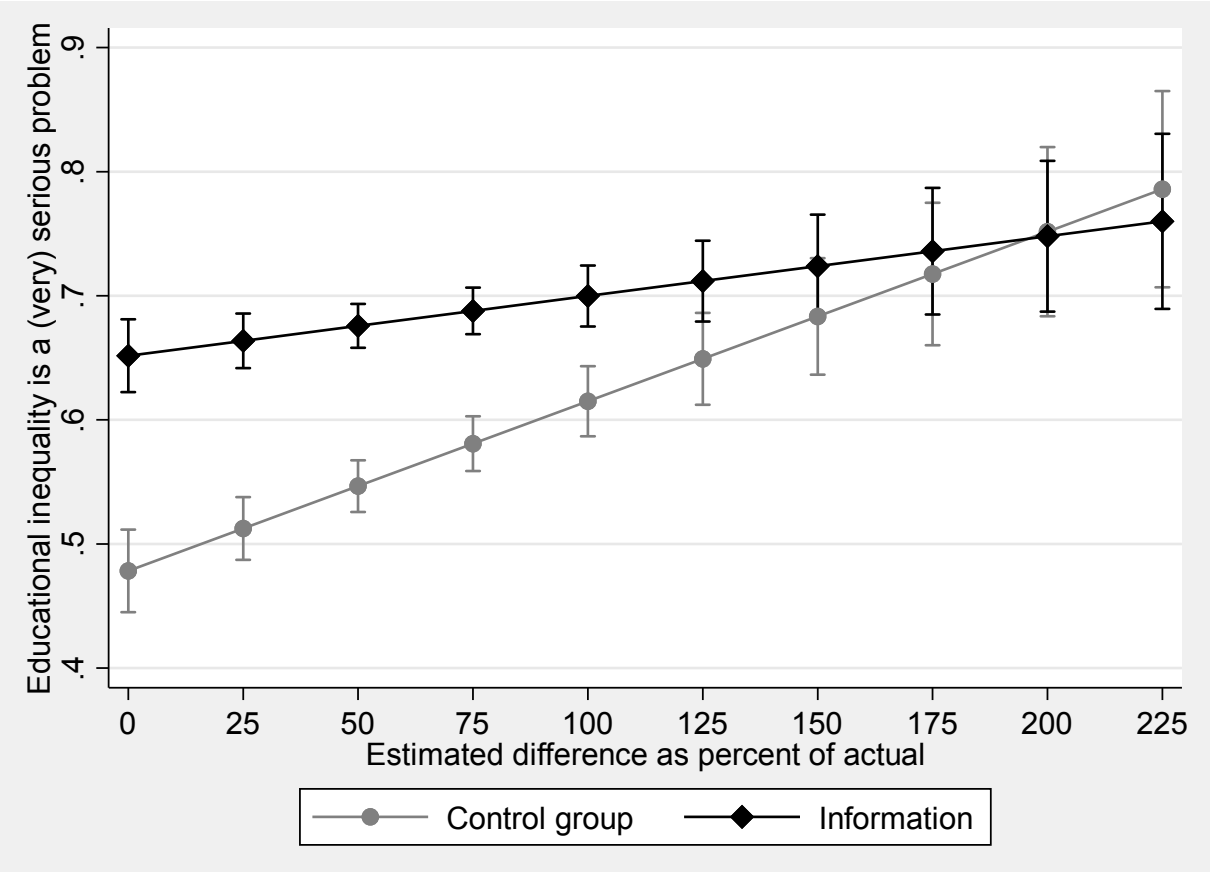
[91] Preferences for increases in public school spending were elicited early in the survey, prior to the experiment on educational inequality. The question was worded similar to the question on school spending discussed in chapter 2. Consistent with our earlier findings, 69 percent respond that spending should greatly increase or increase.

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 most of the effects are quantitatively small. The one exception is a substantial treatment effect on support for compulsory preschool, a policy that requires disadvantaged families to contribute by sending their children to preschool. We show that respondents' doubts about 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 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 equality of opportunities. 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. 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.

Figure 4.1: 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 4.3. 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 4.1: 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 4.1 (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 pref.[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 4.2: 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		0.138	
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 4.3: 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.

Table 4.4: 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 from "1 very unsure" to "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 4.5: 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 stu-dents	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***	0.141***	0.133***	0.176***	0.163***	0.032	0.099***	0.095***	0.137***
	(0.021)	(0.033)	(0.036)	(0.032)	(0.035)	(0.036)	(0.036)	(0.037)	(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 4.6: 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 stu-dents	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 4.7: 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 stu-dents	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" to 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 4.8: Effects of information and effectiveness treatments on preferences for compulsory preschool

	Survey 2016 and 2017				Survey 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 treatment group "information" 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 4.9: Heterogeneity of information treatment effects across subgroups

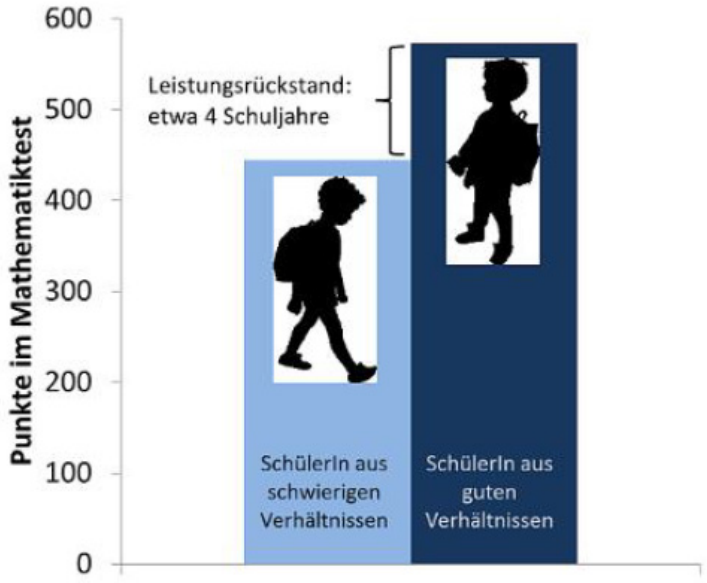
Support for education policies:		Preschool		School					University
	Policy index	Free pre-school for low-income children	Com-pulsory preschool	Spending for disad-vantaged schools	Later tracking	Bonuses for teachers at disadvan-taged schools	Whole-day schooling for all students	Coeducation of children with/ out learning disability	Need-based scholarships
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel I: 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 II: 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 III: 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 I: teachers; Panel II: partisans of the governing parties; Panel III: 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.

Appendix

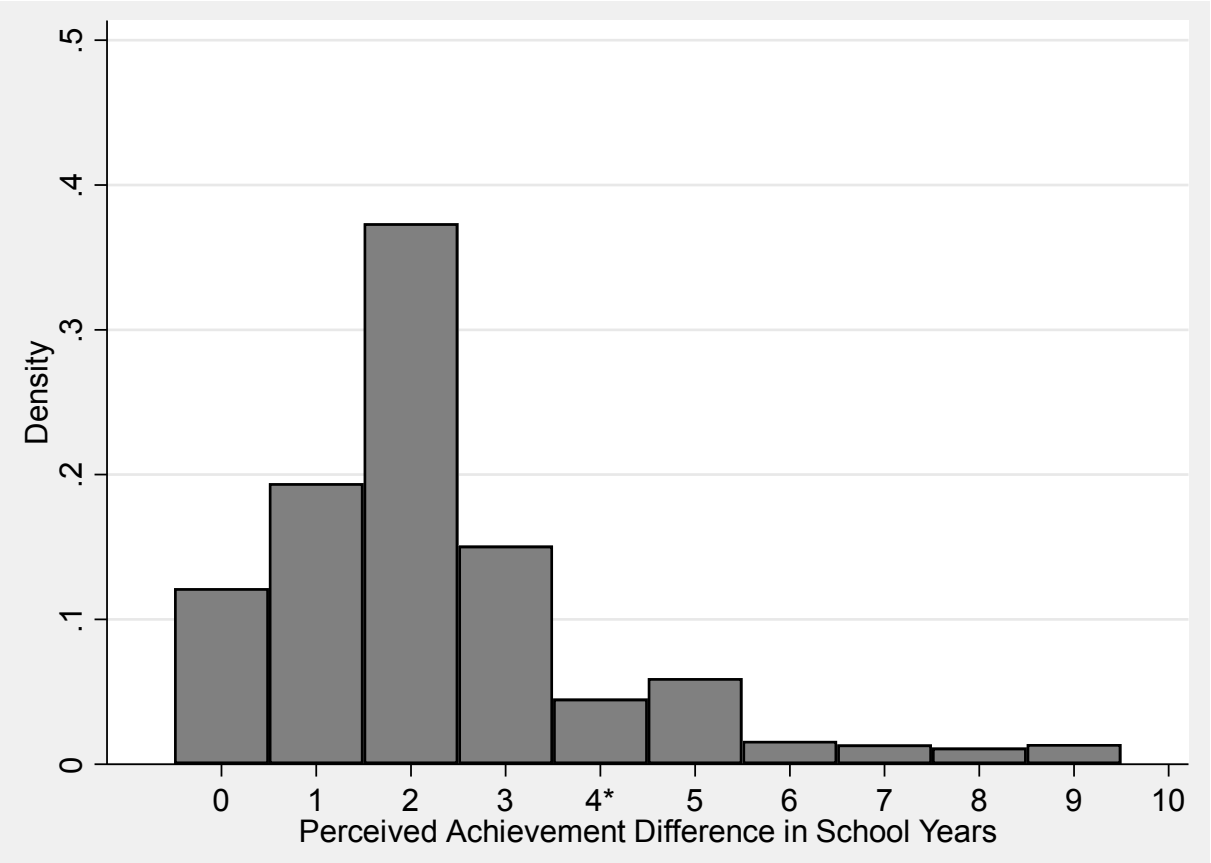
Figure A4.1: Illustration of the information treatment

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).



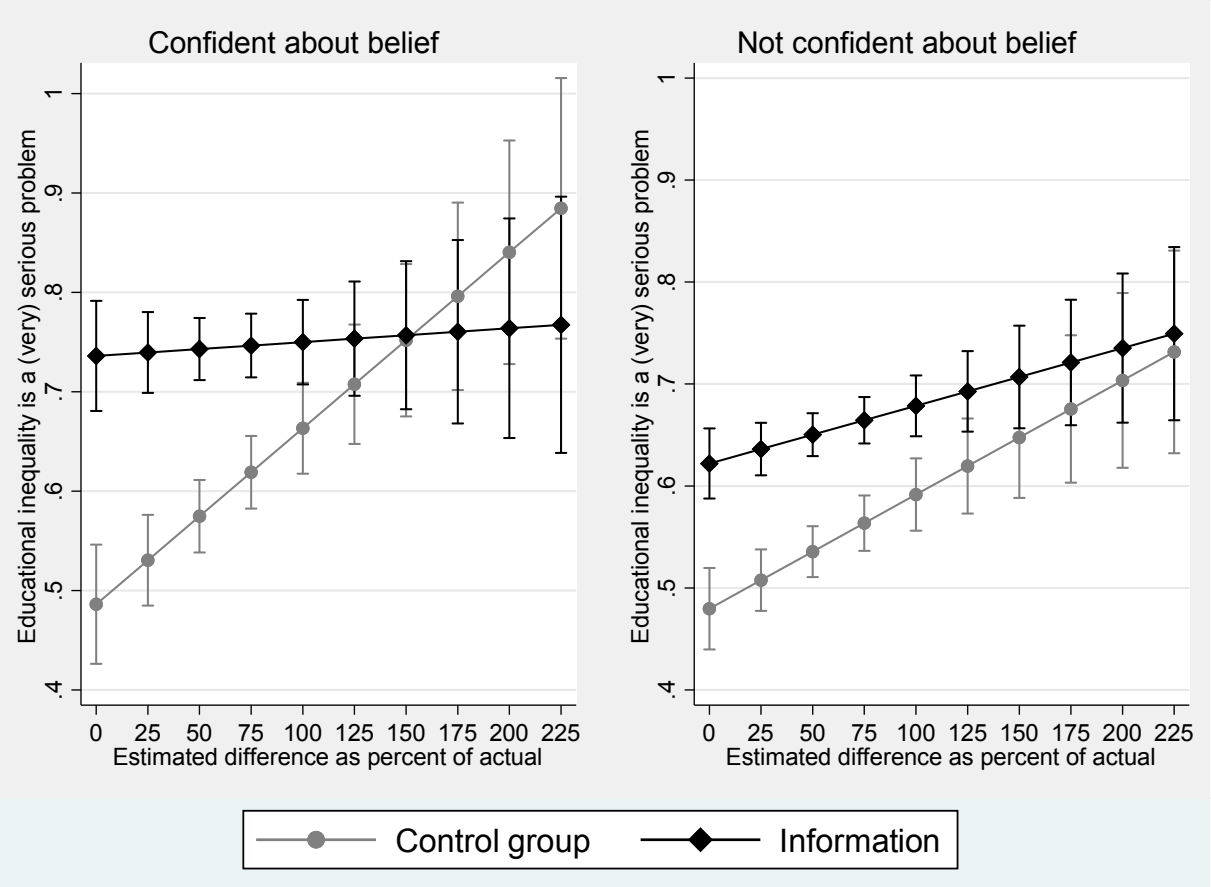
Source: ifo Education Survey 2016, 2017.

