Reducing Inequality through Correcting Misperceptions: Experimental Evidence on Student Aid Take-Up*

Sebastian Riedmiller†

February 24, 2025

Abstract

Financial student aid reduces social inequality and improves educational and economic outcomes, yet a persistent gap exists between take-up and eligibility. To investigate this gap and the means to close it, I conducted an experiment with 6,225 non-receivers of student aid embedded in a survey of 22,222 university students across Germany. Using hypothetical scenarios, I find that 63% of non-receivers systematically underestimate the financial value of student aid, and 86% misperceive their eligibility. Concise information about student aid and individual eligibility increases take-up by 1.1 pp (43%), especially among disadvantaged students. Correcting misperceptions causally increases take-up by up to 55 pp. After take-up, students have a higher total income despite reducing their earned income and financial support from their parents. The findings suggest that correcting misperceptions through concise information can reduce social inequality by alleviating financial concerns among disadvantaged students and their parents.

Keywords: misperceptions, student aid applications, field experiment, information intervention, social benefits, BAföG

JEL Codes: C93, D14, D83, D90, H52, I22

*I thank Eric Bettinger, Matthew Brown, Brendan Moore, Manuela Puente Beccar, Johannes Rottmann, Sebastian O. Schneider, Matthias Sutter, participants at the AFE London, VfS Annual Conference Berlin, BABEEW at UC Davis, Ca' Foscari/EEG workshop in Venice, and seminar participants at Stanford University, IZA Bonn, Leibniz University Hannover, and the University of Cologne for helpful comments and suggestions. I thank Michael Seebauer for his support and coordination in conducting the online survey, Sascha Strobl for his support in designing the survey, and Jannis Bernschein, Alexander Böcking, Leonie Matejko, and Theresa Schaetze for their excellent research assistance. This study was approved by the IRB of the University of Cologne (230011SR) and preregistered with the AEA RCT Registry (AEARCTR-0011249). Financial support by the Joachim Herz Foundation is gratefully acknowledged.

†Max Planck Institute for Research on Collective Goods, Bonn, and University of Cologne. Contact: riedmiller@coll.mpg.de

1 Introduction

Education is a crucial driver for economic growth (Hanushek & Woessmann, 2015), yet social inequality remains a major inhibitor to accessing it. In the US, children from families in the bottom income quintile are 77 times less likely to attend elite colleges than children from the top 1% (Chetty et al., 2020). Part of the problem are the costs of higher education, which are harder to bear for low-income families. While student aid programs exist to tackle this inequality and help covering the cost, many students do not take up their entitlement (Bettinger et al., 2012; Castleman & Page, 2016; Kofoed, 2017; Bird et al., 2021). Previous work shows that information provision about possible aid amounts or the application is often insufficient to increase take-up (Bettinger et al., 2012; Booij, Leuven & Oosterbeek, 2012; Marx & Turner, 2020; Bird et al., 2021), while assisting students with their Free Application for Federal Student Aid (FAFSA) has been found to be more effective (Bettinger et al., 2012; Hoxby & Turner, 2015; Castleman & Page, 2016; Dynarski et al., 2021). One important reason for a gap between students' eligibility for student aid and their actual take-up rates may be systematic misperceptions about eligibility and repayment conditions of means-tested student aid. If these misperceptions prevent students from taking up student aid, this may negatively affect their study pace, performance, graduation rates, and labor market income (see Dynarski, Page & Scott-Clayton, 2023, for an overview).

In this paper, I examine misperceptions as an important potential channel for low take-up rates of student aid and how these misperceptions can be corrected in a randomized controlled trial (RCT). I conducted an online field experiment with 6,225 students who did not receive student aid and were enrolled at universities across Germany, embedded into a survey distributed to 22,222 students. Germany has only one need-based federal student aid program that is not additionally merit-based, the BAföG.[1] With approximately €2.9 billion per year for about 360,000 students, it is also the most extensive student aid program in Germany (Destatis, 2024). Yet, the problems are similar to the FAFSA in the USA as at least 40% of eligible students do not take up BAföG (Herber & Kalinowski, 2019). Since there is no student aid program other than BAföG, the German setting allows me to focus on this program alone to determine at a national level whether students have systematic misperceptions about student aid and their eligibility, and whether correcting these misperceptions increases take-up.

The experiment consists of three waves over one year. In the first wave, I measure perceptions about eligibility and repayment conditions of federal student aid through hypothetical scenarios in three areas. Each scenario describes a short case of a student aid receiver with

[1] Abbreviation for *Bundesausbildungsförderungsgesetz*, which is used as a term for federal student aid.

the necessary information to assess (i) how much money they receive per month, (ii) how much their parents earn for a given amount of student aid, and (iii) how much they have to repay. This allows me to understand how well the students perceive the conditions of federal student aid and if they are systematically wrong in their perceptions. Additionally, students were asked if they believed they were eligible for student aid. Calculating the students' entitlement based on their sociodemographic and economic situation, I can measure whether students misperceived their eligibility. At the end of the survey, a stratified information intervention addresses these conditions and informs students about their individual entitlement to resolve potential misperceptions. This is my treatment group. In the second wave six months later, I elicited misperceptions again and asked students if they took up student aid. Due to a lag in application acceptance, I contacted the students for a third wave another six months later to elicit if pending applications had been successful. Using these waves, I can measure the causal effect of the intervention on misperceptions and take-up rates.

I find that students have systematic misperceptions about student aid conditions in all three areas. On average, they (i) underestimate the amount of student aid by €265 per month, (ii) underestimate the income thresholds for parents by €15,414 per year, and (iii) overestimate the repayment amounts by €2,827. In total, 99.2% have at least one of these misperceptions. Additionally, 63.1% show all three of these misperceptions simultaneously and, therefore, systematically underestimate the financial value of student aid. Among the students classified as eligible for student aid, 86% do not believe they are eligible. The information intervention corrects misperceptions about the conditions by 5.8 percentage points (pp) (32%) and about eligibility by 6 pp (59%). Additionally, the intervention increases take-up by 1.1 pp, or 47%. Correcting misperceptions completely causes an increase in take-up by up to 55 pp.

To analyze heterogeneities in the intervention effect, I use causal random forest estimation (Wager & Athey, 2018; Athey, Tibshirani & Wager, 2019; Athey & Wager, 2019). I find that students from families with relatively low socioeconomic status (SES) and financially disadvantaged students are more likely to take up student aid due to the intervention. After take-up, students have significantly higher total income while they have lower work income and receive less money from their parents. This suggests that correcting misperceptions about student aid conditions and individual eligibility by providing concise information can reduce financial constraints on disadvantaged students, their amount of paid work, and the burden on their parents. Thus, the intervention potentially tackles social inequality both at the student and household levels.

I contribute to several strands of the literature. First, there is a vast literature that empirically investigates the take-up of student aid and loans. Receiving financial support from the state during higher education tackles social inequality as it improves financial well-being, graduation rates, and later-life earnings (Bettinger et al., 2019; Black et al., 2023). Yet, experimental papers find that information is often insufficient to increase take-up rates and enrollment (Bettinger et al., 2012; Booij, Leuven & Oosterbeek, 2012; Peter & Zambre, 2017; Marx & Turner, 2020; Bird et al., 2021; Peter, Spiess & Zambre, 2021). Assistance in filling out the application, however, is effective as it addresses the complexity of the application process, especially of the FAFSA in the USA (Bettinger et al., 2012; Hoxby & Turner, 2015; Castleman & Page, 2016; Dynarski et al., 2021; Dynarski, Page & Scott-Clayton, 2023). Non-experimental evidence also determines self-control problems (Cadena & Keys, 2013) and debt and risk aversion (Fidan & Manger, 2021) as drivers of non-take-up. Yet, misperceptions about student aid might be a crucial determinant of non-take-up. Students might not apply because they underestimate the financial value of student aid and misperceive their own eligibility. I contribute to this literature by systematically measuring misperceptions about student aid conditions and eligibility, and identifying the causal effect of correcting misperceptions on take-up through an information intervention. Additionally, I contribute to the debate on reducing social inequality through student aid by showing that the intervention is particularly effective among disadvantaged students and that take-up alleviates financial constraints.

Second, I contribute to the literature on the role of misperceptions in decision-making. Empirically, misperceptions have been shown to influence, e.g., schooling (Jensen, 2010; Kaufmann, 2014; Reuben, Wiswall & Zafar, 2017), collective action for recycling (Fuhrmann-Riebel et al., 2024), COVID-19 vaccinations (Bartoš et al., 2022), investment behavior (Haaland & Næss, 2023), and insurance demand (Domurat, Menashe & Yin, 2021). With respect to student aid, little is known about the role of misperceptions. One exception are Booij, Leuven & Oosterbeek (2012), who show that subtle information improves knowledge about specific policy parameters of non-means-tested loans available for all students in the Netherlands while it does not increase take-up. In this paper, I look at student aid instead of loans, which is available only to eligible students based on a means-test. Additionally, I measure misperceptions about eligibility and repayment conditions of student aid using hypothetical scenarios instead of asking for specific parameters, and elicit perceived eligibility. I contribute to the literature by showing that misperceptions inhibit the take-up of means-tested student aid as students systematically underestimate the financial value of student aid and their own eligibility. Using a randomized intervention that concisely informs about these conditions and the individual entitlement, I show these misperceptions can be effectively corrected, increasing take-up of means-tested student aid.

Third, since Germany does not charge tuition fees, its student aid program is comparable to a social benefit as the aid is used to cover living expenses. Therefore, it touches on the literature investigating the take-up of general social benefit programs. Like student aid, non-take-up of social benefits despite eligibility is a general problem globally, where take-up rates are often below 50% (Ko & Moffitt, 2022). The discrepancy between take-up and eligibility primarily stems from the filing process's complexity or high transaction cost and unawareness about the program (Currie, 2006; Eurofound, 2015). However, there is mixed evidence on which interventions best solve these problems. A reduction of complexity and transaction cost through assistance or simplifications helps, e.g., for claiming tax benefits (Bhargava & Manoli, 2015; Ihlanfeldt, 2021; Goldin et al., 2022), unemployment aid (Chareyron, Gray & L'Horty, 2018; Castell et al., 2025), or applying for food stamps (Finkelstein & Notowidigdo, 2019; Gray, 2019). Information provision helps in settings where people are unaware of forgoing substantial monetary or service benefits, such as healthcare services (Nguyen, Le & Connelly, 2020; Kacker et al., 2022), social security benefits (Liebman & Luttmer, 2015), student debt repayment (Cox, Kreisman & Dynarski, 2020), but also aforementioned tax benefits (Bhargava & Manoli, 2015; Engström et al., 2019; Pham, 2019) or food stamps (Daponte, Sanders & Taylor, 1999; Finkelstein & Notowidigdo, 2019). Yet, it it remains unclear from this literature how misperceptions about eligibility conditions and own eligibility relate to take-up and if concise information can serve as an intervention to correct misperceptions and increase take-up. This paper can address these questions and thus aims to fill this gap.