Figure A4.2: 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 (four school years). Data source: ifo Education Survey 2016, 2017.

Figure A4.3: 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 4.3. 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 A4.1: Who perceives educational inequality as a problem?

	Educational inequality is a problem			
	Five-point scale		Binary	
	(1)		(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 A4.2: 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" to 5="a very serious problem"; columns (2)-(6): dummy variable coded 1=answer category given in respective column 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 A4.3: 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		0.138	
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 A4.4: 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 A4.5: 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 ≥ 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
“Education” 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 A4.6: 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" to 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.

5 Does Ignorance of Economic Returns and Costs Explain the Educational Aspiration Gap?[92]

A key rationale for government involvement in the education sector is to provide all citizens with the opportunity to obtain the professional qualifications they have the ability and passion to pursue. As we have seen in chapter 4, however, gaps in educational attainment between individuals from different family backgrounds are substantial (Black and Devereux, 2011; Björklund and Salvanes, 2011; Holmlund et al., 2011) and contribute to the persistence of inequality across generations (Corak, 2013; Alvaredo, 2018). These gaps do not just exist in outcomes, but, as our representative survey data from Germany shows, already emerge in individuals' aspirations for higher educational degrees. One potential reason for gaps in educational aspirations is that individuals from families that do not have a background in higher education may underestimate the returns to university education and overestimate its costs. Indeed, evidence indicates that providing information about the actual returns to education and lowering application costs can increase the aspirations and attainment of specific groups of students.[93] This has substantial policy relevance: If the lack of awareness of returns and costs of education differs by socioeconomic background, information campaigns about the returns to higher education and the options to receive student aid might help reduce educational inequality. In this chapter, we study the extent to which differences in the knowledge of returns and costs of university education contribute to the socioeconomic gap in educational aspirations.

We conduct our analysis within two waves of the ifo Education Survey with more than 7,000 respondents. We first elicit respondents' beliefs about the returns and costs of university education. We then provide random treatment groups with different types of information about the returns and costs of university education before eliciting their aspirations for the ideal educational degree for their child. The survey experiments allow us to estimate how information provision affects educational aspirations in the different treat-

[92] This chapter is joint work with Philipp Lergetporer and Ludger Woessmann, both at the ifo Institute in Munich. For helpful comments, we would like to thank Dorothea Kübler, Pia Pinger, Beth Schueler, Felix Weinhardt, and seminar participants at the ifo Institute, the CRC retreat in Tutzing, and the CEPA seminar at Stanford University. We are also most grateful to Franziska Kugler and Elisabeth Grewenig for their help in preparing the surveys. Financial support by Deutsche Forschungsgemeinschaft through SFB-TRR 190 and by the Leibniz Competition (SAW-2014-ifo-2) is gratefully acknowledged.

[93] See, among others, Jensen (2010), Bettinger et al. (2012), Hoxby and Turner (2013), Oreopoulos and Dunn (2013), Delavande and Zafar (2014), Wiswall and Zafar (2015a), Pekkala Kerr et al. (2015), McGuigan et al. (2016), and Baker et al. (2017).

ment groups compared to a control group that does not receive the information. On this basis, we evaluate the extent to which information provision is able to close the educational aspiration gap, focusing on the gap between individuals with and without a university degree.

We choose to focus on educational aspirations because they are a necessary condition for, and a strong predictor of, actual future educational choices.[94] By asking which educational degree respondents consider ideal (rather than realistic) for their hypothetical child, we obtain a measure of respondents' educational aspirations that abstracts from possible institutional or child-specific factors that may constrain actual educational choices (see also Bleemer and Zafar, 2018).[95] Since such constraints might lead to aspirations not translating into actual educational outcomes, our treatment effects should be interpreted as an upper bound of the potential impact of information on gaps in actual educational outcomes.[96] Furthermore, as they can be elicited from the whole population rather than only from parents or students who face educational decisions, focusing on ideal aspirations allows us to gain a representative assessment of the nationwide educational aspiration gap.

Our results indicate that aspirations do indeed differ strongly by educational background. In the control group, 74 percent of university graduates but only 36 percent of those without university education consider a university degree (rather than an apprenticeship degree) the ideal educational outcome for their child. Intriguingly, this aspiration gap of 38 percentage points is similar to the gap in actual university enrollment decisions by family background (Middendorff et al., 2013).

We also find that individuals without university education tend to underestimate the returns and overestimate the costs of university education more than university graduates.[97]

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[94] See, for example, Jacob and Linkow, 2010, Beaman et al. (2012), Spangenberg et al. (2011), and Attanasio and Kaufmann (2014). Decomposition analyses show that the aspirations of parents account for a substantial share of educational outcomes of children of school-leaving age (e.g., Chowdry et al., 2011, Polidano et al., 2013).

[95] One critique of asking respondents to abstract from possible constraints when eliciting choice expectations is that respondents might be unable to follow this instruction (e.g. Manski, 1999). This is not the case in our dataset: In a complementary oversample of parents, we show that unconstrained educational aspirations differ meaningfully from realistic expectations (see section 5.5.3).

[96] That is, if information affects aspirations, it is not entirely clear whether these changes in aspirations translate into actual attainment. If, on the other hand, information does not affect the gap in aspirations, it is unlikely that information effects would change actual educational outcomes.

[97] Throughout, we refer to differences in earnings and unemployment rates by educational degree as "returns" to education without implying that these differences reflect a causal effect of university education on earnings or unemployment.

While respondents who hold a university degree correctly estimate the extent to which university graduates earn more, apprenticeship graduates underestimate the earnings differential. Respondents with and without a university degree underestimate the difference in unemployment rates between university- and apprenticeship graduates, but the magnitude of the underestimation is stronger among the latter group of respondents. Similarly, while university graduates tend to overestimate tuition fees and underestimate available student aid, the extent of this is again stronger among respondents without a university degree. In principle, these informational asymmetries suggest that ignorance among those without university education could contribute to the educational aspiration gap.

Our experimental results show that informing about the actual returns and costs of university education indeed significantly increases the educational aspirations of respondents. However, the information treatment effects are at least as strong among individuals with university education as among individuals without. As a consequence, information provision, if anything, increases rather than decreases the gap in educational aspirations. Specifically, informing participants about the higher earnings of people with a university degree compared to those without a university degree raises the share of respondents aspiring for a university degree by 11 percentage points among university graduates and by 5 percentage points among those without a university education. Informing participants about the magnitude of available government student aid raises educational aspirations by 8 percentage points among university graduates but does not affect aspirations of those without a university degree. Providing information on the lower unemployment rates of university graduates or on the fact that German universities currently do not charge tuition fees does not significantly affect educational aspirations.[98] These results are based on representative samples of the adult population, but we find similar results in the subgroup of parents with children who have not yet completed their education. Our findings cast doubt that ignorance of economic returns and costs of university education among persons without a university degree can explain the educational aspiration gap.

To test whether the information provision indeed raises respondents' knowledge of the costs of university education, we conducted a follow-up survey about two weeks after the experiments. Results show that information provision persistently improves belief accura-

[98] A classic explanation for the socioeconomic gap in university enrollment is that students from low-income backgrounds cannot afford university education because they are credit constrained (e.g., Lochner and Monge-Naranjo, 2012). The fact that information about student aid and tuition fees does not shrink the aspiration gap suggests that unequal university access in Germany is not due to (perceived) short-term credit constraints.

cy and certainty both for respondents with and without a university degree. This implies that our findings are unlikely to be driven by respondents' inattention to the information treatments or by differences in information processing between respondents with different educational backgrounds.

We also present descriptive evidence indicating the extent to which differences in beliefs about the returns and costs of university education can account for the educational aspiration gap. Prior beliefs show the expected associations with educational aspirations.[99] However, these associations and the differences in prior beliefs by educational background are too small to explain the educational aspiration gap.

The costs of university education arise early, but the benefits only realize in the (uncertain) future. Therefore, differences in economic preferences between respondents with and without a university degree could be a complementary explanation for the educational aspiration gap. Focusing on time preferences, risk preferences, and overconfidence, our descriptive analysis shows that, even though these traits differ markedly by educational background, they also cannot account for the educational aspiration gap. In sum, our results suggest that consideration of economic returns, costs, and preferences does not add to an understanding of the educational aspiration gap in Germany. These findings suggest that the scope for interventions aimed at reducing informational or behavioral biases to enhance equity in Germany is limited.

This chapter contributes to the literature on how expectations of college returns and costs relate to educational choices.[100] In particular, we add to a range of experimental studies mostly from the American continent that investigate the effects of information provision on students' educational aspirations and choices.[101] While most related studies are based on small, self-selected student samples, often from disadvantaged backgrounds, our sample is representative of the German adult population, allowing us to provide a representative assessment of the educational aspiration gap in society. In this sense, our study is closest to Bleemer and Zafar (2018), who show positive effects of informing about college returns (but not about costs) on educational aspirations for children in a representative sample of household heads in the United States in a way that reduces socioeconomic aspiration gaps. This finding is not born out in our setting, a difference that might be related to

[99] That is, prior beliefs about the earnings and unemployment premium of university graduates, and about available student aid, are significantly and positively associated with university aspirations.

[100] See, for example, Arcidiacono (2004), Arcidiacono et al. (2012), Kaufmann (2014), Hoxby and Turner (2015), and Belfield et al., 2016.

[101] See Bettinger et al., 2012, Hoxby and Turner (2013), Oreopoulos and Dunn (2013), Dinkelman and Martínez (2014), Wiswall and Zafar (2015b), Hastings et al. (2015), Peter and Zambre (2017), and Baker et al. (2017).

differences in several institutional features between the two settings. For example, rates of university enrollment are traditionally low in Germany, where the apprenticeship sector offers an alternative career path that is valued highly by large parts of the population. Hence, the marginal student to attend university in Germany might differ markedly from the marginal college student in the United States.[102]

The remainder of the chapter is organized as follows. Section 5.1 describes the survey data we use for our analysis. Section 5.2 describes our experimental design. Section 5.3 presents the empirical model. Section 5.4 shows evidence on the socioeconomic gap in educational aspirations and in beliefs about the returns and costs of university education. Section 5.5 presents our experimental results on the effects of providing information about returns and costs on educational aspirations. Section 5.6 adds descriptive evidence on the extent to which differences in prior beliefs about returns and costs as well as differences in economic preferences can account for the educational aspiration gap. Section 5.7 concludes.

5.1 Data

We conduct our analyses within the framework of the ifo Education Survey. For the purposes of this chapter, the sample covers a total of 7,270 respondents aged 18 and above, with 3,302 respondents sampled in 2016 and 3,968 respondents sampled in 2017 (see chapter 4 for further details). Item non-response is again low, for example at 2 percent for main outcome measure of educational aspirations. Treatment status does not predict the share of missing answers for any outcome measure. As in chapter 4, we invited respondents in the online sample of the 2017 survey wave to participate in a later follow-up survey that re-elicits some outcomes, but does not comprise any information treatment. A total of 2,300 respondents (62 percent of the 2017 online sample) participated in the follow-up survey (see section 5.5.4 for further details).

Table 5.1 provides an overview of participants' sociodemographic characteristics. In particular, 19 percent of respondents hold a university degree, 68 percent hold an apprenticeship degree, and 12 percent do not hold any professional degree.[103] Among all respondents, 59 percent have children and 28 percent have children who have not yet completed their education.

[102] In section 5.7, we provide a more detailed discussion of these and other potential explanations for why information on returns and costs does not close the university aspiration gap in Germany.

[103] Throughout this chapter, holding a university degree includes degrees from Germany's so-called universities of applied sciences (*Fachhochschulen*).

5.2 Experimental Design

The goal of our experimental investigation is to evaluate whether ignorance of the returns and costs of university education can contribute to an explanation of the socioeconomic gap in educational aspirations. To this end, we randomly provide information about the economic returns and costs and estimate whether this affects participants' educational aspirations for their children. In what follows, we first describe how we elicit educational aspirations. We then present the different experimental information treatments, which form the basis of our empirical analysis. Next, we report how we elicit participants' beliefs about returns and costs. Finally, we describe our follow-up survey.

5.2.1 Elicitation of Educational Aspirations

In Germany, people have two main options for their educational careers: they can either pursue an apprenticeship or a university education. At the end of lower secondary school (10th grade), the majority of students in Germany chooses either to start vocational training (usually in the form of a dual apprenticeship that combines formal schooling with in-company training) or to continue on an academic track in upper secondary school which leads to the university entrance certificate (*Abitur*).[104] The share of students on the academic path increased over the past decades: While school graduates' enrollment in vocational training was more than twice as high as university enrollment in 1999, the latter exceeded the former by 2013 (Autorengruppe Bildungsberichterstattung, 2014; Thies et al., 2015).

Our main outcome of interest is the aspiration that adults have for the educational outcome of their child. Therefore, we ask participants to answer the following question: "Irrespective of whether you have any children and of which educational degree your child holds or is likely to attain in the future: Which educational degree would match your personal ideal conception for your child?"[105] Respondents are asked to choose one of the two general degree categories available in Germany, either "apprenticeship degree" or "uni-

[104] Students can leave school to start vocational training after grade 10 (grade 9 in some states) at the earliest. Those pursuing the academic track usually remain in secondary school to earn the *Abitur* after grade 12 or 13. While most students with the *Abitur* enroll in university, a non-negligible share of about one quarter takes up vocational training (see Autorengruppe Bildungsberichterstattung, 2016). Focusing on a selected group students in Berlin attending the academic track of upper secondary schools, Peter and Zambre (2017) show that information about labor market benefits and funding possibilities of university education increases university aspirations of students from families without university background. We complement this finding by investigating information effects on the overall university aspiration gap of the German population.

[105] Appendix Table A5.1 presents the exact wording of the questionnaire items used in this chapter.

versity degree."[106] This design allows us to estimate the effect of providing information on respondents' educational aspirations for the generation of their children.

5.2.2 Randomized Information Treatments on Returns and Costs

To test whether respondents' educational aspirations for their children depend on their knowledge of the returns and costs of university education, we devise two survey experiments that randomly assign respondents to a control group and to different information treatment groups. In the first experiment conducted in 2016, we provide participants with information on the economic returns to university education. In the second experiment conducted in 2017, we provide participants with information on the costs—tuition fees and available student aid—of obtaining a university degree.

The first experiment in 2016 focuses on economic considerations regarding the returns of a university education. The sample is randomly split into three groups, one control group and two treatment groups. Respondents in the control group answer the question on educational aspirations described above without any further information. Before answering the same question, the first treatment group is informed that, on average, full-time employed university graduates earn about 2,750 Euro after taxes per month, compared to about 1,850 Euro for those with an apprenticeship degree and 1,400 Euro for those who do not hold any professional degree (own calculations based on the German Microcensus 2013). Respondents in the second treatment group are informed that the average unemployment rate of university graduates is 2.5 percent, while the unemployment rates of those with an apprenticeship degree and those without any degree are 5 percent and 20 percent, respectively (IAB, 2015).

The second experiment, conducted in 2017, focuses on the cost side of pursuing a university education. The sample is split into one control group and three treatment groups. The first treatment informs respondents that university students in all of Germany currently do not have to pay any tuition fees before asking them the same question on educational aspirations as the uninformed control group. While university education tended to be free of charge in Germany, several states had introduced tuition fees of 500 Euro per semester during the time period between 2006 and 2014, and people may not be aware that tuition fees have since been abolished again in all German *Länder* (Lergetporer and Woessmann, 2018). Respondents in the second treatment group are informed that comprehensive pub-

[106] Even though it is possible to obtain both an apprenticeship and a university degree, we ask respondents to choose between the two in order to elicit their main preference. Empirically, the share of individuals who hold both degrees is very small (about 2 percent of respondents in our sample).

lic student aid (known as *BAföG*) is available to university students in Germany, only half of which has to be paid back later at most.[107] The treatment also includes the example that students with two non-working siblings whose parents' gross annual income does not exceed 50,000 Euro would generally be eligible to student aid payments of 649 Euro per month.[108] The third treatment group receives both pieces of information, on the absence of tuition fees and on the availability of student aid.

5.2.3 Elicitation of Beliefs about Returns and Costs

We are also interested in directly assessing the extent to which people with different educational backgrounds misperceive the returns and costs of a university degree. This will enable us to test whether different levels of ignorance are a relevant mechanism through which the information treatments may affect educational aspiration gaps. Much earlier in the survey, before providing information and eliciting aspirations, we therefore measure respondents' beliefs about the returns and costs of university education.

To elicit baseline beliefs for the first experiment, we ask respondents in the first wave to estimate the monthly earnings and the unemployment rates of university graduates and of those without any professional degree. To reduce noise in respondents' estimates, the questionnaire items inform them that current monthly earnings of those with an apprenticeship degree are about 1,850 Euro and that their unemployment rate is about 5 percent, respectively. Therefore, the answers allow us to estimate the university premium perceived by respondents in comparison to apprenticeship education. After giving their respective answers, respondents are asked to report how sure they are that their answer is close to correct on a seven point scale (from "1 very unsure" to "7 very sure").

To elicit baseline beliefs for the second experiment, we ask respondents in the 2017 wave to estimate what level of tuition fees students in their state are generally required to pay. The instructions explicitly mention that respondents should enter a value of zero in case they think no tuition fees are charged. We also ask respondents to estimate the level of public financial aid that university students are eligible for, asking them to imagine the example of students with two non-working siblings whose parents earn 50,000 Euro per year (see Appendix Table A5.1 for details). After giving their answer, respondents again indicate how sure they are about their answers.

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[107] *BAföG* is the well-known acronym of the applicable legislation, *Bundesausbildungsförderungsgesetz*.

[108] Since the exact amount of student aid depends on a large number of household characteristics, we use the example to give respondents an impression of student aid levels in Germany. See www.bafög.de/de/bundesausbildungs--foerderungsgesetz---bafoeg-204.php for the legal provisions and www.bafög.de/de/beispiele-183.php for selected examples [accessed 20 December 2017].

5.2.4 Follow-up Survey

The follow-up survey, conducted in 2017, again asks respondents to estimate the level of tuition fees and available student aid and to state the educational aspirations for their children. In the follow-up survey, all respondents answer the control-group version of the questions, i.e., without any information provision.

This design allows us to speak to the persistence of information effects and to test whether information provision does indeed improve respondents' knowledge of the returns and costs of university education in a way that is still observable after a time period of about two weeks.

5.3 Empirical Model

The random assignment in the experimental design allows us to estimate the causal effects of information provision on educational aspirations in a simple linear probability model. In particular, we estimate the following regression:

$$y_i = \alpha_0 + \sum_k \alpha_1^k T_i^k + \delta' X_i + \varepsilon_i \quad (1)$$

where y_i is a dummy equal to 1 if respondent *i* prefers university education for her child, T_i^k is an indicator of whether respondent *i* received the information treatment *k*, X_i is a vector of control variables, and $\boldsymbol{\varepsilon}_i$ is an error term. The coefficients of interest, a_1^k, are identified by the random assignment of treatment status. Adding control variables should therefore not alter the estimated treatment coefficients, although it might increase the precision of the estimates. Throughout the chapter, we estimate versions of equation (1) with and without covariates.

As we are ultimately interested in the extent to which information provision is able to close the socioeconomic gap in educational aspirations, we also estimate treatment effect heterogeneities with respect to respondents' educational attainment. For this purpose, we extend the model in equation (1) to:

$$y_i = \beta_0 + \sum_k \beta_1^k T_i^k + \beta_2 E_i + \sum_k \beta_3^k T_i^k * E_i + \delta' X_i + \eta_i \quad (2)$$

where E_i equals one if respondent *i* does not hold a university degree. The estimate of β_2 represents the educational aspiration gap, i.e., the association between respondents' educational background and their aspirations in the control group. The estimates of β_1 and $\beta_1 + \beta_3$ reflect the effect of information provision for respondents with and without uni-

versity education, respectively. These are our parameters of interest as they show whether information provision affects the gap in educational aspirations.

While equations (1) and (2) test whether information provision affects respondents' educational aspirations for their children, we are also interested in the extent to which respondents' prior beliefs about the returns and costs of university education can account for the educational aspiration gap. Therefore, we also estimate the following regression:

$$y_i = \gamma_0 + \gamma_1 E_i + \sum_k \gamma_2^k B_i^k + \tau_i \quad (3)$$

where B_i^k is respondent *i*'s belief about the information provided in treatment *k* (i.e., the belief about earnings and unemployment rates across educational groups, tuition fees, and available student aid). The main parameter of interest is γ_1, which represents the educational aspiration gap that remains after accounting for differences in beliefs. The parameters γ_2 capture the association between beliefs and educational aspirations.

Balance across control and treatment groups. If the randomization procedure works as intended, it provides balance between treatment and control groups on all observable and unobservable characteristics. To assess the balance of observable characteristics, columns 2 to 6 of Table 5.1 report the estimation results of the following equations:

$$T_i^k = \theta_0 + \theta_1 X_i + \omega_i \quad (4)$$

for each covariate *X* and each treatment group *k*.

Results indicate that covariates are indeed balanced across the different groups and do not predict treatment status. Of 90 estimates of θ_1, eleven are significant at the 10 percent level or lower, four are significant at the 5 percent level or lower, and one is significant at the 1 percent level. The observed differences match the differences we would expect to observe by chance very closely.

We also test the joint significance of all covariates in predicting treatment status (see the *p*-values of the joint *F* tests at the bottom of Table 5.1). For none of our experiments, covariates are jointly significant in predicting treatment status. Similarly, item non-response on our main outcome measure, educational aspiration for the child, does not predict treatment status. In conclusion, we are reassured that randomization worked as intended, which allows the identification of causal treatment effects.

5.4 Results on Socioeconomic Gaps in Educational Aspirations and Beliefs

Before we report findings from our experimental analysis, we start the presentation of our empirical results by documenting the gaps in educational aspirations and in beliefs about returns and costs of university education between respondents with and without a university background.

5.4.1 The Educational Aspiration Gap

The first question of interest is which share of the German population aspires to university education for their children, and how this share varies with respondents' own educational background. As indicated in Figure 5.1, on average 43 percent of the German population considers a university degree the ideal educational outcome for their children. The majority of 57 percent of the population prefers their children to pursue an apprenticeship degree. Compared to other countries the share of those aspiring to university education is relatively low in Germany. For instance, about 80 percent of respondents in the United States aspire to college education for their children (Bleemer and Zafar, 2018). This difference likely reflects the availability and dominant role of the apprenticeship system in Germany that provides an alternative that is well appreciated—despite the substantial average earnings differences indicated above.

Importantly, the population average masks substantial heterogeneity in aspirations by respondents' own educational background. While only about a third of respondents (36 percent) without a university degree aspire to university education for their children, this share is nearly three quarters (74 percent) among respondents who themselves hold a university degree. This difference of 38 percentage points is the educational aspiration gap that motivates our analysis in this chapter.

Interestingly, the aspiration shares correspond closely to the actual current university enrollment decisions of children from different educational backgrounds in Germany (Middendorff et al., 2013). Among children whose parents do not have a university degree, 43 percent enroll in the upper-secondary school track (*gymnasiale Oberstufe*) that leads to a university entrance certificate and 23 percent eventually enroll in university. By contrast, among children who have at least one parent with a university degree, 79 percent enroll in the upper-secondary school track and 77 percent enroll in university. The similarity between the aspirations elicited in our survey and actual enrollment decisions corroborates the relevance of our outcome of interest by suggesting a leading role of parental aspira-

tions for ultimate educational decisions of children and for actual intergenerational educational mobility.

5.4.2 Gaps in Beliefs about Returns and Costs of University Education

A commonly hypothesized explanation for gaps in educational aspirations is that individuals without a university degree underestimate the returns and overestimate the costs of university education. In this section, we investigate the prevalence of imperfect information and informational asymmetries regarding earnings and unemployment rates by educational groups, as well as regarding the average level of tuition fees and available student aid. We regress respondents' expressed beliefs about these measures on an indicator for individuals who do not hold a university degree. To facilitate interpretation, we center respondents' beliefs at the correct value of the respective variable and express them in relative terms by dividing through the respective correct value:[109] $\frac{Estimated\ value - Correct\ value}{Correct\ value}$.

The results in Panel I of Table 5.2 indicate that respondents without a university degree are more likely to underestimate the returns to university education. As indicated by the regression constants in columns 1 and 2, respondents with a university degree correctly estimate the earnings of university graduates, but they overestimate the unemployment rate by more than 280 percent.[110] That is, even the average university graduate is partially ignorant about the labor market returns to a university degree.

Importantly, the gap between beliefs and true values is significantly larger for respondents without a university education. The significant coefficients on the indicator for not having a university education show that people without a university education underestimate the earnings differential by an additional 4 percent and the unemployment differential by an additional 130 percent.[111] Consistent with their less correct beliefs, respondents without university education are also significantly less certain that their answers are close to correct (columns 3 and 4).

[109] As the correct value of tuition fees is zero, we divide tuition fees by 100 Euro. For unemployment rates, we multiply by -1 so that higher values correspond to lower unemployment estimates. To avoid being driven by severe outliers on the expressed beliefs, we trim the top and bottom 2 percent of the belief distributions throughout.

[110] Appendix Figure A5.1 depicts the distributions of beliefs about returns and costs of university education for respondents with and without a university degree.

[111] Taking into account the entire distribution, two sample Kolmogorov-Smirnov tests also reject the null hypothesis that beliefs do not differ by respondents' education.