The paper is structured as follows. Section 2 explains the context of student aid in Germany. In section 3, I explain the experimental design and data collection. The intervention effects on misperceptions and take-up are described section 4. Section 5 concludes the paper.

2 Federal Student Aid in Germany

In Germany, the only need-based federal student aid program is the BAföG. With an annual volume of €2.9 billion and 360,000 students who received on average €663 per month in 2023, the BAföG is by far the largest student aid program in Germany (Destatis, 2024). Additionally, only 4% of students receive merit-based scholarships (Kroher et al., 2023). Since no other need-based aid exists, I can focus only the BAföG program to measure misperceptions and take-up of overall student aid on a national level.

The amount of student aid one receives is split equally into a non-refundable grant and an interest-free loan. Students can receive a maximum of €934 per month, comparable to a Pell Grant and a Direct Subsidized Loan in the USA.[2] Similarly to the FAFSA, students must apply for BAföG every year and pass the means-test. The administration computes how much students' parents can contribute to the cost of living while attending university. This amount is deducted from the maximum potential aid of €934 to calculate the individual financial aid the respective student is entitled to.[3] Then, the student's monthly salary above €520 is deducted from their entitlement. Students can receive the aid at most for the same time as the standard period of study of their major, which is usually five years for a bachelor's and master's program. The application for student aid does not have a deadline. The only restriction is that one cannot receive student aid for any month before the application. This allows me to analyze how correcting misperceptions increases take-up as the students who misperceive their eligibility can immediately apply once they correct their misperception.

Student aid in Germany is mainly used for living expenses as students do not have to pay tuition fees but only an administrative fee of around €600 per year for attending a public university. Public universities host 88% of all students (Destatis, 2023), and the overall best-ranked universities in Germany are all public. Therefore, the university entrance barrier in Germany is low, but students still need to finance their living expenses. Due to financial constraints, students from lower SES families have to work more to cover these expenses, which prolongs study time (Triventi, 2014; Avdic & Gartell, 2015) and impairs academic performance (Callender, 2008). Therefore, student aid can be used as an instrument to tackle social inequality even after enrollment, especially since forgoing financial aid results in lower persistence and graduation rates, higher workload during studies, and lower earnings after graduation (persistence: Glocker, 2011; Fack & Grenet, 2015; Castleman & Long, 2016; Bettinger et al., 2019; Denning, 2019; Nguyen, Kramer & Evans, 2019; Murphy & Wyness, 2023; workload: Park & Scott-Clayton, 2018; Denning, 2019; Herber & Kalinowski, 2019; Kofoed, 2022; earnings: Bettinger et al., 2019; Denning, Marx & Turner, 2019).

How much aid students receive severely depends on their parents' income. For the student aid calculation, the income from two years ago is considered.[4] Parents with one child can have an annual gross income of up to €85,000, with two children of up to €120,000 until the children are not eligible for student aid anymore. The average gross income of couples with at least one child was €91,000 in Germany in 2021, the relevant year for my data

[2] A Pell Grant of \$7,395 and a Direct Subsidized Loan of \$4,750 sum up to \$12,145 per year, which equals €11,245 with an exchange rate of 1.08€/\$. The maximum student aid in Germany is 11,208€ per year.

[3] The maximum amount is reduced to €812 if the student is health insured through their parents. The amount is increased by €160 for each child of the student. The values are based on the program's modalities in 2023/24, when data collection for this study took place.

[4] The student aid calculation does not consider current income because one has to hand in the income tax receipt of the parents, which is usually only available with a lag of two years. If the parents' current income is smaller, one can request to use this income instead.

collection (Destatis, 2022). Given the magnitude of these thresholds, it is likely that students underestimate them and therefore potentially misperceive their own eligibility for student aid.

Irrespective of the accumulated aid, the loan part of student aid is capped at €10,010, so a receiving student cannot acquire more debt than this. Repayment of the loan starts five years after the standard period of study has ended, so usually when the student already entered the labor market. Additionally, the student receives a discount of up to 21% if the loan is repaid in one lump sum. In case the student has a net income below €1,605 per month[5], the repayment can be deferred, which is comparable to the income-driven repayment in the US. A crucial difference is that the loan in Germany stays interest-free throughout the repayment period. This makes the loan more beneficial compared to other contexts like the US, the UK, or the Netherlands, and it mitigates the influence of debt aversion on take-up. It also creates room for misperceptions, however. Students who do not know that only half of the student aid is an interest-free loan and that this is capped at €10,010 might overestimate the potential debt and not take up aid despite eligibility.

Despite its benefits, take-up of student aid is low. At least 40% of eligible students do not take up their entitlement (Herber & Kalinowski, 2019). The problem is not that students apply and do not pass the means-test but that they do not apply. 80% of the students state that they never applied, from which 63-76% think that their parents' or their spouse's income is too high to be eligible (Kroher et al., 2023). Given the discrepancy between eligibility and take-up, some students must be wrong and misperceive their eligibility.

With the structure and environment of federal student aid in Germany, students likely have misperceptions about the financial value of student aid. That is, they could underestimate the amounts one can receive per month, underestimate income thresholds for parents for eligibility, and overestimate the repayment amounts. Additionally, they could misperceive their own eligibility. These misperceptions could influence take-up. German student aid, therefore, provides the ideal setting to analyze the effect of correcting misperceptions on take-up through concise information.

3 Experimental Design and Sample

The experimental design, the incentive structure, the variables collected, the information intervention, and the research hypotheses were preregistered at the AEA RCT registry (AEARCTR-0011249[6]) before the data collection started. The preregistration was updated

[5] Additional allowances apply if one is married and/or has children to take care of.
[6] https://doi.org/10.1257/rct.11249-5.0

before the second wave to include additional control variables and a second intervention. Since some students applied but did not have a decision yet, I contacted them again in a third wave to see if the application was successful to measure take-up. All additions were preregistered before they were implemented. The study was ethically approved by the Faculty of Management, Economics, and Social Sciences of the University of Cologne ethics committee (230011SR).

3.1 Data Collection Waves

The experiment was conducted in three waves to measure if concise information about the eligibility and repayment conditions corrects misperceptions and increases take-up of student aid. The first wave was collected in May 2023. May was deliberately chosen since the summer term at German universities starts in April. Every eligible student who did not apply for student aid in April has already forgone one month of potential aid. Assuming everyone who planned to apply for the summer term applied in April, the data collection started in May, so only students who did not intend to apply were treated.

The survey was distributed through the general student committees of the 83 public universities in Germany. The committees contacted students with a separate email that exclusively advertised participation in the survey, as part of their monthly newsletter distributed via email, and/or through their Instagram channels. During the first wave, students were asked for an email address and for consent to be contacted directly for the second wave.

At the beginning of the first wave, I asked students about their monthly income, as displayed in Figure B.1. Specifically, students were presented with input fields on how much money they receive from different sources, e.g., their parents, work, scholarships, and federal student aid. If they indicated not to receive any federal student aid, participants were asked if they had applied for this semester or a previous semester. Only students who did not receive student aid and did not apply for this semester were considered for the experiment.

To determine if a student was eligible for federal student aid, I asked participants about their parents' monthly net income in increments of €500. I deliberately asked for net instead of gross income because parents' net income is more tangible to the students and easier for them to answer precisely (Anderson & Holt, 2017). Additionally, I elicited the students' confidence in these income reports for each parent using a slider from 0-100% in increments of 10%. This enables me to measure who knows what their parents earned and who only gave a guess. The elicitation is displayed in Figure B.2. I also asked participants for their parents' and their own marital status, how many siblings they had, and whether they lived with their parents. This allows me to check who fulfilled the general eligibility conditions and how much student aid they could expect if they applied.

For all participants, I elicited misperceptions about student aid eligibility and repayment conditions. Additionally, students were asked if they believed to be eligible. How misperceptions are measured is explained in detail in Section 3.2.

After the misperception elicitation, students were asked why they did not apply for student aid. I elicited several reasons using a 5-point Likert scale matrix where students had to indicate for each reason whether it applied to them or not. The matrix comprised reasons related to not being eligible, such as "My parents have said that their income is too high" or "I have too many assets", but also reasons related to deciding against student aid, such as "I receive enough financial support from my parents" or "I do not want to take on any debt". The complete list of potential reasons is shown in Figure B.3. The order of the reasons displayed was randomized.

At the end of the first wave, a stratified subsample of the participants received an information intervention that tackled potential misperceptions. The stratification and content of the intervention are explained in section 3.3.

The second wave was collected six months later, in November and December 2023, to leave time for the student aid offices to review applications. Unfortunately, six months was insufficient as many students did not have their final application decision in the second wave. For this reason, students were contacted for a third wave from July to September 2024. Students were contacted directly via email. In both recontacts, students started by entering their monthly income from different sources such that take-up can be measured through positive student aid amounts. In case no student aid was indicated, participants were explicitly asked if they applied and, if yes, whether the application was accepted, pending, or declined.

Additionally, students were asked about which semester they were in, what study field they were enrolled in, at which university they were studying, who mainly handled their finances, if someone in their closest circle received student aid, if they had ever talked to anyone about applying and with whom, and how wealthy they think their parents were compared to other families in the first wave. In the second wave, students were asked if they and/or their parents were born in Germany, if their parents were civil servants, and if their parents had a postsecondary degree. I also elicited impatience, debt aversion, and impulsivity using 10-point Likert scale questions. The current GPA and enrollment status were elicited in the second and third waves.

Students received lottery tickets for their participation in the survey. Each student received 10 tickets with the chance to win additional tickets during the survey. In the first wave, 100 tickets were randomly selected to win €25 each; in the second and third wave, 200 tickets were randomly selected to win €50 each. Each student could only be picked once per wave, so drawing two winning tickets of the same person was ruled out. The increased incentives in the second wave were already announced to participants in the first wave to reduce attrition.

3.2 Measuring Misperceptions

I use hypothetical case scenarios of student aid receivers to elicit how well participants perceive the eligibility and repayment conditions of federal student aid. This approach is similar to using scenarios to measure expectations (e.g. Manski, 2004; Attanasio & Kaufmann, 2014; Boneva & Rauh, 2018; Boneva, Golin & Rauh, 2022). Yet, it also works for perception elicitation as it enables me to give the participants all the necessary information to assess a case and state their perception without only asking for maximum and minimum thresholds of eligibility and repayment conditions. Therefore, I can measure more specifically how well the students can assess the dynamics of student aid and if they have a good perception of its conditions.

I use three different scenarios: One to elicit perceptions of how much financial aid a student can receive per month, one to elicit how much a student's parents can earn for a given amount of student aid, and one for how much a student has to repay. The scenarios were designed in a way that online student aid calculators cannot assess the correct answers without additional information.[7] Additionally, I recorded if participants left the online survey website on each survey page from the three scenarios and the last additional page. This serves as a proxy to control whether they seek further information to give better answers. The scenario for the amount of student aid reads as follows:

Anna (22) is a student and lives in a student dormitory. Her father is an employee and had a gross annual income of €60,000 two years ago. Her mother is a housewife and had no income. Anna has free health and long-term care insurance through her parents. She has no assets of her own. Her little sister Sophie (14) is still in school.

Below this scenario, the participants were asked how much student aid Anna receives per month. The information on the housing situation, income, insurance, and siblings is sufficient to assess the correct amount of student aid Anna receives.

For this scenario, two additional questions were asked. The participants were told that Anna's mother now had an income of €20,000 two years ago and asked how much student

[7] The student aid calculators are programmed to map complex cases, so they explicitly ask for further information, e.g., the parents' tax burden or the loan amount of student aid. This information is incorporated in the scenarios without explicitly showing it to avoid redundancies.

aid Anna would receive in this case. Analogously, the participants were told that Anna now has assets worth €18,000 instead and asked how much student aid she receives in this case. These two changes were used to measure how well the participants perceived the amount of student aid per month more broadly with different income and wealth amounts. The two questions were randomized in order. Participants received an extra lottery ticket for each correct answer. An answer was counted as correct if the entered amount was in the €200-interval around the actual student aid amount. Table C.1 presents the correct values for each question per scenario. For each of the three questions, students were asked how confident they were in their answer with a slider from 0-100%. Following the survey guide from Stantcheva (2023), this allows me to elicit the point estimate for the deviation from the correct value and how strongly these deviations are anchored into the students' perceptions of student aid.

Similarly, the scenario on the income thresholds for parents reads as follows:

Max (20) is in his first semester at university and lives in a shared flat. He has no siblings. His mother is single and works as an employee. His father has broken off contact and cannot be reached. Max has free health and long-term care insurance through his mother. He has no assets of his own. Max receives €360 a month in BAföG.

In this case, students were asked how much Max's mother earned gross per year. I deliberately chose a scenario where only one parent contributes to the student aid calculation. This is easier to answer as participants do not have to consider two incomes. At the same time, I can still measure participants' perceptions of parents' income thresholds for a given student aid entitlement. One more question was asked based on this scenario. I told participants to imagine that Max now has a sister who is also studying and lives in a student dormitory. Students then were asked how much Max's mother earned in this case, given that Max still receives €360 per month. An answer was counted as correct if it was in the €15,000-interval around the actual income of Max's mother. As before, students were asked to indicate how confident they were in their answers.

The third scenario on repayment of the loan reads as follows:

Sara (29) started working after completing her Bachelor's degree. During her 3-year studies, she received €250 BAföG per month. In total, she received €9,000. Sara repays her BAföG loan in installments.

Here, participants were asked how much Sara has to repay. Two changes were surveyed for the repayment scenario. First, I told students to consider that Sara would repay her loan all at once and asked how much Sara would have to repay in this case. This was asked to measure how well students perceive discounts for repaying the whole loan at once. Second, I told students to imagine that Sara received €500 per month for 5 years instead, such that she received €30,000 in total. This change was surveyed to measure if students knew the student loan is capped at a maximum debt of €10,010. The two additional questions were randomized in order. An answer was counted as correct if it was in the €1,000-interval around the actual repayment amount. Analogously to the other scenarios, students were additionally asked for their confidence in their answers.

For each correct answer, students received an additional lottery ticket to win the prize of €25 or €50. The same scenarios, only with different names, were used in the second wave of data collection to measure how misperceptions on an individual level change over time.

In addition to the scenarios, I elicited the participants' believed individual eligibility for student aid. Each student was asked "Do you think you would get BAföG if you applied for it?" with answers on a 5-point Likert scale ranging from "Definitely Yes" to "Definitely No".

3.3 The Information Intervention

At the end of the first survey, randomly selected students received information about federal student aid. This is the treatment group. The control group did not receive information. The information intervention had two pages in the survey. On the first page, students received concise information about income thresholds for parents for student aid eligibility, the maximum amounts of financial aid one can receive per month, the repayment cap of €10,010 and additional discounts for repaying the loan all at once, and information on age and wealth limits of the applicants. Additionally, links to the official website of the federal student aid and the application were displayed. Figure B.4 shows this page.

On the second page, students eligible for student aid based on their answers received information on how much student aid they could receive if they applied. Students who were not eligible or for whom the entitlement could not be calculated received information on how much their parents can earn per month for them to be eligible instead. Figure B.5 displays the second page.

The intervention was stratified at the cohort level, balancing universities by number of students, federal state, distribution channel of the survey invitation, and university specialization using the minMSE approach (Schneider & Schlather, 2017). Students from the same university, study program, and cohort were always assigned to the same group to minimize spillovers. Appendix A.1 provides a detailed description of the stratification process.

3.4 The Sample

The first wave was collected from May 2 to May 31, 2023. In total, 22,222 students from all 83 public universities participated and finished the survey. The median participation took approximately 15 minutes. Students with a degree program invalid for federal student aid, e.g., PhD candidates, and invalid answers during the misperception questions are excluded.[8] Summary statistics for the remaining 21,869 participants are displayed in Table C.2, split between students who applied for student aid and students who did not.

Students were recontacted in November to participate in the second wave. Data collection took place from November 2 to December 15, 2023. Out of the 17,636 students who consented to be recontacted, 12,096 participated in the second wave, corresponding to a response rate of 68.6%. Median participation took approximately 12 minutes. 6,225 of these did not apply for student aid before the first wave and indicated no institutional reason for ineligibility.[9] This group is the experimental sample. Comparing the experimental sample to all students who participated in the first wave that could have been part of the experiment, I do not find evidence for selective attrition, as shown in Table C.3. The only difference is that the ones who participated in both waves are less likely to think they are eligible for student aid and have lower misperceptions with respect to income thresholds for parents. Since students who believe they are eligible and who severely underestimate the income thresholds for parents are more likely to apply for student aid between the two waves, the reported take-up rates can be interpreted as a lower bound.

The experimental sample is similar to a representative sample of non-receivers from a nationwide survey among students in Germany from 2021 (Becker et al., 2024). The comparison of the experimental and this representative sample is shown in Table C.4. Students in the experiment are younger, more likely female, single, and do not live with their parents. Yet, most differences are small, which suggests that the experimental sample is a good representation of the German non-receivers of student aid.

The balance table for the experimental sample is displayed in Table 1. As we can see from the last column, the treatment and control groups are not significantly different from each other in any of the sociodemographic variables or the response rate. Focussing on the last three rows, we see that students have misperceptions in all three areas. Pooling both groups in Table C.3, students in the experimental sample underestimate the amounts of student aid by €265, underestimate the income thresholds for parents by €15,414, and overestimate

[8] All scenario-answers of student aid amounts above €10,000 per month, income thresholds for parents above €500,000 per year, and repayment amounts above €100,000 were excluded.

[9] 276 students were excluded who did not take up student aid because they are foreigners, study longer than their standard period of study, receive another scholarship, changed their subject, or study something not covered by student aid. These students are institutionally ineligible and cannot receive student aid.

Table 1: Balance Table of Experimental Sample

	Control Group (N=3265)		Treatment Group (N=2960)		Diff. t-test
Variable	Mean	SD	Mean	SD	p-value
Age	24.284	4.089	24.318	3.786	0.731
Female (=1)	0.621	0.485	0.626	0.484	0.673
Monthly Income in Wave 1 in €	1048.488	485.785	1045.062	507.313	0.787
Migration background (=1)	0.206	0.405	0.201	0.401	0.617
Single (=1)	0.966	0.180	0.963	0.190	0.419
Study year	3.654	1.908	3.636	1.902	0.718
Lives with parents (=1)	0.160	0.366	0.164	0.370	0.673
East Germany (=1)	0.179	0.383	0.181	0.385	0.847
Believes to be eligible (=1)	0.087	0.282	0.090	0.287	0.655
Potentially eligible (=1)	0.354	0.478	0.353	0.478	0.928
Response rate	0.673	0.469	0.678	0.467	0.604
Misperception Area (in €)					
Amounts of Student Aid	−266.051	216.423	−262.996	224.42	0.585
Income Thresholds for Parents	−14951.85	24695.91	−15923.55	23028.3	0.108
Repayment Amounts	2887.425	4317	2760.481	4148.531	0.237

Notes: The table shows the summary statistics of the experimental sample's control and treatment group participating in the first and second data collection wave. The last column shows the p-value corresponding to two-sided t-tests of the means of each group, respectively. Misperceptions are coded as the deviation from the correct value in each elicitation question and averaged per area based on the three hypothetical scenarios used for elicitation. Negative signs indicate that students underestimated the correct values and vice versa.

the repayment amounts by €2,827, on average. As the p-values in the last column of Table 1 show, these misperceptions are not significantly different between the control and the treatment group. Thus, the only difference is that one group received additional information about the eligibility and repayment conditions of student aid and their potential entitlement, and the other did not. This allows me to identify the causal effect of this information on misperceptions and take-up rates that were measured as part of the second and third wave of data collection.