In Panel II, we report equivalent estimates for beliefs about tuition fees and student aid. Respondents with a university degree turn out to overestimate tuition fees by 206 Euro per semester and underestimate student aid by 62 percent. Again, this pattern is significantly more pronounced for respondents without a university education. They overestimate tuition fees by an additional 75 Euro and underestimate student aid by an additional 4 percent. As before, respondents without a university education are less certain about the accuracy of their answers, particularly for beliefs about tuition fees (columns 3 and 4). Next, we investigate to what extent these biased beliefs determine the socioeconomic gap in university aspirations.

5.5 Experimental Results on the Effects of Information on Aspirations

This section presents our main results. We analyze the extent to which alleviating the described biases in beliefs through randomized information provision affects educational aspirations. We provide evidence on the effects of providing information on the returns to and costs of university education, respectively, to the adult population. We also report results for the subgroup of parents. Finally, we test whether the information treatments of the main survey have persistent effects in a follow-up survey conducted about two weeks later.

5.5.1 Providing Information on Returns to University Education

Our experimental interventions show that providing participants with information about the respective earnings levels of people with different educational degrees increases their aspiration for their children to obtain a university education. Columns 1 of Table 5.3, which is based on equation (1), shows that earnings information increases the share of respondents who aspire to university education for their children by 5 percentage points. Informing respondents about unemployment rates across educational groups yields a smaller, statistically insignificant increase of 2 percentage points. The inclusion of standard covariates in column 2 does not affect the qualitative results.[112]

Estimating treatment effects by participants' own educational background reveals that providing information about the returns to university education does not, however, reduce the socioeconomic gap in educational aspirations. Quite to the contrary, the estimates in

[112] The covariates include the following sociodemographic characteristics: age, gender, income, employment status, born in Germany, living in West Germany, municipality size, living with a partner, parent status, risk tolerance, and patience.

columns 3 and 4—based on equation (2)—indicate that the treatment effects of earnings and unemployment information tend to be even stronger for the group of university graduates (see also Figure 5.2). Providing earnings information significantly increases university aspirations among respondents with university education by 11 percentage points and without university education by 5 percentage points. A similar, albeit statistically insignificant, pattern emerges for information on unemployment rates, which increases aspirations of the two groups of respondents by 8 and 1 percentage points, respectively. While the differences between the two groups do not reach statistical significance, the point estimates indicate that treatment effects are substantially larger in the group of university graduates.[113] Thus, if anything, it is the university graduates who respond most strongly to information provision by raising the educational aspirations for their children. The results clearly show that the educational aspiration gap cannot be attributed to the underestimation of returns to university education among respondents without a university degree.

5.5.2 Providing Information on Costs of University Education

In the second experiment, we investigate whether incorrect beliefs about the costs of university education can account for the difference in educational aspirations across educational backgrounds. While the benefits of university education accrue over long time horizons, its costs are more immediate. Hence, costs of university education might be more salient when stating educational aspirations which, in turn, might render cost information more effective for mitigating the educational aspiration gap, in particular given the fact described above that respondents overestimate tuition fees and underestimate student aid.

Our results indicate, however, that informing about the costs of university education also does not reduce the aspiration gap. As shown in Table 5.4, informing respondents that universities in Germany generally do not charge tuition fees does not affect the expressed aspirations in the entire sample (columns 1 and 2). It also does not have heterogeneous effects on respondents with and without a university degree (columns 3 and 4).[114]

Providing information on the level of student aid does in fact widen the educational aspiration gap. While there is no effect on university aspirations on average (column 1), this

[113] Among respondents without university education, 85 percent hold an apprenticeship degree and 15 percent do not hold any degree. Treatment effects of providing earnings- and unemployment information are marginally significantly stronger for the latter group (results available upon request).

[114] The gap in educational aspirations turns out slightly larger in the 2017 survey than in the 2016 survey (41 versus 38 percentage points; see the coefficients on not having a university education in column 3 of Tables 5.3 and 5.4).

information treatment significantly increases the aspirations of respondents with a university degree by 8 percentage points but does not affect aspirations of those without a university degree (column 3 and 4). As a consequence, informing participants about the availability of student aid widens the gap in aspirations by 8 percentage points (marginally significant). For the third treatment, where respondents receive both pieces of information on tuition fees and student aid, there are again no significant effects.[115]

Overall, our results suggest that neither a lack of information on the benefits nor on the costs of university education is able to explain the gap in educational aspirations in Germany.[116]

5.5.3 Treatment Effects on Parents

The fact that our sample is representative for the German adult population enables us to assess the nationwide educational aspiration gap. A potential concern with the above results, however, is that the inability to close the aspiration gap through information provision might be driven by respondents who do not have children, and hence perceive the question as inconsequential. If information updating is costly (Benabou and Tirole, 2016), these respondents might fail to respond to new information in a hypothetical scenario, even though they would consider the information in an actual choice situation. To rule out that our results are driven by such inertia in aspirations, we repeat the analysis for the subsample of parents whose children are still in the education system (N=920 in the returns experiment and N=1,058 in the costs experiment).

The results for the subgroup of parents are very similar to the full adult population. As in the full sample, there is a positive treatment effect of providing parents with earnings information, although this effect does not reach statistical significance at conventional levels on average (Appendix Table A5.2). However, investigating heterogeneous treatment

[115] Among respondents without university education, we find that information about tuition fees increases university aspirations of those without any degree (marginally significant) while not affecting the aspirations of respondents with an apprenticeship degree. Information on student aid does affect the aspirations of either of the two groups (results available upon request).

[116] The fact that we elicited respondents' beliefs prior to the information experiments allows us to estimate effect heterogeneities by initial beliefs. If the information treatments affect university aspirations through genuine belief updating, we would expect that treatment effects are larger the more a respondent underestimates (overestimate) university returns (costs). If, on the other hand, the information treatments merely operate through increasing the salience of returns and costs when making educational choices, we would not expect such treatment effect heterogeneities. In line with Bleemer and Zafar (2018), we find no significant effect heterogeneities by prior beliefs, suggesting that the treatment effects in Table 5.3 and 5.4 are salience-based (results available upon request). This interpretation is consistent with the finding that providing earnings information increases university aspirations of university graduates, even though these respondents hold correct initial beliefs about the earnings differential.

effects by respondents' educational background reveals a positive, significant, and large effect of 20 percentage points on parents who are university graduates themselves, and a small and insignificant effect on parents without university education. Consequently, the educational aspiration gap among parents, if anything, tends to increase with information provision on earnings.

A similar picture emerges for providing information on the costs of university education. Appendix Table A5.3 shows that information on tuition fees and student aid does not close the aspiration gap among parents. In particular, information on student aid significantly increases the educational aspirations of parents with a university degree by 12 percentage points, whereas the point estimate is smaller and statistically insignificant for parents without a university degree.

Taken together, the finding that information on economic returns and costs of university education does not account for the educational aspiration gap in the German population is mirrored in the subsample of parents. Thus, our results are not driven by respondents without children who might perceive the question on educational aspirations as less relevant.[117]

While aspirations for ideal, as opposed to realistic, educational degrees have the advantage that they are, in principle, less constrained by real-life institutional or child-specific factors (and thus potentially more responsive to information treatments), another potential concern could be that parents internalize observed constraints into their ideal aspirations (Manski, 1999). This could in principle account for our finding that treatment effects for respondents without a university degree are rather limited. To assess this possibility, we use data from the oversample of parents in the 2015 wave of the ifo Education Survey (see chapter 3 for details). Among parents of children who did not yet complete their educational career, we elicited parents' subjective likelihood that their child would graduate from university, as well as their ideal educational aspirations for their child (both measured on a 5-point Likert scale).

Expectably, parents without a university education report lower likelihoods of their children graduating from university. Importantly, though, the gap in educational aspirations between parents with and without a university degree is large (Appendix Table A5.4, column 1) and remains significant when conditioning on the subjective expectation that the

[117] In our subsample of respondents without children, we do not find any significant treatment effects (results available upon request). Thus, the above finding that information provision on returns and costs tends to increase the aspiration gap is entirely driven by parents.

child will actually obtain a university degree (columns 2 and 3). These findings show that respondents' aspirations are not entirely determined by the realistic likelihood of what degree a child will obtain. Thus, the internalization of real-life constraints is unlikely to account for the unresponsiveness of the educational aspiration gap with respect to information provision.

5.5.4 Persistence of Information Treatments in the Follow-up Survey

To assess whether the information treatments truly change the information status of participants, we resurvey the online sample in the 2017 wave of the ifo Education Survey about two weeks after the main survey. The follow-up survey again asks respondents about their educational aspirations for their child, as well as their beliefs about tuition fees and student aid, but does not contain any new information treatment. This allows us to test whether improved knowledge persists over a two-week period, which also addresses the potential concern that the limited treatment effects reported above are due to respondents not understanding or internalizing the information provided by the treatments.

Follow-up participation is high, with 62 percent of respondents (2,300 of the 3,696 online respondents in the main survey) participating again. Participation in the follow-up survey is not related to treatment status in the main survey, reducing potential concerns of bias from non-random selection into the follow-up. First, treatment status in the main survey does not predict participation in the follow-up survey (Appendix Table A5.5).[118] Second, follow-up respondents' background characteristics are well balanced between respondents who had been assigned to the control group and the three information treatment groups in the main survey (Appendix Table A5.6).

Table 5.5 reports the effects of providing information during the main survey on beliefs about tuition fees and student aid expressed about two weeks later in the follow-up survey. Respondents' answers to the same belief questions in the main survey are powerful predictors for their answers in the follow-up survey. This considerable test-retest correlation strengthens confidence in our survey measures of beliefs.

[118] As the follow-up survey could only be conducted in the online part and not the offline part of the original sample, participants in the follow-up survey differ from participants in the representative main survey in several background characteristics. Of the significant differences shown in Appendix Table A5.5, only risk tolerance and patience remain significant (and age becomes significant) when restricting the analysis to the participants in the online sample of the main survey, indicating that differences are mostly driven by our restriction of the follow-up survey to the online sample and not by individual decisions to participate in the follow-up survey.

More importantly, the randomized provision of information about fees and aid during the main survey significantly improves the accuracy of respondents' beliefs about the levels of tuition fees and student aid in the follow-up survey. In particular, informing respondents that there are no tuition fees significantly reduces respondents' estimates of tuition fees in the follow-up survey both in the fee-information-only treatment and in the joint treatment with aid information (with the former reaching significance only among university graduates, columns 1 and 2). As respondents on average overestimated the level of tuition fees in the main survey, the information treatments thus lead to an improved knowledge of tuition fees among participants two weeks later. Furthermore, these persistent treatment effects do not differ significantly between respondents with and without a university education.

Similarly, informing about the level of student aid in the main survey significantly increases respondents' estimates of student aid in the follow-up survey both in the aid-information-only treatment and in the joint treatment with fee information (columns 3 and 4). Given the initial underestimation of student aid in the main survey, the positive treatment effects again indicate that information provision persistently improves beliefs about the level of available student aid. Again, the information treatment effects do not differ significantly between those with and without a university degree.[119]

Information provision also significantly increases how certain respondents are about the accuracy of their beliefs. Results in columns 5 to 8 show that respondents who received the respective cost information in the main survey are more certain that their beliefs are close to correct in the follow-up survey. The same is not true for respondents who received the other piece of information that is not the subject of the respective belief question. There is no significant difference between those with and without a university education in the extent to which information provision raises certainty about their beliefs.

Despite persistent effects on improved beliefs about the costs of university education, the information treatments still do not reduce the educational aspiration gap in the follow-up survey. As shown in Appendix Table A5.7, the effects of providing information about tuition fees and student aid in the main survey on educational aspirations in the follow-up survey are very similar to the immediate effects in the main survey (see Table 5.4) in being mostly small and statistically insignificant. The effect of providing information on student aid to individuals with a university degree is positive but shy of statistical significance, while the

[119] Interestingly, the magnitude of treatment effects on beliefs elicited in the follow-up survey is unrelated to the time elapsed between the main survey and the follow-up survey (results available upon request).

difference in treatment to individuals without a university degree remains marginally significant.

Overall, the information treatments lead to persistent improvements of belief accuracy and certainty among respondents with and without a university degree about two weeks after the provision of the information in the main survey. This indicates that participants did process the information they received in the main survey and remember it in the follow-up survey, documenting that the information treatments do in fact lead to a persistently improved information status. Importantly for the interpretation of our analysis, the consistency of these findings across educational backgrounds also suggests that inattention or differences in information processing in the survey environment across educational backgrounds are unlikely to explain the lack of information treatment effects on the educational aspiration gap.

5.6 Descriptive Evidence on Gaps in Beliefs, Preferences, and Aspirations

We complement our analysis by providing descriptive evidence on the extent to which differences between those with and without a university degree in (i) their beliefs about economic returns and costs of university education and (ii) in their economic preferences can account for the educational aspiration gap.