4 Causal Effects of the Information Intervention

To test if concise information can causally correct misperceptions and, through that, increase take-up of student aid, this section is organized as follows. I first show how the information intervention changed misperceptions about the student aid criteria and about one's own eligibility. Second, I turn to the direct effect of the intervention on student aid take-up. Third, I combine these two channels to identify the causal effect of correcting misperceptions on take-up rates. Last, I discuss heterogeneous treatment effects to show which students are particularly targeted by the information intervention to take up student aid.

4.1 Intervention Effects on Misperceptions

Misperceptions are a potential driver of non-take-up since they might cause students to question their eligibility, the amount of student aid they can receive, and how much they need to repay. As shown in Table 1, the average student underestimates the student aid amount and the income thresholds for parents, and overestimates the repayment amount. This pattern of misperceptions does not only happen on average but for the majority of the sample, as the distributions of misperceptions in Figure C.1 show. In fact, 99.2% of the students either underestimate the amounts of student aid, underestimate the income thresholds for parents, or overestimate the repayment amounts. Additionally, 63.1% show all three of these misperceptions simultaneously. This means that a clear majority of students underestimates the financial value of student aid in all three areas.

To analyze if concise information about these student aid conditions corrects misperceptions, I estimate the following model. I focus on the effect on underestimators since correcting their misperceptions improves their view of the financial value of student aid, which could cause them to take up student aid. Results from OLS estimation are presented in Table 2. Table C.5 includes the coefficients for both over- and underestimators. All standard errors are clustered at the study field per university, so one level above the stratification, following Chaisemartin & Ramirez-Cuellar (2024) and Abadie et al. (2022).

$$MDiff_i = \beta_0 + \beta_1 Int_i + \beta_2 (Int_i \times Overest_i) + \beta_3 Overest_i + \delta_j X_{ij} + \alpha_s + \gamma_u + \epsilon_i \qquad (1)$$

The correction of misperceptions is measured as the individual difference in misperceptions, $MDiff_i$, where second-wave misperceptions are subtracted from first-wave misperceptions. Both are quantified as the average absolute deviation from the correct values from the scenarios' questions in percent. Int_i is the indicator equal to 1 for participants who received the information intervention. $Overest_i$ is the indicator that shows if an individual overestimates the financial value of student aid, so it is equal to 1 for students who overestimate the amounts of student aid, overestimate the income thresholds for parents, and underestimate the repayment amounts in the first wave for at least one question per scenario. I control for misperceptions per area in the first wave to measure treatment effects independent of high or low initial misperceptions. Additionally, I control for sociodemographic and control variables from the survey, reasons for non-take-up, and preferences, mentioned in Section 3.

Table 2: Intervention Effect on Difference in Misperceptions from 1st to 2nd Wave

	Correction of Misperceptions (in %)				
	Amounts of Student Aid	Income Thresh. for Parents	Repayment Amounts	Pooled Domains	Total Number
	(1)	(2)	(3)	(4)	(5)
Info-Intervention (=1)	0.037***	0.013	0.144***	0.058**	0.040***
	(0.008)	(0.009)	(0.042)	(0.024)	(0.010)
Mean (Control Group)	0.091	0.085	0.254	0.180	0.113
Observations	6,225	6,225	6,225	6,225	6,225
R^2	0.373	0.493	0.391	0.370	0.354
F Statistic	25.323***	41.360***	27.286***	24.966***	23.332***

Notes: The table shows the intervention effects on the correction of misperceptions from the first to the second wave. Misperceptions are measured as the absolute deviation from the correct values in the elicitation scenarios, divided by these correct values to determine the misperceptions in %. Misperceptions are averaged per area for columns (1)-(3), and over all areas for column (4). Column (5) uses the total number of misperceptions, measured as the number of answers to the elicitation scenarios outside the incentivized interval as explained in Section 3.2. The outcome is the correction in misperceptions, calculated as first-wave minus second-wave misperceptions, such that positive coefficients show a stronger correction of misperceptions. The positive coefficients in row 1 show that the intervention reduced misperceptions for the participants who underestimated the financial value of student aid significantly.
I control for misperceptions in the first wave, all sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Control variables are captured by X_{ij}. Study field and university fixed effects are included with α_s and γ_u, respectively. The error term is given by ϵ_i. Table 2 shows the coefficients for β_1.

The information intervention significantly corrected misperceptions for the underestimators. I find significantly positive effects of the intervention on the correction of misperceptions for different areas of student aid in columns 1 and 3 of Table 2, and the total number of questions from the scenarios answered within the incentivized bounds in column 5. Students who underestimated the correct value for all questions correct their misperceptions due to the intervention by overall 5.8 pp (32%) more than the control group, as shown in column 4. I find similar significances using the average misperceptions per area instead of the single answers to identify overestimators, displayed in Table C.6. Thus, the information intervention significantly corrected misperceptions of students who underestimated the financial value of student aid.

Potential misperceptions about student aid eligibility and repayment conditions might also cause students to believe they are not eligible even though they are. The questions on

Table 3: Intervention Effect on Misperceptions About Own Eligibility

	Correction of Eligibility Misperceptions (=1)			
	Eligible students: without own income		Eligible students: with own income	
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.041***	0.030**	0.060***	0.052***
	(0.015)	(0.014)	(0.017)	(0.017)
Constant	0.101***	0.698***	0.106***	0.658***
	(0.009)	(0.199)	(0.011)	(0.252)
Misperceived Eligibility W1 (=1)	0.869	0.869	0.862	0.862
Study Field FE	No	Yes	No	Yes
University FE	No	Yes	No	Yes
Observations	2,361	2,361	1,786	1,786
R^2	0.004	0.118	0.008	0.132
F Statistic	9.208***	2.310***	13.931***	2.005***

Notes: The table shows the intervention effects on the correction of misperceptions about the participants' own eligibility for student aid from the first to the second wave. Only participants are considered who are classified as eligible for student aid and misperceive this eligibility in wave 1, so participants that do not believe to be eligible, hence answer the Likert scale question on perceived eligibility in wave 1 with "Rather No", "Definitely No", or "Cannot give a clear answer". The correction of misperceptions is equal to 1 for students who change their eligibility belief or apply for student aid after wave 1. The fraction of students who misperceive their own eligibility in wave 1 is shown below the constant. To determine eligibility, the student's sociodemographic and economic situation excluding their own income is used for columns (1) and (2), and including their income for columns (3) and (4). The positive coefficients in row 1 show that the intervention corrected misperceived eligibility significantly by 3 to 6 pp.
I control for all misperceptions from the scenarios, confidence in these answers, sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects. Clustered standard errors are in parentheses.
$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

the sociodemographic and economic background of the students allow me to determine the individual eligibility of students for aid. Additionally, the question on perceived eligibility allows me to measure the extent of misperceptions about own eligibility and how these misperceptions change due to the information intervention.

To measure the intervention effect, I focus on the students who are eligible but who think that they are not. That is, I first restrict the sample to those with a positive calculated entitlement, the eligible students. To determine eligibility, I use two approaches: Excluding the students' own income, and including it. The means-test of student aid is calculated first considering parental income. Yet, the students' earnings can reduce the amount of student aid they receive after a successful application. Therefore, I distinguish between the more inclusive approach without students' income and the conservative calculation, including students' income. Next, I drop students who answer the Likert scale question on perceived eligibility with "Rather Yes" or "Definitely Yes", so the students who know they are eligible. The remaining sample consists of students who are eligible but do not believe to be. Table 3 shows OLS results for the intervention effect on the correction of the eligibility misperceptions, which equals 1 for students who indicate in the second wave that they believe to be eligible or who apply for student aid after the first wave.

I find that 86-87% of the eligible students do not believe they are eligible for student aid, as shown in the first row below the coefficients of Table 3. That is, the large majority of eligible students have misperceptions about their eligibility. Yet, concise information about the conditions of student aid and their potential entitlement helps to resolve these misperceptions. As shown in the first row, the intervention corrects these misperceptions by 3-6 pp. Given that already 10% of the control group correct their eligibility misperceptions after participating in the first wave, as shown by the constant in columns 1 and 3, the intervention amplifies this correction by 30-57%. Using all changes in the Likert scale question on perceived eligibility as the outcome instead of the binary variable in Table C.7, I find similar results.

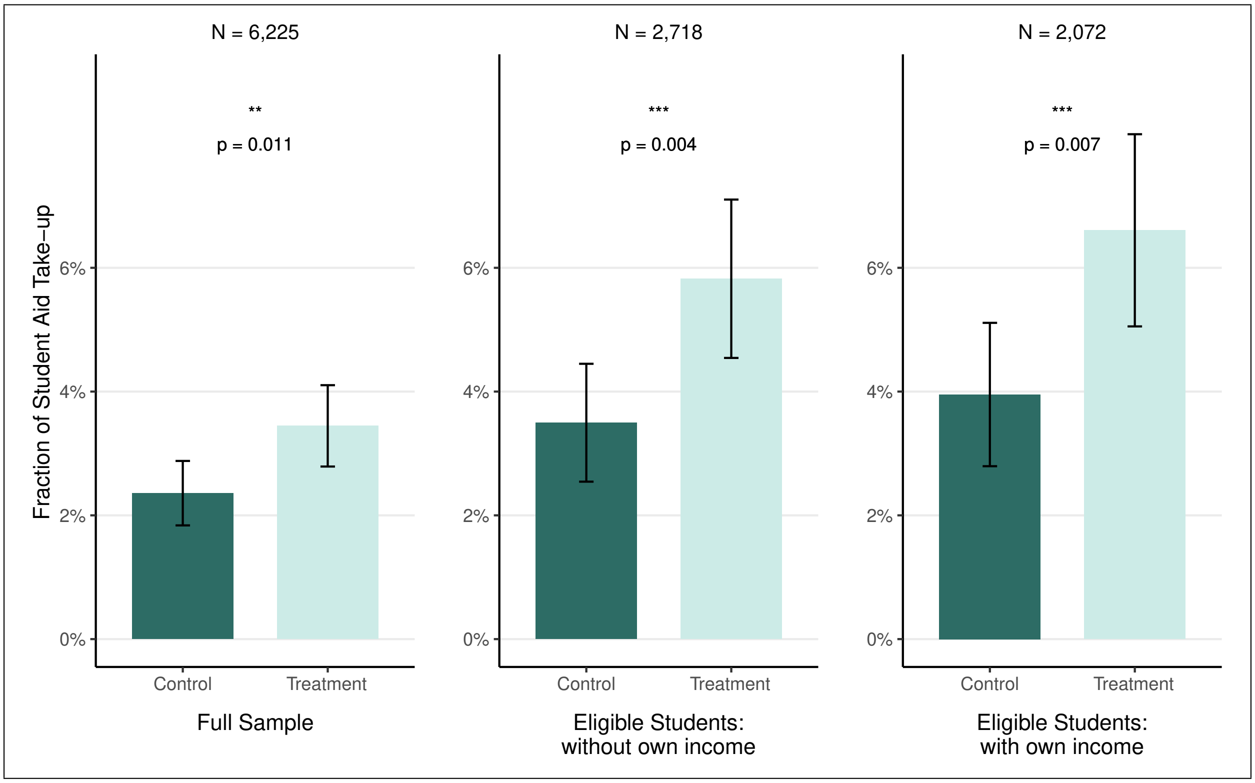
Overall, the intervention significantly corrected misperceptions of both the general student aid conditions and individual eligibility. This raises the question if the intervention also increased take-up rates, which is addressed next.

4.2 Intervention Effects on Take-Up Rates

To show how the information intervention changed take-up, I compare take-up rates between control and treatment group students after the first wave. In the second and third waves, students were asked for their income from student aid. All students who indicate a positive amount must have taken up student aid after the first wave since only students without student aid and an application are part of the experiment. Additionally, eligible students who indicated a pending application in the second wave but did not participate in the third wave are imputed to take up. If these students had participated in the third wave, they most likely would have indicated a positive student aid amount since they already applied and had a positive calculated entitlement. All results hold when these students are not considered for take-up. The individual eligibility calculation allows me to identify the causal effect of the information intervention on take-up for all students in the sample and directly among eligible students.

In Figure 1, I compare the fraction of student aid take-up between the control and treatment groups for the full sample and two restrictions of eligible students. In the middle panel, I do not consider their own income to determine eligibility as this is not part of the

Figure 1: Intervention Effect on Student Aid Take-up for Full Sample and Eligible Students



Notes: The figure shows the increase in the fraction of student aid take-up for the control and treatment groups. In the left panel, the full sample is used to calculate the fractions. In the middle and right panel, only the eligible students, excluding and including their own income when determining eligibility, are displayed. The sample size and p-values of the difference between the two groups are reported above the bars.

means-test. Income is considered for the right panel, however, as the student's salary can reduce the amount of student aid they receive per month. Students who learn about their eligibility might reduce working hours to receive their full student aid entitlement. Therefore, both cases to determine individual eligibility are depicted.

The treatment group has a significantly higher take-up rate in all three panels than the control group. While 2.4% of the control group in the full sample take up student aid, 3.5% in the treatment group do. The information intervention, therefore, led to a significant 1.1 pp increase in take-up, corresponding to an effect size of 46%. While students in the control group receive €506 per month, on average, students in the treatment group receive €531 after take-up. This suggests that more entitled students react to the intervention. In line with this, I find stronger intervention effects among eligible students. In the middle panel, we see an increase from 3.5% to 5.8%, and in the right panel from 4.0% to 6.7%, corresponding to an effect size of 66% and 68%, respectively. This suggests that the intervention effect was driven by students that I classify as eligible for student aid. Regression results for the full sample are presented in Tables C.8 and C.9, and for the eligible students in Tables C.10 and

C.11. Probit estimations are shown in Appendix D as robustness checks.[10]

Most students who receive student aid take up their entitlement at the beginning of their studies. Only 1.4% of students take up student aid after their first semester.[11] Since the students in the experimental sample are already enrolled, the intervention effect can be interpreted as increasing this fraction. With a 1.1 pp increase, the intervention nearly doubles this fraction. Yet, 2.1% of the control group also take up aid without the intervention, which suggests that I measure a lower bound. Even with this lower bound, the economic significance is already quite large. Assuming that students would receive the current average student aid of €663 per month after scaling up, a 1.1 pp increase in take-up would be equivalent to €180 million more student aid per year.[12]

One might argue that spillovers could have biased the intervention effect. Since the treatment was carefully stratified and participants are spread across the country, spillovers are unlikely to be a concern. Yet, some circumstances could facilitate spillovers, such as the number of participants at a single university or university size. To test this, I compare the intervention effect of 1.1 pp to different specifications of university level intervention effects which could have facilitated spillovers. Results are reported in Table C.13. No specification yields significantly different intervention effects. This supports that spillovers are unlikely to have biased the intervention effect.

4.3 Correcting Misperceptions to Increase Take-Up

Until now, we have seen that the intervention effectively corrects misperceptions and increases take-up. Yet, we do not know the causal effect of correcting misperceptions on increasing take-up. To analyze this, I can make use of the experimental design and estimate the local average treatment effect (LATE) (Imbens & Angrist, 1994; Angrist, Imbens & Rubin, 1996). All assumptions to estimate the LATE are fulfilled. A detailed discussion is provided in Appendix A.3.

The LATE yields the causal effect of correcting misperceptions on take-up for the com-

[10] As preregistered, I also analyze the effect of a second, cross-randomized intervention to test if information about eligibility alone increases take-up. The intervention was part of an email sent to all participants where 200 students of each the control and treatment group received an extra paragraph informing only about their eligibility for student aid. Due to a lack of power, I do not find significant effects. OLS regression results are reported in Table C.12.

[11] The national take-up rate is 11% (Deutscher Bundestag, 2021). In my survey, only 12.5% of the students who receive student aid at some point take up aid after their first semester. Taken together, only 1.4% of all students take up aid after the first semester.

[12] In total, there are 2.9 million students, of which approximately 470,000 are not eligible due to institutional factors (e.g. non-EU citizen, second training) and approximately 360,000 who already receive federal student aid (Destatis, 2024). If 1.1 pp of the rest receive €663 per month, this adds up to €180 million per year.

pliers, i.e., the students whose misperceptions are correctable through information. As first stage, I estimate the treatment effect on correcting misperceptions and use the resulting estimates for the effect on take-up. Formulas 2 and 3 show the two-stage least squares model (2SLS).

$$MDiff_i = \beta_0 + \beta_1 Int_i + \delta_j X_{ij} + \alpha_s + \gamma_u + \epsilon_i \tag{2}$$

$$Takeup_i = \pi_0 + \pi_1 \hat{MDiff}_i + \mu_j X_{ij} + \alpha_s + \gamma_u + \eta_i \tag{3}$$

In the first stage, $MDiff_i$ is the correction of misperceptions from the first to the second wave, and Int_i is the intervention indicator. In the second stage, $Takeup_i$ is the indicator for take-up as the dependent variable, and $\hat{MDiff}_i$ is the estimate for the correction of misperceptions from the first stage as the explanatory variable. I include misperceptions in the first wave, sociodemographic and control variables from the survey, reasons for non-take-up, and preferences mentioned in Section 3, which are captured by X_{ij}. Study field and university fixed effects are included with α_s and γ_u, respectively. The error terms are given by ϵ_i and η_i. Results for the 2SLS-estimator are shown in Table 4 for different misperception specifications.

I analyze the effect of misperceptions about own eligibility, using only an indicator equal to 1 for students that correct their misperceived eligibility in columns 1 and 3, as well as using changes in the Likert scale to identify the correction in columns 2 and 4. The first and the second two columns again differ in how the student's eligibility is calculated: excluding the student's income or not. The last two columns show the 2SLS-coefficient for correcting misperceptions about student aid eligibility and repayment conditions pooling over all scenario-questions in column 5, and using the total number of answers within the incentivized bounds in column 6. The coefficients in the first row show that correcting misperceptions causally increases take-up. All coefficients are significantly positive and vary between 0.384 and 0.551. That is, correcting misperceptions completely, so to 100%, leads to an increase of take-up between 38.4 and 55.1 pp. The significant effects in all six specifications show that correcting misperceptions causally affects take-up such that correcting misperceptions can increase take-up rates substantially.[13]

One might argue that the instrument is weak as the first-stage F-statistic is below 10 in the first three columns. Yet, the persistently positive effects of similar magnitude for the remaining three columns with higher F-statistics show that even if the instrument is weak, there is evidence for a causal effect of correcting misperceptions on take-up.

In line with that, I find evidence that students took up student aid because they learned

[13] I find similar significances when I use a probit model as second stage, as shown in Table D.4.

Table 4: Causal Effect of Correcting Misperceptions on Student Aid Take-Up (LATE)

	Take-Up of Student Aid (=1)					
	Eligible Students: without own income		with own income		Scenarios	
	Binary	*Likert*	*Binary*	*Likert*	*Pooled*	*Total*
	(1)	(2)	(3)	(4)	(5)	(6)
Correction of Misperceptions (in %)	0.551**	0.535**	0.398***	0.444***	0.384**	0.424**
	(0.242)	(0.210)	(0.148)	(0.166)	(0.166)	(0.172)
Observations	2,361	2,361	1,786	1,786	6,225	6,225
1st stage F Statistic	4.330	6.487	9.642	11.597	14.475	24.503

Notes: The table shows results from 2SLS estimation of the correction of misperceptions from the first to the second wave on student aid take-up with the information intervention as instrument. The correction of misperceptions is measured as the difference between misperceptions in the first and the second wave, where columns (1)-(4) use misperceptions about the participant's own eligibility and columns (5)-(6) about the financial value of student aid based on answers to the elicitation scenarios. For columns (1) and (2), the participants' eligibility is calculated excluding their own income. The correction of misperceptions is measured using a binary variable or all changes in the Likert scale, respectively. Analogously, columns (3) and (4) include the student's income for the eligibility calculation. For column (5), all percentage deviations from the correct values of the scenario elicitation questions are pooled. For column (6), the total number of answers outside the incentivized interval around the correct value is used as misperception. The coefficients show the percentage increase in take-up through a correction of misperceptions by 1% for the compliers, the students whose misperceptions can be reduced through information.
I control for all misperceptions from the scenarios in the first wave, confidence in these answers, sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects in both stages. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

about their forgone entitlement. As part of the second wave, I asked students from the treatment group that took up student aid why they applied. The share of answers is shown in Table C.14. With 90.5%, most students who answered this question said the information that they could possibly expect a positive aid amount was the driver for their application. This underlines that the intervention helped students to realize they are eligible for student aid, thereby correcting misperceptions about their eligibility. Additionally, more than half of the students answered that the monthly student aid amount and parental income information led them to apply. This shows that also misperceptions about the student aid conditions were targeted through the intervention.[14]

Overall, the results show that correcting misperceptions about eligibility and repayment conditions and individual eligibility causally increases take-up. This correction is the driving

[14]Students could be unaware of student aid before the intervention. This is unlikely the case here. The BAföG program is the most prominent student aid in Germany and very salient. In this survey, no student indicated as a reason for non-take-up that they had not heard about BAföG before. In representative surveys, it is not listed as a reason for non-take-up (see Middendorff et al., 2017; Kroher et al., 2023).

mechanism behind the intervention effect on take-up. Yet, it is unclear which students are particularly targeted by the information intervention to take up student aid. For this, I will analyze the heterogeneity of the intervention effects next.