5.6.1 Can Differences in Prior Beliefs Account for the Educational Aspiration Gap?

Our experimental results show that information provision cannot close the gap in educational aspirations between respondents with different educational backgrounds. In this section, we provide complementary descriptive analysis to assess the role of prior beliefs in the aspiration gap. In particular, we estimate equation (3) for control-group respondents to get a sense of how much of the aspiration gap remains after conditioning on respondents' beliefs about the returns and costs of a university degree.[120]

[120] This analysis addresses another potential concern for why our information treatments might not close the educational aspiration gap: defiant reactions to preference-incongruent information might give rise to bias from so-called "backfire effects" (Nyhan and Reifler, 2010). In our experiments, this behavioral anomaly could arise if respondents who prefer an apprenticeship degree for their children reinforce this position after learning that the relative returns to this degree are lower than expected. This bias is not a concern in the analysis of this section, which is restricted to the uninformed control group.

As Table 5.6 shows, prior beliefs about the earnings and unemployment premium of university graduates are indeed significantly positively associated with university aspirations. While these associations point in the expected directions, conditioning on the differences in prior beliefs reduces the educational aspiration gap of 38 percentage points by only 2 percentage points.[121] Likewise, additionally controlling for the certainty with which respondents hold the beliefs does not mitigate the educational aspiration gap (column 3). The differences in prior beliefs about the returns to university education between those with and without a university degree (as shown in Table 5.2) and their association with educational aspirations (Table 5.6) are simply too small to account for a noteworthy share of the educational aspiration gap.

Table 5.7 presents results of the analogous analysis for prior beliefs about costs of university education. Again, respondents with higher beliefs about available student aid are (marginally significantly) more likely to aspire to a university degree for their children. Beliefs about tuition fees are unrelated to educational aspirations. Similar to the above findings, controlling for beliefs about university costs leaves the gap in aspirations unaffected.

These results corroborate our previous conclusion that information asymmetries about the returns and costs of university education between persons with and without a university degree cannot account for the educational aspiration gap. While differences in prior beliefs exist and are relevant for educational aspirations, these associations are not large enough to be able to account for the gap in educational aspiration.

5.6.2 Can Differences in Economic Preferences Account for the Educational Aspiration Gap?

Finally, in a similar descriptive analysis we investigate the extent to which differences in economic preferences by educational background can account for the persistent gap in educational aspirations. So far, our analysis has focused on the role of asymmetric information regarding the returns and costs of university education. However, the costs of university education have to be incurred early on whereas the returns accrue only much later, so that, according to classic human capital investment theory, educational decisions depend on the present discounted value of education (Becker, 1964). Thus, a potential alternative explanation for the gap in aspirations is that respondents with and without a uni-

[121] Similarly, Belfield et al. (2016) find for high-school students in the United Kingdom that perceived returns of university education do not change the association between intended university attendance and parental education levels.

versity degree differ in time preferences and other economic preferences that determine the present discounted value of educational choices.

We evaluate the role of three such traits for the educational aspiration gap. In addition to time preferences, we investigate risk preferences and overconfidence. Our focus on risk preferences is motivated by the notion that educational decisions are characterized by uncertainty about whether a degree will be completed and whether returns will materialize. Individuals with lower levels of risk tolerance might therefore prefer lower levels of education (Altonji, 1993). Relatedly, overconfidence might affect educational aspirations because of its link to the expected success of degree completion (Koch et al., 2015; Reuben et al., 2017).[122]

We measure respondents' time and risk preferences at the end of our survey using experimentally validated survey questions from Falk et al. (2016). Patience is elicited by the question: "In comparison to others, are you a person who is generally willing to give up something today in order to benefit from that in the future or are you not willing to do so?" Respondents record their answers on an 11-point Likert scale from "0 completely unwilling to give up something today" to "10 very willing to give up something today". Similarly, respondents answer the question on risk tolerance—"How do you see yourself: are you a person who is generally willing to take risks, or do you try to avoid taking risks?"—on an 11-point Likert scale from "0 completely unwilling to take risks" to "10 very willing to take risks".

To obtain a measure of relative overconfidence, we apply the method described by Ortoleva and Snowberg (2015) which compares the accuracy of respondents' beliefs with how confident they are that their beliefs are close to correct. In particular, the ifo Education Survey contains a number of questions that measure beliefs about different educationally relevant parameters, each followed by a question about the respondents' certainty that their expressed belief is close to correct (from "1 very unsure" to "7 very sure"). For each question, we regress certainty on a fourth-order polynomial of belief accuracy. Next, we subtract the predicted certainty for each respondent from her actual reported certainty. In a final step, we aggregate these relative measures of overconfidence over all questions into our final measure of overconfidence.

[122] Since our study focuses on educational aspirations for children, the link from respondents' behavioral traits to these aspirations might be more indirect than to the respondents' aspirations for themselves. However, parents' behavioral traits have been shown to predict children's educational choices (Wölfel and Heineck, 2012).

A necessary condition for the three dimensions of economic preferences to be able to account for the educational aspiration gap is that they differ between respondents with and without a university degree. Table 5.8 shows that respondents without a university education indeed have significantly lower values of patience, risk tolerance, and overconfidence.[123] These results, which are well in line with previous studies (e.g., Dohmen et al., 2011; Golsteyn et al., 2014), set the stage for analyzing the extent to which differences in these economic preferences are able to account for the gap in educational aspirations.

It turns out that, just as in the case of misinformation about returns and costs, conditioning on the three economic preferences does not close the educational aspiration gap. Table 5.9 shows results from regressions analogous to equation (3). Whether considered individually or jointly, risk tolerance and overconfidence do not enter the model significantly, whereas patience is significantly positively associated with educational aspirations. Still, accounting for differences in patience reduces the educational aspiration gap only from 40 to 39 percentage points.

In sum, we find differences in time and risk preferences and overconfidence by respondents' educational degrees that are consistent with the previous literature. Similar to the results on information asymmetries above, though, these differences cannot account for the large gap in educational aspirations.

5.7 Conclusions

As in many other countries, there is a large gap in actual and aspired university enrollment by parents' educational background in Germany. In our representative survey, the share of the adult population that aspires for their children to go to university is 38 percentage points lower among those without a university degree than among those with a university degree. This chapter investigates whether lack of information on the returns and costs of university education among persons without a university degree can explain this educational aspiration gap. Using experiments with randomized information provision, we find that—although respondents without a university degree are more likely to underestimate the returns and overestimate the costs of higher education—alleviating these informational asymmetries does not close the educational aspiration gap. If anything, university graduates respond more strongly to the provided information by raising their educational aspirations, widening rather than closing the gap. Both respondents with and without a university education who received the information treatment show improved knowledge

[123] For this analysis, we pool the 2016 and 2017 waves of the ifo Education Survey. About 12 percent of respondents participated in both waves. Standard errors are clustered at the individual level.

of university returns and costs in a follow-up survey about two weeks after the experiment, indicating that participants indeed processed the provided information. Furthermore, economic preferences that are important for educational decisions—time preferences, risk preferences, and overconfidence—differ by respondents' educational background, but they also cannot account for the gap in educational aspirations.

Our results indicate that consideration of the standard parameters of the traditional economic model of educational choices—returns, costs, time preferences, and other economic preferences relevant for intertemporal choices—does not seem to add to an understanding of the educational aspiration gap in Germany. Consequently, there appears limited scope for policy interventions aimed at alleviating imperfect information such as information campaigns to close the gap in educational aspirations.

In contrast to the results presented here, a host of previous studies documents substantial effects of informing (prospective) students about returns and costs on educational aspirations and choices (e.g., Oreopoulos and Dunn, 2013; Wiswall and Zafar, 2015a; Peter and Zambre, 2017; Baker et al., 2017). The fact that this literature generally focuses on small subgroups of the population, such as low-income students, or students who self-selected into an academic track, is a likely reason for why its findings differ from those in our representative samples. Thus, while information effects on marginal students clearly carry policy relevance, they are uninformative about how information affects overall educational inequality in the society.[124]

We are only aware of one other representative study which investigates the effects of information about the returns and costs of university education on educational inequality: Bleemer and Zafar (2018) find that information about college returns decreases the educational aspiration gap between respondents with and without college education. Cost information, in contrast, does not affect the gap.[125] Several differences between the U.S. and Germany might account for diverging effects of information provision on the aspiration gap in both countries.[126] First, university enrollment rates are traditionally lower in Ger-

[124] Another potential reason for why most of the published studies find significant information treatment effects might be that null findings are hard to publish and therefore often not written up (e.g., Franco et al., 2014).

[125] Bleemer and Zafar's (2018) outcome variable is the reported likelihood that respondents would recommend a four-year college education for a hypothesized 15-year old child of a friend. For their subsample of parents, they also elicit the likelihood that the actual child attends college. In the subsequent discussion, we refer to the former measure of aspirations. Note, however, that their results on aspirations for the actual child are very similar.

[126] See Henderson et al. (2015) for a discussion of differences and similarities of the U.S. and the German education system.

many, a fact most likely due to the dominant role of the German apprenticeship sector which offers a valued alternative to university education. Consequently, baseline university aspiration is much higher in the U.S. than in Germany (82 percent versus 43 percent), and the university aspiration gap by university background is much smaller (12 percentage points versus 38 percentage points). Therefore, one potential reason for why earnings information in Bleemer and Zafar (2018) only increases college aspirations among respondents without college education (and not, as in our case, also among those who hold a tertiary degree) might be ceiling effects, since almost all respondents with a college degree (90 percent) aspire to college education for their children.

A second reason for why information mitigates inequality in educational aspirations in the U.S. but not in Germany might be cross-national differences in university returns and costs. Earnings returns to a university degree tend to be larger in the U.S. (OECD, 2017), which might render earnings information more effective to close the aspiration gap in the U.S. than in Germany. On the cost side, university tuition fees in the U.S. are among the highest in the world, while Germany is one of the countries which does not charge university tuition fees (OECD, 2017). Thus, a priori, one would expect that (perceived) short-term credit constraints are more likely to generate educational inequality in the U.S. than in Germany. Empirically, however, informing about university costs has the same null effect on university aspirations in both countries.

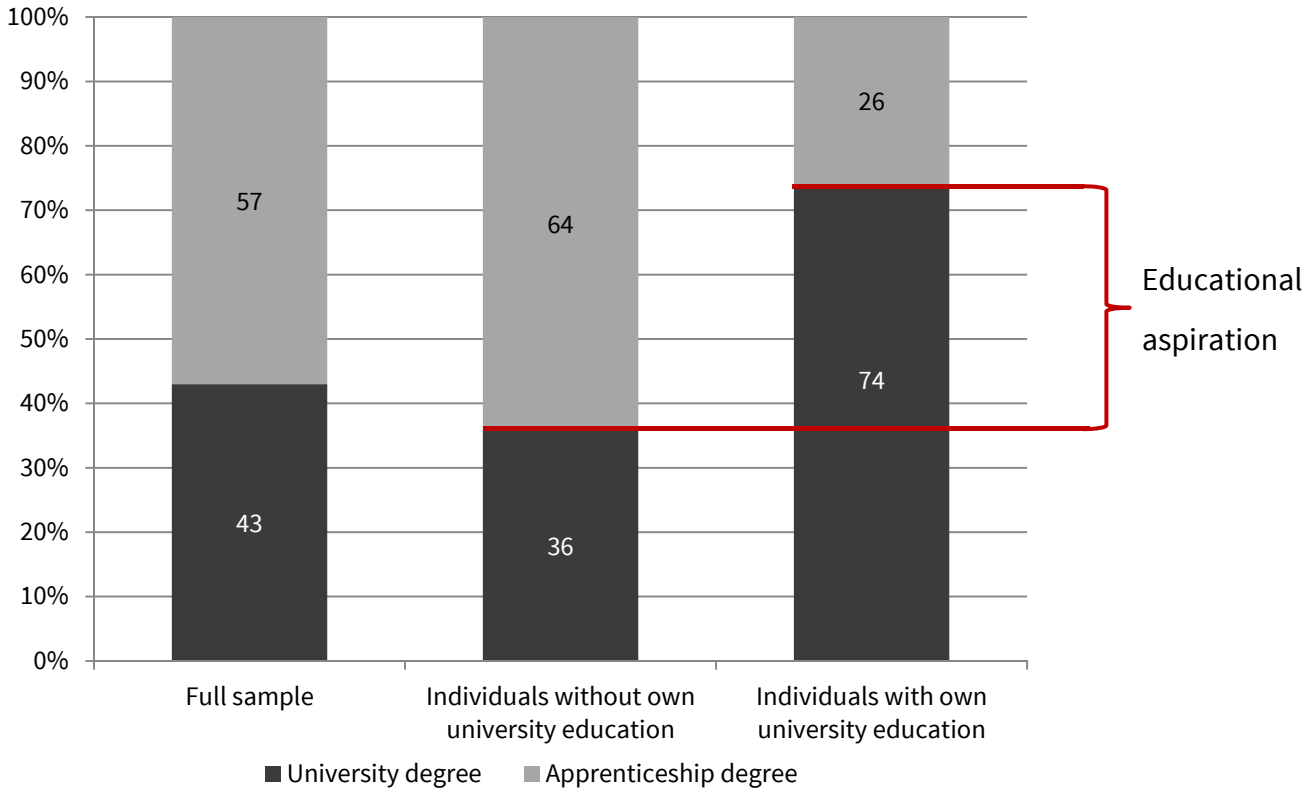
Finally, differences in the populations' preferences might account for different findings for the U.S. and Germany. In particular, given that tuition costs are substantial in the U.S., it might be that monetary consequences of educational decision are more salient for determining aspirations in the U.S. than in Germany. While we are not aware of any comparative evidence in this regard, labor market considerations seem to be important for determining educational choices in both countries, which renders this explanation somewhat unlikely.[127] Similarly, economic preferences which are important for educational decisions (in particular, time and risk preferences) are similar in both countries, though U.S. citizens seem to be slightly more patient and more risk taking than Germans (see Falk et al., 2018). In sum, these differences in preferences are relatively small, so that it is quite unlikely that

[127] In the United States 85 percent (70 percent) of college freshmen report that being able to "get a better job" (to "make more money") was "very important" for their decision to go to college (on a 3-point scale from "very important" to "not important"; see Eagan et al., 2017). In Germany, 61 percent of upper secondary school graduates who took up university studies, and 53 percent of those who pursue an apprenticeship education state that "favorable occupational- and earnings conditions" were important for their educational choice (stating a value of 1 or 2 on a 5-point scale from "1 very important" to "5 not important at all"; see Schneider et al., 2017).

they account for the differences in the effect of information on returns of a university degree on educational aspirations.