4.4 Heterogeneity of Intervention Effects

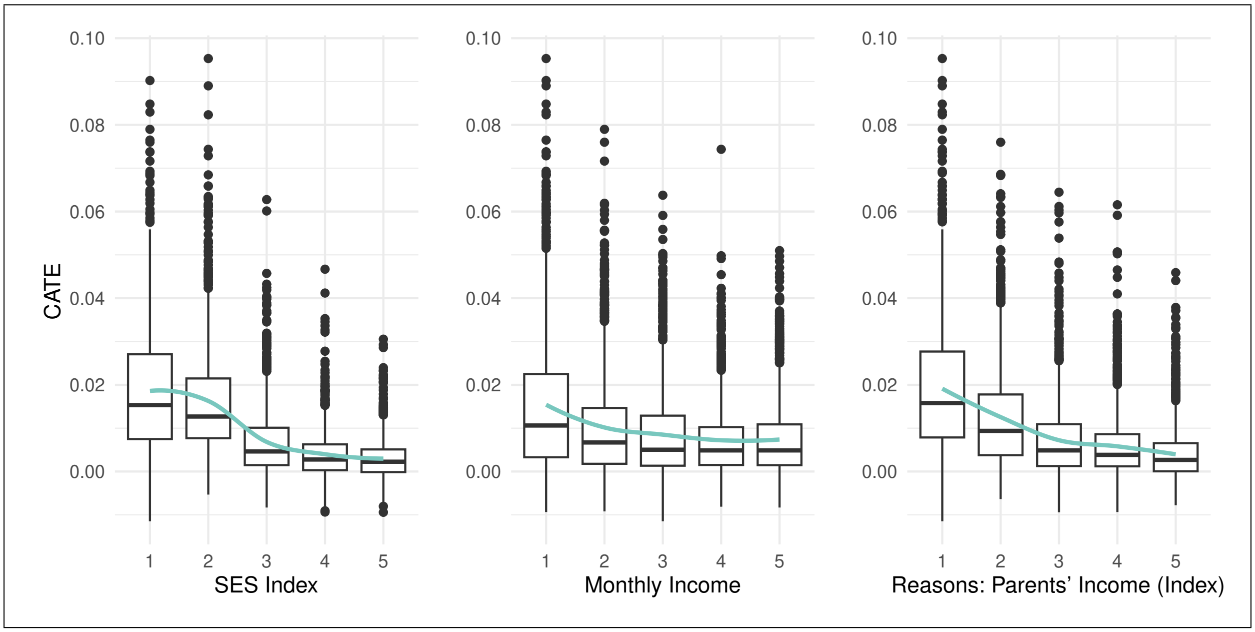
To analyze which students are particularly affected by the intervention and took up student aid, I use the causal random forest algorithm (Wager & Athey, 2018; Athey & Wager, 2019), which has gained increasing attention for analyzing heterogeneous treatment effects (e.g. Davis & Heller, 2017; Serra-Garcia & Szech, 2023). Before I apply the algorithm, I use principal component analysis (PCA) to create an index for socioeconomic status (SES). The index comprises parents' income with the highest weight, followed by the belief that parents are relatively poor compared to other families, migration background, parents' education, and if one parent has already died. A higher SES-Index corresponds to a higher SES.

Analogously, I use PCA as a dimension reduction technique to comprise different reasons for non-take-up of student aid that students indicated on a 5-point Likert scale. The PCA yields three components. The first captures application or student aid program-related reasons such as application complexity or debt aversion. The second captures reasons related to their parents' income being too high for eligibility and receiving enough financial support from their parents. The third captures reasons related to the student's own financial situation, such as earning too much or having too many assets. Higher values in these components correspond to a higher agreement with the respective reasons why one has not applied for student aid. The SES-Index and the three components of non-take-up reasons are used for the causal forest analysis instead of the variables they comprise. A detailed description of the PCA and the indices' construction is provided in Appendix A.2.

Following Athey & Wager's (2019) algorithm, I first train a pilot causal forest on all variables, including misperceptions, the SES-Index, other sociodemographic characteristics, and the reasons for non-take-up. Then, I train a second forest on only the variables that received above-average variable importance.[15] Both causal forests used clustering on the study field per university level, one level above the strata from the treatment assignment. Last, following the algorithm, I use the second forest to estimate out-of-bag predictions. That is, I estimate the conditional average treatment effects (CATE) for each observation within the sample using only trees that did not use the respective observation for the prediction. The CATEs from these predictions for the quintiles of the three most important variables

[15]Variables included in more sample splits within the trees of the causal forest to reduce the heterogeneity of the subsamples have a higher variable importance.

Figure 2: Conditional Average Treatment Effects of Variable Quintiles



Notes: The figure shows the conditional average treatment effects from causal forest estimation for the three most important variables to explain the heterogeneity of the intervention effects following the causal forest algorithm. Boxplots for variable-quintile are displayed. The mean-CATEs are connected with a fitted line.

for heterogeneity based on the causal forest are presented in Figure 2.

The CATEs indicate that students with higher financial constraints and more disadvantaged backgrounds react more strongly to the intervention. Starting from the left panel, the most important variable is the SES-Index. We can see that especially students with low SES have high CATEs. In line with this, students with low income show higher CATEs. Additionally, we see that students with a low index of reasons related to high parents' income react strongly to the treatment, meaning they do not think their parents' income is too high for eligibility and do not receive enough financial support from their parents. This suggests that more disadvantaged students seem to have been especially affected by the intervention and take up student aid.

Analyzing these heterogeneities not only for the predicted CATEs but also the true intervention effects, I estimate the following model:

$$Takeup_i = \beta_0 + \beta_1 Int_i + \beta_2 X_i + \beta_3 (Int_i \times D_i) + \delta_1 Aid_i + \alpha_s + \gamma_u + \epsilon_i \tag{4}$$

The outcome variable $Takeup_i$ is an indicator equal to 1 if the student took up student aid after the first wave. Int_i equals 1 if the student received the information intervention. X_i is the respective variable proposed by the causal forest, as shown in Figure 2, and D_i is an indicator equal to 1 for students below the 40%-quantile, so in the lowest two quintiles of X_i.

Table 5: Heterogeneous Intervention Effects on Student Aid Take-Up

	Take-Up of Student Aid (=1)			
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	-0.001	0.003	0.003	-0.008^{*}
	(0.003)	(0.004)	(0.003)	(0.004)
SES-Index	-0.007^{***}			-0.002
	(0.002)			(0.003)
Intervention X	0.030^{***}			0.024^{***}
Low Quintiles SES (=1)	(0.008)			(0.009)
Monthly Income (in %)		-0.009^{**}		-0.012^{***}
		(0.004)		(0.004)
Intervention X		0.019^{***}		0.014^{**}
Low Quintiles Income (=1)		(0.007)		(0.007)
Reasons: Parents' Income (Index)			-0.011^{***}	-0.010^{***}
			(0.002)	(0.002)
Intervention X			0.017^{**}	0.007
Low Quintiles Reasons: P. Income (=1)			(0.008)	(0.009)
Calculated Entitlement (in 100€)	0.006^{***}	0.008^{***}	0.006^{***}	0.005^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
Mean Take-Up - High Quintiles Control	0.015	0.020	0.011	0.007
Mean Take-Up - Low Quintiles Control	0.037	0.029	0.044	0.058
Observations	6,225	6,225	6,225	6,225
R^2	0.038	0.032	0.041	0.046
F Statistic	2.987^{***}	2.474^{***}	3.177^{***}	3.432^{***}

Notes: The table shows results from OLS estimation of the heterogeneity driving variables and interaction terms on a dummy variable equal to 1 if the participant took up student aid since wave 1. All students who were classified as eligible and indicated to have applied for student aid but did not participate in the third wave were imputed to take up. Explanatory variables were selected through causal random forest estimation. The SES-Index gives the socioeconomic status of students constructed using PCA. The index for reasons for non-take-up is constructed using PCA, where the more the students indicated that their parents' income was why they did not apply, the higher the index. The individual income is divided by the average income of the whole sample to show effects in %. Low Quintiles dummies are equal to 1 if the participant ranked below the 40%-quantile on the SES-, the Reasons-Index, or income, respectively. I control for the calculated student aid entitlements. Study field and university fixed effects are included. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Since all variables that drive heterogeneities are related to the student's needs, I also include their calculated student aid entitlement to estimate effects independent of this entitlement, captured by Aid_i. I control for study field and university fixed effects, captured by α_s and γ_u. The regression is estimated for all three heterogeneity-driving variables separately and jointly. Results are shown in Table 5.

The estimation results corroborate the findings from the causal forest predictions. As a result of the intervention, students from the lower SES quintiles are 3 pp more likely to take up student aid, independent of their entitlement. Similarly, students from the lower income quintiles are 1.9 pp more likely to take up student aid, and students who rank low on the index of reasons for non-take-up related to parents' income are 1.7 pp more likely. In all three cases, the intervention effect for the higher quintiles in row 1 becomes insignificant and close to zero. That is, the whole intervention effect on take-up is explained by the groups of students with low SES, low income, and who do not indicate that their parents earn too much for the means-test and support them enough. Including all interaction terms and variables, the effects of low SES and income stay significant. Similar patterns are found for the eligible students and using the stricter take-up definition, reported in Tables C.15 to C.19.[16] This shows that especially students in need of financial support react to the information in the intervention and take up student aid.

To test if take-up of student aid can reduce financial concerns, I use the panel structure of the survey and look at the income changes over time, comparing eligible students who take up aid to those who do not. Results are shown in Table C.20. In line with the heterogeneous intervention effects, I find that students who take up aid start out with significantly lower income in the first wave. While the income of all students significantly increases over time, the increase is stronger for students who take up aid. Non-receivers of aid increase their income from work, which suggests that they take on a job or increase their working hours. Student aid receivers, on the other hand, even decrease their income from work from the first to the third wave. Additionally, they also reduce the monthly support from their parents over time and receive significantly less than the non-receivers. This suggests that take-up not only reduces the students' financial concerns through an increase in total income but also the strain on parents who do not have to support their children as much after take-up.

The intervention contributes to reducing social inequality in higher education, which is the purpose of student aid. By correcting misperceptions, it helps disadvantaged students to realize their eligibility for student aid and alleviates financial distress through take-up. Since students have a lower workload after take-up, they potentially have favorable downstream benefits such as a shorter study time and better grades, as suggested in earlier work (Callender, 2008; Triventi, 2014; Avdic & Gartell, 2015; Bettinger et al., 2019; Black et al., 2023). Additionally, the reduction in parental support indicates that the families also benefit from take-up, in line with Bhargava et al. (2025). Since the intervention is particularly effective for students from low-SES backgrounds, it eases the financial burden on the whole family as the student requires less support. As a result, it addresses social inequality at both the student and household levels.

[16] The respective probit estimations are reported in Tables D.6 to D.11.

5 Conclusion

Student aid aims to reduce social inequality in higher education. Yet, many students do not take up the financial student aid to which they are entitled, resulting in higher dropout rates, higher levels of paid work during their studies, and lower earnings later in life (see Dynarski, Page & Scott-Clayton, 2023, for an overview). One main reason why students do not take up student aid could be that misperceptions about the program led them to underestimate its financial value and question their eligibility. In fact, I show that students systematically underestimate the financial value of student aid, but that concise information about the program conditions and eligibility corrects misperceptions and increases take-up, especially among financially disadvantaged students.

In an experiment with 6,225 non-receivers of student aid embedded into a panel survey of 22,222 university students across Germany, I use hypothetical scenarios to elicit misperceptions about the student aid conditions. Given that Germany has only one federal student aid program, I can focus on this program alone to measure misperceptions and take-up of student aid on a national level. On average, 99.2% of the students underestimate how much financial aid one can receive per month, how much parents can earn for a given entitlement, or overestimate how much must be repaid. Additionally, 86% of the students who are entitled to student aid based on their sociodemographic and economic situation believe they are not eligible.

Providing concise information about these conditions and individual entitlement to a stratified subset of students leads to a significant correction of misperceptions six months later. Additionally, the intervention increased student aid take-up by 1.1 pp (47%) for all students and up to 2.7 pp (68%) for eligible students. The mechanism behind this effect is the correction of misperceptions, which causally increases take-up by up to 55 pp.

Heterogeneity analysis reveals that the intervention was particularly effective among students from lower socioeconomic status and income. Additionally, student aid take-up is associated with higher total income one year after the intervention, but lower income from work and lower financial support from parents. This suggests that take-up not only reduces the students' financial constraints but also relieves their parents. As a consequence, the intervention tackles social inequality at the student and the household levels.

Using national statistics on student aid, a back-of-the-envelope calculation reveals the intervention's potential effect. Providing concise information about the eligibility and repayment conditions of student aid and individual entitlement could increase the total funding available to students by €180 million per year if scaled up to all non-receivers.

The findings show that correcting misperceptions through concise information about student aid conditions and individual entitlement is a powerful mechanism to increase take-up. The intervention could be a feasible and scalable policy to tackle social inequality in higher education. Since disadvantaged students particularly take up aid due to the intervention, the results suggest that correcting students' misperceptions could help them take up their entitlement and achieve better educational and economic outcomes.

References

Abadie, A., S. Athey, G. W. Imbens & J. M. Wooldridge (2022). When Should You Adjust Standard Errors for Clustering? *The Quarterly Journal of Economics* 138(1), 1–35.

Anderson, D. M. & J. K. Holt (2017). *Do High School Students Know Their Parents' Income?* Research Brief. Illinois Education Research Council.

Angrist, J. D., G. W. Imbens & D. B. Rubin (1996). Identification of Causal Effects Using Instrumental Variables. *Journal of the American Statistical Association* 91(434), 444–455.

Athey, S., J. Tibshirani & S. Wager (2019). Generalized random forests. *The Annals of Statistics* 47(2).

Athey, S. & S. Wager (2019). Estimating Treatment Effects with Causal Forests: An Application. *Observational Studies* 5(2), 37–51.

Attanasio, O. P. & K. M. Kaufmann (2014). Education choices and returns to schooling: Mothers' and youths' subjective expectations and their role by gender. *Journal of Development Economics* 109, 203–216.

Avdic, D. & M. Gartell (2015). Working while studying? Student aid design and socioeconomic achievement disparities in higher education. *Labour Economics* 33, 26–40.

Bartoš, V., M. Bauer, J. Cahlíková & J. Chytilová (2022). Communicating doctors' consensus persistently increases COVID-19 vaccinations. *Nature* 606(7914), 542–549.

Becker, K. et al. (2024). *Die Studierendenbefragung in Deutschland (2021).*

Bettinger, E., O. Gurantz, L. Kawano, B. Sacerdote & M. Stevens (2019). The Long-Run Impacts of Financial Aid: Evidence from California's Cal Grant. *American Economic Journal: Economic Policy* 11(1), 64–94.

Bettinger, E. P., B. T. Long, P. Oreopoulos & L. Sanbonmatsu (2012). The Role of Application Assistance and Information in College Decisions: Results from the H&R Block Fafsa Experiment. *The Quarterly Journal of Economics* 127(3), 1205–1242.

Bhargava, P., S. E. Black, J. T. Denning, R. Fairlie & O. Gurantz (2025). A Family Affair: The Effects of College on Parent and Student Finances. *NBER Working Paper.*

Bhargava, S. & D. Manoli (2015). Psychological Frictions and the Incomplete Take-Up of Social Benefits: Evidence from an IRS Field Experiment. *American Economic Review* 105(11), 3489–3529.

Bird, K. A., B. L. Castleman, J. T. Denning, J. Goodman, C. Lamberton & K. O. Rosinger (2021). Nudging at scale: Experimental evidence from FAFSA completion campaigns. *Journal of Economic Behavior & Organization* 183, 105–128.

Black, S. E., J. T. Denning, L. J. Dettling, S. Goodman & L. J. Turner (2023). Taking It to the Limit: Effects of Increased Student Loan Availability on Attainment, Earnings, and Financial Well-Being. *American Economic Review* 113(12), 3357–3400.

Boneva, T., M. Golin & C. Rauh (2022). Can perceived returns explain enrollment gaps in postgraduate education? *Labour Economics* 77, 101998.

Boneva, T. & C. Rauh (2018). Parental Beliefs about Returns to Educational Investments - The Later the Better? *Journal of the European Economic Association* 16(6), 1669–1711.

Booij, A. S., E. Leuven & H. Oosterbeek (2012). The role of information in the take-up of student loans. *Economics of Education Review* 31(1), 33–44.

Cadena, B. C. & B. J. Keys (2013). Can Self-Control Explain Avoiding Free Money? Evidence from Interest-Free Student Loans. *The Review of Economics and Statistics* 95(4), 1117–1129.

Callender, C. (2008). The impact of term-time employment on higher education students' academic attainment and achievement. *Journal of Education Policy* 23(4), 359–377.

Castell, L., M. Gurgand, C. Imbert & T. Tochev (2025). Take-up of Social Benefits: Experimental Evidence from France. *American Economic Journal: Economic Policy*. Forthcoming.

Castleman, B. L. & L. C. Page (2016). Freshman Year Financial Aid Nudges: An Experiment to Increase FAFSA Renewal and College Persistence. *Journal of Human Resources* 51(2), 389–415.

Castleman, B. L. & B. T. Long (2016). Looking beyond Enrollment: The Causal Effect of Need-Based Grants on College Access, Persistence, and Graduation. *Journal of Labor Economics* 34(4), 1023–1073.

Chaisemartin, C. de & J. Ramirez-Cuellar (2024). At What Level Should One Cluster Standard Errors in Paired and Small-Strata Experiments? *American Economic Journal: Applied Economics* 16(1), 193–212.

Chareyron, S., D. Gray & Y. L'Horty (2018). Raising Take-Up of Social Assistance Benefits through a Simple Mailing: Evidence from a French Field Experiment. *Revue d'économie politique* 128(5), 777–805.

Chetty, R., J. N. Friedman, E. Saez, N. Turner & D. Yagan (2020). Income Segregation and Intergenerational Mobility Across Colleges in the United States*. *The Quarterly Journal of Economics* 135(3), 1567–1633.

Cox, J. C., D. Kreisman & S. Dynarski (2020). Designed to fail: Effects of the default option and information complexity on student loan repayment. *Journal of Public Economics* 192, 104298.

Currie, J. (2006). The take-up of social benefits. In: *Public Policy and the Distribution of Income*. Ed. by A. J. Auerbach, J. M. Quigley & D. E. Card. Russell Sage Foundation, 80–148.

Daponte, B. O., S. Sanders & L. Taylor (1999). Why Do Low-Income Households not Use Food Stamps? Evidence from an Experiment. *The Journal of Human Resources* 34(3), 612–628.

Davis, J. M. & S. B. Heller (2017). Using Causal Forests to Predict Treatment Heterogeneity: An Application to Summer Jobs. *American Economic Review* 107(5), 546–550.

Denning, J. T. (2019). Born under a Lucky Star: Financial Aid, College Completion, Labor Supply, and Credit Constraints. *Journal of Human Resources* 54(3), 760–784.