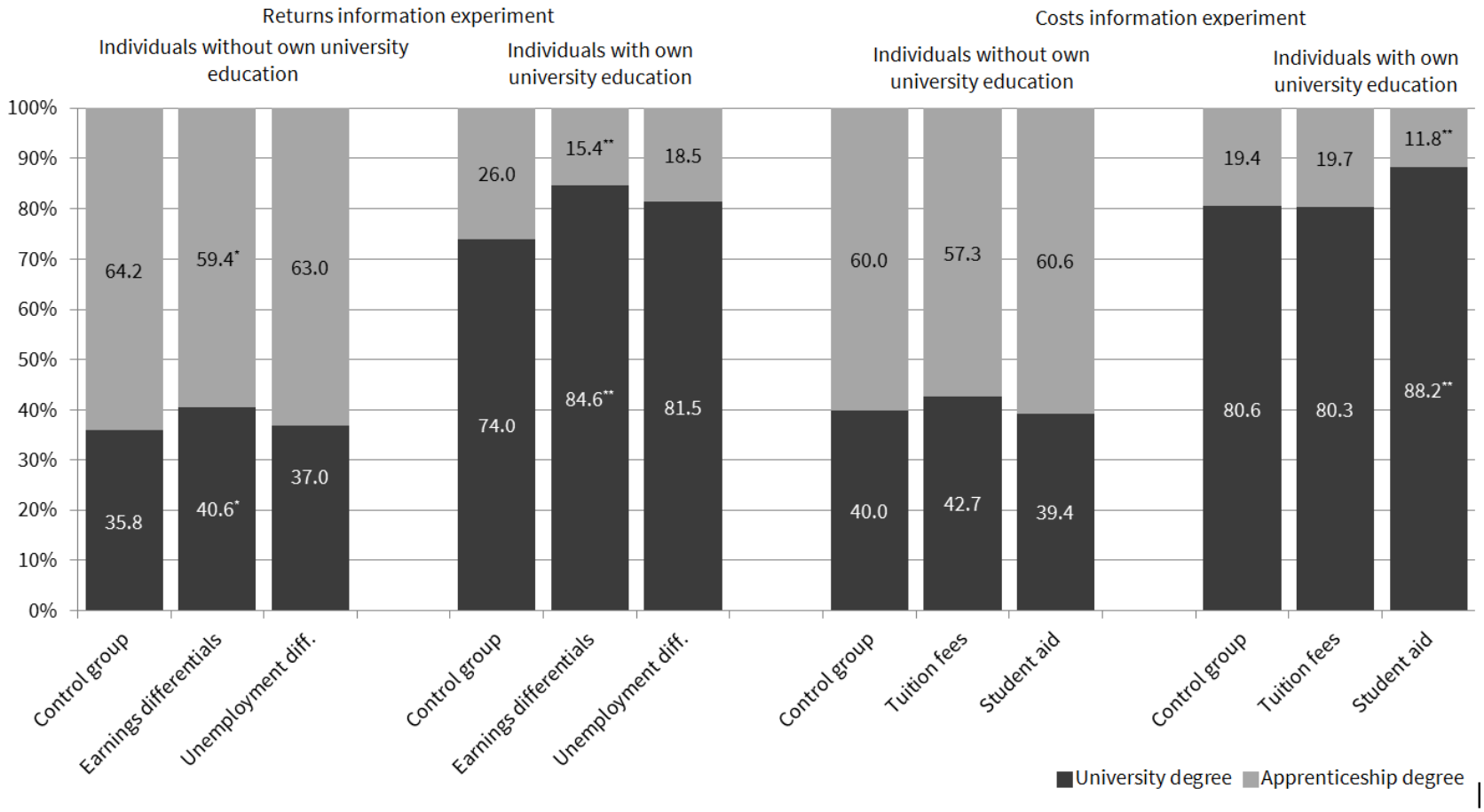
Independent of the exact reasons for why information provision does not close the educational aspirations gap in a representative German sample, our results have important implications for our understanding of the mechanisms of the intergenerational persistence of educational attainment. In contrast to earlier work mostly based on data from the United States, we show that providing information on university returns and costs is not sufficient for aligning the aspirations of those with and without university backgrounds. Thus, the large and persistent inequalities in university access by parental education in Germany do not seem to be due to a market failure induced by asymmetric information regarding pecuniary consequences of educational choices. This is consistent with the literature emphasizing the importance of non-pecuniary reasons for educational choices (e.g., Beffy et al., 2012; Wiswall and Zafar, 2015b). One such non-pecuniary reason might be the identity of parents and their children: Parents without a university degree might not aspire to university education for their children because university studies might lead to an alienation of the children from family identities (Akerlof and Kranton, 2002). Similarly, educational aspiration gaps might emerge from differences in the expected consumption value of university education or its cognitive costs (Belfield et al., 2016). We consider investigation of the empirical relevance of these non-pecuniary explanations for the educational aspiration gap an important area for future research.

Figure 5.1: The educational aspiration gap: Adults' aspiration for the education of their child



Notes: Response to the question, "Irrespective of whether you have any children and of which educational degree your child holds or is likely to attain in the future: Which educational degree would match your personal ideal conception for your child?" Control group, weighted means. Data source: ifo Education Survey 2016 and 2017.

Figure 5.2: Effects of information experiments on adults' aspiration for the education of their child



Notes: Effects of random information provision about earnings differentials, unemployment differentials, tuition fees, and student aid, respectively, on respondents' ideal educational degree for her child. See column 3 of Tables 5.3 and 5.4 for underlying regression estimates. Significance levels of difference from respective control group: ** p<0.05, * p<0.1. Data source: ifo Education Survey 2016 and 2017.

Table 5.1: Summary statistics of background variables and balancing tests

	Mean	Covariates predicting treatment status for experiment with information on				
	[SD]	Earnings differential	Unemployment differential	Tuition fees	Student aid	Tuition fees and student aid
	(1)	(2)	(3)	(4)	(5)	(6)
Age	50.5	0.001	0.001	-0.001	-0.001*	-0.001*
	[18.7]	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female	0.513	-0.021	0.016	-0.016	-0.011	-0.019
		(0.025)	(0.026)	(0.026)	(0.026)	(0.026)
Monthly household income (in €)	2221.4	-0.000	-0.000	-0.000	-0.000	-0.000
	[1392.0]	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Education						
No degree	0.123	-0.030	-0.054	-0.048	-0.015	0.024
		(0.043)	(0.046)	(0.046)	(0.043)	(0.043)
Apprenticeship degree	0.684	0.050*	0.042	0.066**	0.013	0.027
		(0.029)	(0.030)	(0.028)	(0.027)	(0.028)
University degree	0.193	-0.050	-0.020	-0.058*	-0.008	-0.054*
		(0.034)	(0.035)	(0.030)	(0.030)	(0.030)
Employment status						
Student	0.090	-0.078	-0.035	0.045	0.072*	0.056
		(0.058)	(0.060)	(0.043)	(0.042)	(0.044)
Active	0.503	-0.008	-0.021	0.022	0.007	0.044*
		(0.025)	(0.026)	(0.026)	(0.026)	(0.026)
Not active	0.408	0.033	0.034	-0.037	-0.032	-0.065**
		(0.025)	(0.026)	(0.027)	(0.027)	(0.027)
Born in Germany	0.948	0.061	-0.037	-0.005	-0.056	0.047
		(0.060)	(0.057)	(0.063)	(0.060)	(0.061)
Living in West Germany	0.800	-0.001	0.036	0.018	0.004	0.012
		(0.031)	(0.032)	(0.031)	(0.030)	(0.031)
Municipality size (7-point scale)	4.330	-0.019***	-0.008	0.000	0.002	-0.001
	[1.770]	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)

(continued on next page)

Table 5.1 (continued)

	Mean	Covariates predicting treatment status for experiment with information on				
	[SD]	Earnings differential	Unemployment differential	Tuition fees	Student aid	Tuition fees and student aid
	(1)	(2)	(3)	(4)	(5)	(6)
Partner in household	0.549	0.010	0.023	-0.029	0.008	-0.017
		(0.026)	(0.026)	(0.026)	(0.026)	(0.027)
Has children	0.588	0.020	0.024	-0.024	-0.022	-0.027
		(0.026)	(0.027)	(0.027)	(0.026)	(0.027)
Parent of child currently in school	0.283	0.007	-0.006	0.003	-0.005	0.003
		(0.028)	(0.028)	(0.029)	(0.029)	(0.029)
Offline	0.182	0.020	0.051	-0.035	-0.047	-0.057
		(0.037)	(0.036)	(0.041)	(0.040)	(0.042)
Risk tolerance (11-point scale)	4.230	0.002	0.000	-0.002	0.001	0.002
	[2.509]	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Patience (11-point scale)	5.978	-0.002	0.002	-0.001	0.011**	0.001
	[2.487]	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Item non-response on aspiration for child	0.023	0.062	0.083	-0.106	0.002	-0.070
		(0.083)	(0.082)	(0.152)	(0.146)	(0.145)
F test for joint significance (*p* value)		0.499	0.372	0.204	0.903	0.309
Observations	7,270	2,701	2,616	2,001	2,051	1,996

Notes: First column: sample means; standard deviations in brackets (for non-binary variables). Subsequent columns: Each cell reports the coefficients from estimating equation (4) for the respective experiment (standard errors in parentheses). *p*- values of *F* tests for joint significance are based on regressions of treatment status on all covariates jointly. Regressions weighted by survey weights. Municipality size is measured on a scale from 1 (population < 2,000) to 7 (population > 500,000). Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2016 and 2017.

Table 5.2: Differences in beliefs about returns and costs of university education by respondents' own education level

Panel I: Beliefs on earnings and unemployment differentials

	Beliefs on		Certainty of beliefs on	
	Earnings differentials	Unemployment differentials	Earnings differentials	Unemployment differentials
	(1)	(2)	(3)	(4)
No university education	-0.043***	-1.328***	-0.217***	-0.153**
	(0.016)	(0.366)	(0.080)	(0.077)
Constant	-0.010	-2.809***	3.808***	3.497***
	(0.014)	(0.339)	(0.073)	(0.070)
Observations	3,106	3,096	3,205	3,185
R^2	0.0040	0.0076	0.0034	0.0018

Panel II: Beliefs on tuition fees and student aid

	Beliefs on		Certainty of beliefs on	
	Tuition fees	Student aid	Tuition fees	Student aid
	(1)	(2)	(3)	(4)
No university education	0.753***	-0.044**	-1.407***	-0.638***
	(0.123)	(0.018)	(0.084)	(0.071)
Constant	2.056***	-0.615***	4.700***	3.569***
	(0.095)	(0.016)	(0.073)	(0.062)
Observations	3,762	3,782	3,835	3,838
R^2	0.0085	0.0023	0.0874	0.0252

Notes: OLS regressions. No university education: dummy equal to 1 if respondent does not hold a university degree. Dependent variable: columns (1)-(2): beliefs as indicated in the column header, expressed as difference from the correct value, divided by the correct value (tuition fees: divided by 100 Euro); columns (3)-(4): certainty that belief is close to correct on 7-point Likert scale. Top and bottom 2 percent of the belief distribution trimmed in the belief samples. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2016 and 2017.

Table 5.3: Effects of information about returns to university education on educational aspiration

	Aspiration for child: University degree			
	(1)	(2)	(3)	(4)
Information on earnings differentials	0.047*	0.059**	0.106**	0.079
	(0.025)	(0.024)	(0.047)	(0.049)
Information on unemployment differentials	0.019	0.018	0.075	0.084
	(0.026)	(0.025)	(0.053)	(0.054)
No university education			-0.382***	-0.306***
			(0.042)	(0.046)
Information on earnings differentials x No university education			-0.058	-0.021
			(0.054)	(0.055)
Information on unemployment differentials x No university education			-0.063	-0.082
			(0.060)	(0.060)
Control mean	0.433		0.740	
Covariates	No	Yes	No	Yes
Observations	3,229	3,128	3,223	3,128
R^2	0.0015	0.0836	0.1085	0.1396
Effect of information on earnings differentials for "No university education"			0.048*	0.058**
Effect of information on unemployment differentials for "No university education"			0.012	0.002

Notes: OLS regressions. Information was provided to a random subgroup of respondents. Dependent variable: dummy variable coded 1 if respondent prefers a university degree as ideal educational outcome for her child. No university education: dummy equal to 1 if respondent does not hold a university degree. Covariates: age, gender, income, employment status, born in Germany, living in West Germany, municipality size, living with a partner, parent status, risk tolerance, and patience. Bottom rows show estimates of Wald tests for H_0: $\beta_1 + \beta_3 = 0$ based on equation (2). Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2016.

Table 5.4: Effects of information about costs of university education on educational aspiration

	Aspiration for child: University degree			
	(1)	(2)	(3)	(4)
Information on tuition fees	0.006	0.002	-0.003	-0.016
	(0.026)	(0.025)	(0.042)	(0.040)
Information on student aid	0.008	0.004	0.076**	0.067*
	(0.026)	(0.025)	(0.037)	(0.036)
Information on both	-0.013	-0.020	0.027	0.024
	(0.026)	(0.025)	(0.040)	(0.039)
No university education			-0.406***	-0.325***
			(0.034)	(0.035)
Information on tuition fees x No university education			0.030	0.040
			(0.051)	(0.050)
Information on student aid x No university education			-0.082*	-0.069
			(0.047)	(0.046)
Information on both x No university education			-0.033	-0.036
			(0.050)	(0.048)
Control mean	0.493		0.806	
Covariates	No	Yes	No	Yes
Observations	3,939	3,848	3,934	3,847
R^2	0.0003	0.0963	0.1216	0.1579
Effect of information on tuition fees for “No university education”			0.027	0.024
Effect of information on student aid for “No university education”			-0.006	-0.003
Effect of information on both for “No university education”			-0.006	-0.012

Notes: OLS regressions. Information was provided to a random subgroup of respondents. Dependent variable: dummy variable coded 1 if respondent prefers a university degree as ideal educational outcome for her child. No university education: dummy equal to 1 if respondent does not hold a university degree. Covariates: age, gender, income, employment status, born in Germany, living in West Germany, municipality size, living with a partner, parent status, risk tolerance, and patience. Bottom rows show estimates of Wald tests for H_0: $\beta_1 + \beta_3 = 0$ based on equation (2). Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2017.

Table 5.5: Effects of information provision in the main survey on beliefs about costs of university education in the follow-up

	Beliefs in follow-up survey on				Certainty of beliefs in follow-up survey			
	Tuition fees		Student aid		Tuition fees		Student aid	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information on tuition fees	-0.964	-2.214***	0.028	-0.034	0.333***	0.372**	0.125	-0.044
	(0.723)	(0.838)	(0.072)	(0.097)	(0.092)	(0.176)	(0.079)	(0.146)
Information on student aid	-0.282	-0.966	0.167**	0.136	0.074	0.026	0.322***	0.426***
	(0.671)	(0.809)	(0.072)	(0.104)	(0.085)	(0.163)	(0.081)	(0.161)
Information on both	-1.939***	-1.513	0.140**	0.103	0.293***	0.066	0.241***	0.309**
	(0.639)	(1.020)	(0.069)	(0.093)	(0.092)	(0.178)	(0.081)	(0.153)
No university education		2.545***		0.064		-0.623***		-0.471***
		(0.884)		(0.104)		(0.126)		(0.115)
Information on tuition fees x No university education		1.469		0.077		-0.010		0.253
		(1.238)		(0.132)		(0.205)		(0.173)
Information on student aid x No university education		0.741		0.038		0.106		-0.106
		(1.172)		(0.136)		(0.190)		(0.186)
Information on both x No university education		-0.704		0.044		0.334		-0.057
		(1.286)		(0.126)		(0.207)		(0.179)
Dependent variable in main survey	0.550***	0.531***	0.263***	0.262***	0.537***	0.502***	0.435***	0.415***
	(0.072)	(0.073)	(0.080)	(0.080)	(0.017)	(0.019)	(0.019)	(0.020)
Constant	5.022***	3.248***	-0.204***	-0.250***	1.637***	2.215***	1.555***	1.956***
	(0.492)	(0.675)	(0.068)	(0.095)	(0.084)	(0.135)	(0.075)	(0.116)
Observations	2,293	2,293	2,295	2,295	2,289	2,289	2,290	2,290
R^2	0.1078	0.1185	0.0434	0.0447	0.3130	0.3267	0.2186	0.2344
Effect of information on tuition fees for "No university education"		-0.745		0.043		0.362***		0.209**
Effect of information on student aid for "No university education"		-0.225		0.173*		0.131		0.320***
Effect of information on both for "No university education"		-2.217***		0.147*		0.400***		0.252***

Notes: OLS regressions. Dependent variables recorded in follow-up survey conducted about two weeks after the main survey (median interval: 12 days). Information was provided to a random subgroup of respondents in the main survey. No university education: dummy equal to 1 if respondent does not hold a university degree. Dependent variable: columns (1)-(4): beliefs as indicated in the column header, expressed as difference from the correct value, divided by the correct value (tuition fees: divided by 100 Euro); columns (5)-(8): certainty that belief is close to correct on 7-point Likert scale. Bottom rows show estimates of Wald tests for H_0: $\beta_1 + \beta_3 = 0$ based on equation (2). Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2017.

Table 5.6: Prior beliefs on returns to university education and the educational aspiration gap

	Aspiration for child: University degree		
	(1)	(2)	(3)
No university education	-0.382***	-0.361***	-0.361***
	(0.042)	(0.041)	(0.041)
Beliefs on earnings differentials		0.268***	0.247***
		(0.062)	(0.062)
Beliefs on unemployment differentials		0.006**	0.006**
		(0.003)	(0.003)
Certainty of beliefs on earnings differentials			0.008
			(0.018)
Certainty of beliefs on unemployment differentials			0.023
			(0.018)
Constant	0.740***	0.771***	0.664***
	(0.037)	(0.037)	(0.060)
Observations	1,036	966	966
R^2	0.0936	0.1216	0.1285

Notes: OLS regressions. Control group only. No university education: dummy equal to 1 if respondent does not hold a university degree. Beliefs on earnings and unemployment differentials: expressed as difference from the correct value, divided by the correct value. Certainty: certainty that belief is close to correct on 7-point Likert scale. Dependent variable: dummy variable coded 1 if respondent prefers a university degree as ideal educational outcome for her child. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2016.

Table 5.7: Prior beliefs on costs of university education and the educational aspiration gap

	Aspiration for child: University degree		
	(1)	(2)	(3)
No university education	-0.406***	-0.404***	-0.402***
	(0.034)	(0.035)	(0.037)
Beliefs on tuition fees		0.008	0.008
		(0.006)	(0.006)
Beliefs on student aid		0.079*	0.085*
		(0.046)	(0.046)
Certainty of beliefs on tuition fees			-0.011
			(0.011)
Certainty of beliefs on student aid			0.022*
			(0.012)
Constant	0.806***	0.832***	0.808***
	(0.027)	(0.040)	(0.063)
Observations	1,031	963	962
R^2	0.1161	0.1307	0.1346

Notes: OLS regressions. Control group only. No university education: dummy equal to 1 if respondent does not hold a university degree. Beliefs on tuition fees and student aid: expressed as difference from the correct value, divided by the correct value (tuition fees: divided by 100 Euro). Certainty: certainty that belief is close to correct on 7-point Likert scale. Dependent variable: dummy variable coded 1 if respondent prefers a university degree as ideal educational for her child. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2017.

Table 5.8: Difference in behavioral traits by respondents' education level

	Patience	Risk tolerance	Overconfidence
	(1)	(2)	(3)
No university education	-0.712***	-0.776***	-0.516***
	(0.081)	(0.085)	(0.055)
Observations	7,214	7,236	6,775
R^2	0.0129	0.0153	0.0177

Notes: OLS regressions. No university education: dummy equal to 1 if respondent does not hold a university degree. Includes wave fixed effects. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2016 and 2017.

Table 5.9: Behavioral traits and the educational aspiration gap

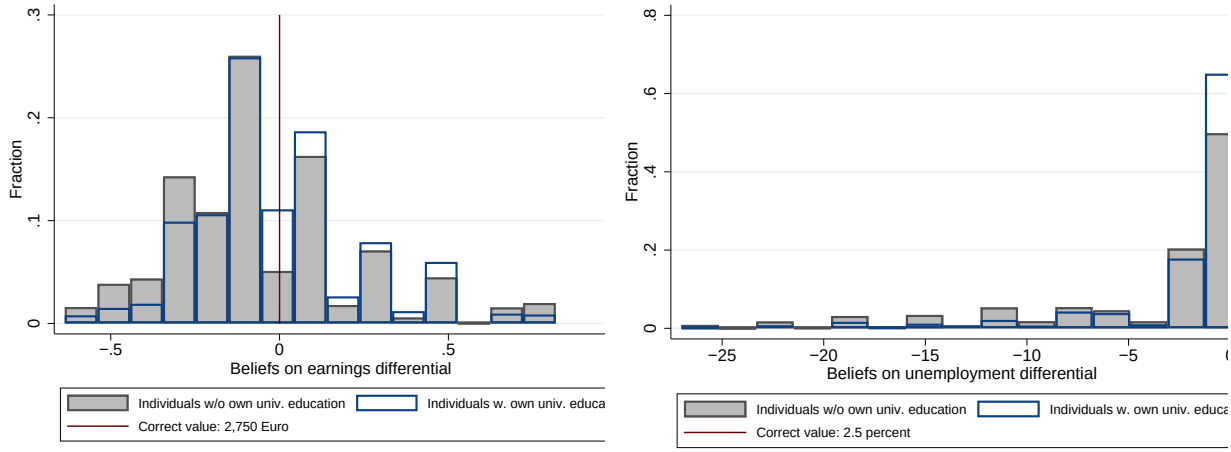
	Aspiration for child: University degree				
	(1)	(2)	(3)	(4)	(5)
No university education	-0.397***	-0.385***	-0.393***	-0.389***	-0.385***
	(0.027)	(0.027)	(0.027)	(0.028)	(0.028)
Patience		0.011**			0.014***
		(0.005)			(0.005)
Risk tolerance			0.000		-0.006
			(0.005)		(0.005)
Overconfidence				0.010	0.010
				(0.007)	(0.008)
Constant	0.776***	0.677***	0.747***	0.756***	0.694***
	(0.022)	(0.044)	(0.036)	(0.028)	(0.048)
Observations	2,067	2,061	2,065	1,940	1,940
R^2	0.1060	0.1102	0.1070	0.1116	0.1160

Notes: OLS regressions. Control group only. No university education: dummy equal to 1 if respondent does not hold a university degree. Dependent variable: dummy variable coded 1 if respondent prefers a university degree as ideal educational outcome for her child. Includes wave fixed effects. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2016 and 2017.

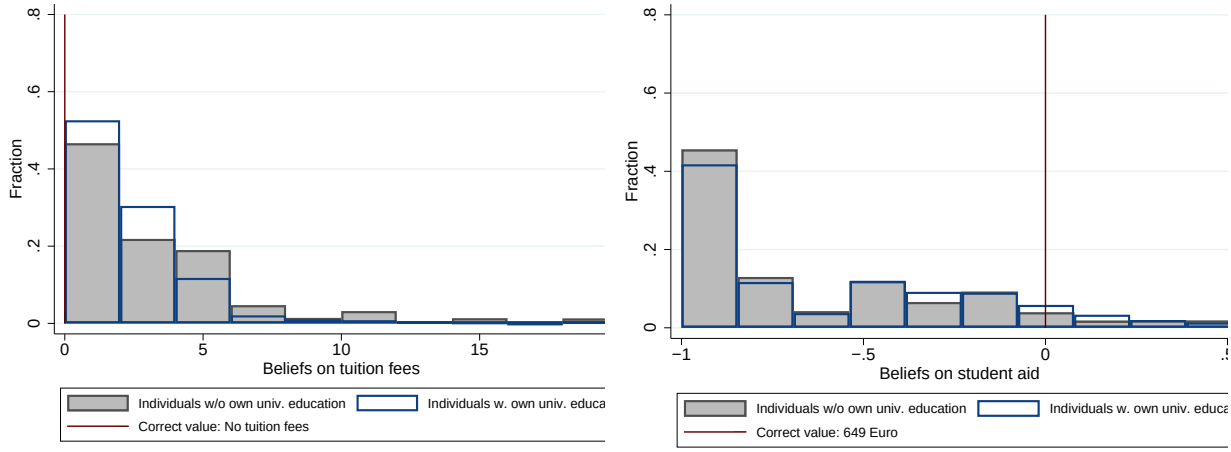
Appendix

Figure A5.1: Distributions of beliefs about returns and costs of university education by respondents' own education level

Panel I: Beliefs on earnings and unemployment differentials



Panel II: Beliefs on tuition fees and student aid



Notes: Distribution of beliefs about returns (Panel I) and costs (Panel II) of university education for respondents who do and do not hold a university degree, and correct values. Beliefs as indicated in respective caption. Top and bottom 2 percent of the belief distributions trimmed. Weighted distributions. Data source: ifo Education Survey 2016 and 2017.

Table A5.1: Wording of survey questions

Wave	No.	Group	Wording	Answer categories
2016	6	All	Those holding an apprenticeship degree (apprenticeship) currently earn a monthly average of 1,850 Euro after taxes (full-time position). What is your best guess, how much do the following groups with lower/higher education attainment earn on average? - Those without a professional degree - University graduates	Two answers in Euro per month (after taxes), open-ended
2016	7	All	The unemployment rate of those holding an apprenticeship degree (apprenticeship) is currently about 5 percent. What is your best guess, what is the unemployment rate for the following groups with lower/higher education attainment? - Those without a professional degree - University graduates	Two answers in percent, open-ended
2017	7	All	What is your best guess, how much do students in your state generally pay in tuition fees at the moment? (Enter a value of "0" if you guess that students in your state do not generally have to pay tuition fees at the moment.)	In Euro per semester (half year), open-ended
2017	8	All	What is your best guess, how much public student aid (BAföG) are students generally eligible for whose parents earn 50,000 Euro before tax per year? Think of students who have two non-working siblings, do no longer live with their parents, and are covered by their family's health insurance. (Enter a value of "0" if you guess these students do not receive BAföG.)	In Euro per month, open-ended
2016/ 2017	23/ 21	Control	Irrespective of whether you have any children and of which educational degree your child holds or is likely to attain in the future: Which educational degree would match your personal ideal conception for your child?	Single choice: Apprenticeship degree (apprenticeship), University degree
2016	23	Treatment "Earnings differential"	Those without professional degree earn a monthly average of about 1,400 Euro after taxes, those holding an apprenticeship degree (apprenticeship) about 1,850 Euro and university graduates about 2,750 Euro. Irrespective of whether … [see Control]	[see Control]
2016	23	Treatment "Unemployment differential"	The unemployment rate of those without a professional degree is currently 20 percent, that of those holding an apprenticeship degree (apprenticeship) is about 5 percent and for university graduates the unemployment rate is about 2.5 percent. Irrespective of whether … [see Control]	[see Control]

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Table A5.1 (continued)

Wave	No.	Group	Wording	Answer categories
2017	21	Treatment "Tuition fees"	Currently, students in all of Germany do not have to pay tuition fees. Irrespective of whether … [see Control]	[see Control]
2017	21	Treatment "Student aid"	In Germany, comprehensive public student aid (BAföG) is available, only half of which has to be paid back later at most. For example, students with two non-working siblings whose parents earn 50,000 Euro before tax per year at most are generally eligible for 649 Euro per month. Irrespective of whether … [see Control]	[see Control]
2017	21	Treatment "Both"	Currently, students in all of Germany do not have to pay tuition fees. In Germany, comprehensive student aid, which has to be paid back in half at most, is available. For example, a student with two non-working siblings, whose parents pre-tax earnings do not exceed 50,000 Euro per year, is generally eligible for payments of 649 Euro per month. Irrespective of whether … [see Control]	[see Control]

Notes: No. refers to position of the question in the respective survey.

Table A5.2: Effects of return information on educational aspiration: Parents

	Aspiration for child: University degree			
	All respond-ents	Parents	All respond-ents	Parents
	(1)	(2)	(3)	(4)
Information on earnings differentials	0.047*	0.066	0.106**	0.196**
	(0.025)	(0.046)	(0.047)	(0.089)
Information on unemployment differentials	0.019	0.066	0.075	0.115
	(0.026)	(0.046)	(0.053)	(0.098)
No university education			-0.382***	-0.277***
			(0.042)	(0.087)
Information on earnings differentials x No university education			-0.058	-0.158
			(0.054)	(0.102)
Information on unemployment differentials x No university education			-0.063	-0.063
			(0.060)	(0.110)
Control mean	0.433	0.454	0.740	0.690
Observations	3,229	920	3,223	920
R^2	0.0015	0.0038	0.1085	0.0721
Effect of earnings information for "No university education"			0.048*	0.039
Effect of unemployment information for "No university education"			0.012	0.053

Notes: OLS regressions. Sample restriction for parents includes only respondents who state that at least one of either their oldest or youngest child is still in formal education. Information was provided to a random subgroup of respondents. Dependent variable: dummy variable coded 1 if respondent prefers a university degree as ideal educational outcome for her child. No university education: dummy equal to 1 if respondent does not hold a university degree. Bottom rows show estimates of Wald tests for H_0: $\beta_1 + \beta_3 = 0$ based on equation (2). Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2016.

Table A5.3: Effects of cost information on educational aspiration: Parents

	Aspiration for child: University degree			
	All respond-ents	Parents	All respond-ents	Parents
	(1)	(2)	(3)	(4)
Information on tuition fees	0.006	0.002	-0.003	-0.016
	(0.026)	(0.047)	(0.042)	(0.074)
Information on student aid	0.008	0.057	0.076**	0.122**
	(0.026)	(0.048)	(0.037)	(0.049)
Information on both	-0.013	-0.024	0.027	-0.078
	(0.026)	(0.048)	(0.040)	(0.091)
No university education			-0.406***	-0.448***
			(0.034)	(0.058)
Information on tuition fees x No university education			0.030	0.054
			(0.051)	(0.090)
Information on student aid x No university education			-0.082*	-0.046
			(0.047)	(0.072)
Information on both x No university education			-0.033	0.098
			(0.050)	(0.105)
Control mean	0.493	0.507	0.806	0.858
Observations	3,939	1,058	3,934	1,057
R^2	0.0003	0.0035	0.1216	0.1086
Effect of information on tuition fees for "No university education"			0.027	0.038
Effect of information on student aid for "No university education"			-0.006	0.076
Effect of information on both for "No university education"			-0.006	0.020

Notes: OLS regressions. Sample restriction for parents includes only respondents who state that at least one of either their oldest or youngest child is still in formal education. Information was provided to a random subgroup of respondents. Dependent variable: dummy variable coded 1 if respondent prefers a university degree as ideal educational outcome for her child. No university education: dummy equal to 1 if respondent does not hold a university degree. Bottom rows show estimates of Wald tests for H_0: $\beta_1 + \beta_3 = 0$ based on equation (2). Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2017.

Table A5.4: Aspiration vs. expected likelihood of obtaining a university degree

	Aspiration for child: University degree		
	(1)	(2)	(3)
No university education	-0.194***	-0.082**	-0.071**
	(0.034)	(0.034)	(0.033)
Subjective likelihood that child earns a university degree			
Continuous measure		0.166***	
		(0.010)	
Dummy: unlikely			-0.171***
			(0.036)
Dummy: likely			0.313***
			(0.033)
Constant	0.738***	0.119**	0.550***
	(0.030)	(0.051)	(0.040)
Observations	2,258	2,258	2,258
R^2	0.0247	0.1834	0.1913

Notes: OLS regressions. Sample: parents of children who did not yet complete their educational career (2015 survey). Dependent variable: dummy variable coded 1 if respondent states that she would consider a university degree the ideal educational outcome for her child (by selecting "4" or "5" on a 5-point Likert scale). No university education: dummy equal to 1 if respondent does not hold a university degree. Subjective likelihood that child earns a university degree is recorded on a 5-point Likert scale from "1 impossible" to "5 absolutely certain". Dummy "unlikely" is coded 1 if respondents answer 1 or 2 on the 5-point scale. Dummy "likely" is coded 1 if respondents answer 4 or 5 on the 5-point scale. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2015.

Table A5.5: Prediction of participation in the follow-up survey

	Participation in follow-up survey	
Treatment status in the main survey		
Information on tuition fees	-0.001	(0.022)
Information on student aid	-0.031	(0.022)
Information on both	-0.032	(0.022)
Covariates		
Age	-0.001	(0.001)
Female	-0.068***	(0.017)
Monthly household income	0.000*	(0.000)
Education (baseline: no degree)		
Apprenticeship degree	0.062**	(0.028)
University degree	0.054	(0.033)
Employment status (baseline: student)		
Active	0.133***	(0.031)
Not active	0.103***	(0.037)
Born in Germany	0.061*	(0.038)
Living in West Germany	0.036*	(0.019)
Municipality size	0.006	(0.005)
Partner in household	0.015	(0.018)
Has children	-0.000	(0.020)
Risk tolerance	-0.010***	(0.003)
Patience	0.014***	(0.003)
Constant	0.321***	(0.068)
Observations	3,866	
R^2	0.0255	

Notes: OLS regressions. Dependent variable: dummy variable coded 1 if respondent participated in the follow-up survey. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2017.

Table A5.6: Summary statistics and balancing tests: Follow-up survey

	Mean	Covariates predicting treatment status in main-survey experiment with information on		
	[SD]	Tuition fees	Student aid	Tuition fees and student aid
	(1)	(2)	(3)	(4)
Age	48.1	-0.000	-0.001	-0.002*
	[15.3]	(0.001)	(0.001)	(0.001)
Female	0.475	-0.028	0.010	-0.018
		(0.033)	(0.033)	(0.033)
Monthly household income	2396.3	-0.000*	-0.000*	-0.000
	[1466.4]	(0.000)	(0.000)	(0.000)
Education				
No degree	0.079	0.054	0.068	0.031
		(0.060)	(0.059)	(0.060)
Apprenticeship degree	0.579	0.021	-0.009	0.051
		(0.033)	(0.033)	(0.034)
Academic degree	0.343	-0.039	-0.012	-0.065*
		(0.035)	(0.034)	(0.035)
Employment status				
Student	0.100	-0.030	0.049	0.057
		(0.062)	(0.058)	(0.057)
Active	0.686	0.069**	0.035	0.036
		(0.035)	(0.035)	(0.035)
Not active	0.214	-0.071*	-0.070*	-0.076**
		(0.038)	(0.038)	(0.038)
Born in Germany	0.959	0.077	0.140*	0.050
		(0.081)	(0.084)	(0.077)
Living in West Germany	0.811	-0.038	-0.016	-0.010
		(0.039)	(0.040)	(0.039)
Municipality size	4.343	-0.010	-0.005	-0.020**
	[1.774]	(0.009)	(0.009)	(0.009)

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Table A5.6 (continued)

	Mean	Covariates predicting treatment status in main-survey experiment with information on		
	[SD]	Tuition fees	Student aid	Tuition fees and student aid
	(1)	(2)	(3)	(4)
Partner in household	0.585	-0.059*	-0.040	-0.053
		(0.033)	(0.033)	(0.034)
Has children	0.561	0.001	-0.016	-0.049
		(0.033)	(0.033)	(0.033)
Parent of child currently in school	0.326	-0.010	0.006	-0.039
		(0.036)	(0.036)	(0.037)
Risk tolerance	4.245	-0.002	0.001	-0.002
	[2.519]	(0.007)	(0.006)	(0.007)
Patience	6.176	-0.004	0.011	0.004
	[2.384]	(0.007)	(0.007)	(0.007)
F test for joint significance (*p* value)		0.490	0.248	0.208
Observations	2,300	1,184	1,189	1,157

Notes: Follow-up survey. First column: sample means; standard deviations in brackets (for non-binary variables). Subsequent columns: Each cell reports the coefficients from estimating equation (4) for the respective experiment (standard errors in parentheses). *p* values of *F* tests for joint significance are based on regressions of treatment status on all covariates jointly. Regressions weighted by survey weights. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2017.

Table A5.7: Effects of cost information on educational aspiration: Follow-up survey

	Aspiration for child: University degree	
	(1)	(2)
Information on tuition fees	-0.000	-0.011
	(0.029)	(0.048)
Information on student aid	-0.036	0.055
	(0.029)	(0.044)
Information on both	-0.004	0.059
	(0.029)	(0.045)
No university education		-0.339***
		(0.039)
Information on tuition fees x No university education		0.036
		(0.059)
Information on student aid x No university education		-0.100*
		(0.056)
Information on both x No university education		-0.061
		(0.056)
Control mean	0.543	0.788
Observations	2,300	2,300
R^2	0.0009	0.1029
Effect of information on tuition fees for "No university education"		0.024
Effect of information on student aid for "No university education"		-0.045
Effect of information on both for "No university education"		-0.002

Notes: OLS regressions. Information was provided to a random subgroup of respondents in the main survey. Dependent variable: dummy variable coded 1 if respondent prefers a university degree as ideal educational outcome for her child. No university education: dummy equal to 1 if respondent does not hold a university degree. Bottom rows show estimates of Wald tests for H_0: $\beta_1 + \beta_3 = 0$ based on equation (2). Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Data source: ifo Education Survey 2017.

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