Denning, J. T., B. M. Marx & L. J. Turner (2019). ProPelled: The Effects of Grants on Graduation, Earnings, and Welfare. *American Economic Journal: Applied Economics* 11(3), 193–224.

Destatis (2022). Laufende Wirtschaftsrechnungen. Einkommen, Einnahmen und Ausgaben privater Haushalte. *Wirtschaftsrechnungen* 15(1).

— (2023). *Anteil der Studierenden an privaten Hochschulen auf 12 % gestiegen.* N054. Statistisches Bundesamt.

— (2024). *14 % mehr Ausgaben für BAföG-Leistungen im Jahr 2023.* 297. Statistisches Bundesamt.

Deutscher Bundestag (2021). *Zweiundzwanzigster Bericht nach § 35 des Bundesausbildungsförderungsgesetzes zur Überprüfung der Bedarfssätze, Freibeträge sowie Vomhundertsätze und Höchstbeträge nach § 21 Absatz 2.* Bundesregierung Deutschland.

Domurat, R., I. Menashe & W. Yin (2021). The Role of Behavioral Frictions in Health Insurance Marketplace Enrollment and Risk: Evidence from a Field Experiment. *American Economic Review* 111(5), 1549–1574.

Dynarski, S., C. Libassi, K. Michelmore & S. Owen (2021). Closing the Gap: The Effect of Reducing Complexity and Uncertainty in College Pricing on the Choices of Low-Income Students. *American Economic Review* 111(6), 1721–1756.

Dynarski, S., L. Page & J. Scott-Clayton (2023). College costs, financial aid, and student decisions. In: *Handbook of the Economics of Education.* Ed. by S. Machin, L. Woessmann & E. A. Hanushek. Elsevier, 227–285.

Engström, P., E. Forsell, J. Hagen & A. Stefánsson (2019). Increasing the take-up of the housing allowance among Swedish pensioners: a field experiment. *International Tax and Public Finance* 26(6), 1353–1382.

Eurofound (2015). *Access to social benefits: Reducing non take up.* Publications Office of the European Union.

Fack, G. & J. Grenet (2015). Improving College Access and Success for Low-Income Students: Evidence from a Large Need-Based Grant Program. *American Economic Journal: Applied Economics* 7(2), 1–34.

Fidan, M. & C. Manger (2021). Why do German students reject free money? *Education Economics*, 1–17.

Finkelstein, A. & M. J. Notowidigdo (2019). Take-Up and Targeting: Experimental Evidence from SNAP. *The Quarterly Journal of Economics* 134(3), 1505–1556.

Fuhrmann-Riebel, H., B. D'Exelle, K. López Vargas, S. Tonke & A. Verschoor (2024). Correcting misperceptions about trends and norms to address weak collective action — Experimental evidence from a recycling program. *Journal of Environmental Economics and Management* 128, 103046.

Glocker, D. (2011). The effect of student aid on the duration of study. *Economics of Education Review* 30(1), 177–190.

Goldin, J., T. Homonoff, R. Javaid & B. Schafer (2022). Tax filing and take-up: Experimental evidence on tax preparation outreach and benefit claiming. *Journal of Public Economics* 206, 104550.

Gray, C. (2019). Leaving benefits on the table: Evidence from SNAP. *Journal of Public Economics* 179, 104054.

Haaland, I. & O.-A. E. Næss (2023). Misperceived Returns to Active Investing. *CESifo Working Paper* (10257).

Hair, J. F. (1998). *Multivariate data analysis*. Prentice Hall.

Hanushek, E. A. & L. Woessmann (2015). *The Knowledge Capital of Nations: Education and the Economics of Growth*. The MIT Press.

Herber, S. P. & M. Kalinowski (2019). Non-take-up of student financial aid—A microsimulation for Germany. *Education Economics* 27(1), 52–74.

Hoxby, C. M. & S. Turner (2015). What High-Achieving Low-Income Students Know about College. *American Economic Review: Papers & Proceedings* 105(5), 514–17.

Ihlanfeldt, K. (2021). Property Tax Homestead Exemptions: An Analysis of the Variance in Take-Up Rates Across Neighborhoods. *National Tax Journal* 74(2), 405–430.

Imbens, G. W. & J. D. Angrist (1994). Identification and Estimation of Local Average Treatment Effects. *Econometrica* 62(2), 467.

Jensen, R. (2010). The (Perceived) Returns to Education and the Demand for Schooling. *The Quarterly Journal of Economics* 125(2), 515–548.

Kacker, S., M. Macis, P. Gajwani & D. S. Friedman (2022). Providing vouchers and value information for already free eye exams increases uptake among a low-income minority population: A randomized trial. *Health Economics* 31(3), 541–551.

Kaufmann, K. M. (2014). Understanding the income gradient in college attendance in Mexico: The role of heterogeneity in expected returns: Income gradient in college attendance. *Quantitative Economics* 5(3), 583–630.

Ko, W. & R. A. Moffitt (2022). Take-Up of Social Benefits. In: *Handbook of Labor, Human Resources and Population Economics*. Ed. by K. F. Zimmermann. Springer International Publishing, 1–43.

Kofoed, M. S. (2017). To Apply or Not to Apply: FAFSA Completion and Financial Aid Gaps. *Research in Higher Education* 58(1), 1–39.

Kofoed, M. S. (2022). Pell Grants and Labor Supply: Evidence from a Regression Kink. *Upjohn Institute working paper.*

Kroher, M. et al. (2023). *Die Studierendenbefragungin Deutschland: 22. Sozialerhebung. Die wirtschaftliche und soziale Lage der Studierenden in Deutschland 2021.* Bundesministerium für Forschung und Bildung.

Liebman, J. B. & E. F. P. Luttmer (2015). Would People Behave Differently If They Better Understood Social Security? Evidence from a Field Experiment. *American Economic Journal: Economic Policy* 7(1), 275–99.

Manski, C. F. (2004). Measuring Expectations. *Econometrica* 72(5), 1329–1376.

Marx, B. M. & L. J. Turner (2020). Paralysis by analysis? Effects of information on student loan take-up. *Economics of Education Review* 77, 102010.

Middendorff, E., B. Apolinarski, K. Becker, P. Bornkessel, T. Brandt, S. Heißenberg & J. Poskowsky (2017). *Die wirtschaftliche und soziale Lage der Studierenden in Deutschland 2016. 21. Sozialerhebung des Deutschen Studentenwerks durchgeführt vom Deutschen Zentrum für Hochschul- und Wissenschaftsforschung.* Bundesministerium für Forschung und Bildung.

Murphy, R. & G. Wyness (2023). Testing Means-Tested Aid. *Journal of Labor Economics* 41(3), 687–727.

Nguyen, H. T., H. T. Le & L. B. Connelly (2020). Who's declining the "free lunch"? New evidence from the uptake of public child dental benefits. *Health Economics* 30(2), 270–288.

Nguyen, T. D., J. W. Kramer & B. J. Evans (2019). The Effects of Grant Aid on Student Persistence and Degree Attainment: A Systematic Review and Meta-Analysis of the Causal Evidence. *Review of Educational Research* 89(6), 831–874.

Park, R. S. E. & J. Scott-Clayton (2018). The Impact of Pell Grant Eligibility on Community College Students' Financial Aid Packages, Labor Supply, and Academic Outcomes. *Educational Evaluation and Policy Analysis* 40(4), 557–585.

Peter, F., C. K. Spiess & V. Zambre (2021). Informing students about college: Increasing enrollment using a behavioral intervention? *Journal of Economic Behavior & Organization* 190, 524–549.

Peter, F. H. & V. Zambre (2017). Intended college enrollment and educational inequality: Do students lack information? *Economics of Education Review* 60, 125–141.

Pham, A. (2019). Firm Take-Up of a Corporate Income Tax Cut. *National Tax Journal* 72(3), 575–598.

Reuben, E., M. Wiswall & B. Zafar (2017). Preferences and Biases in Educational Choices and Labour Market Expectations: Shrinking the Black Box of Gender. *The Economic Journal* 127(604), 2153–2186.

Schneider, S. O. & M. Schlather (2017). A new approach to treatment assignment for one and multiple treatment groups. *CRC Discussion Papers* (228).

Serra-Garcia, M. & N. Szech (2023). Incentives and Defaults Can Increase COVID-19 Vaccine Intentions and Test Demand. *Management Science* 69(2), 1037–1049.

Stantcheva, S. (2023). How to Run Surveys: A Guide to Creating Your Own Identifying Variation and Revealing the Invisible. *Annual Review of Economics* 15(1), 205–234.

Triventi, M. (2014). Does working during higher education affect students' academic progression? *Economics of Education Review* 41, 1–13.

Wager, S. & S. Athey (2018). Estimation and Inference of Heterogeneous Treatment Effects using Random Forests. *Journal of the American Statistical Association* 113(523), 1228–1242.

Appendix A: Additional Technical Explanations

A.1 Stratification of the Information Intervention

The information intervention was stratified at the cohort level. That is, I created a list with all public universities in Germany, how many students are enrolled there, in which federal state they are, if it is a general university or has a technical or other specialization, and what distributional channels for inviting participants was agreed upon with their respective general student committee. Next, I used the minMSE approach (Schneider & Schlather, 2017) to match universities and create two balanced groups considering the mentioned information.

Additionally, I created two groups out of the 18 study fields in Germany[17] that each comprise approximately 50% of the student population while considering that some fields have overlapping courses. For example, mechanical and electrical engineering are selected into the same group due to their content-related overlap. The control and treatment groups are constructed based on the university and study field groups. In the first university group, the first cohort[18] of the first study field group is assigned to treatment while the second cohort[19] is not. Analogously, for the second university group, the first cohort of the first study field group is assigned to control while the second cohort is not, and so forth for each cohort of each study field and university. Therefore, spillovers are minimized since students from the same cohort of a given study field and university are assigned to the same group. At the same time, treatment is still distributed balancedly across universities, study fields, and cohorts.

A.2 Construction of the SES-Index and the Reasons-Indices

Before I apply the causal forest algorithm to analyze heterogeneous treatment effects of the information intervention, I use principal component analysis to construct an index for the socioeconomic status of students. I include monthly income in €, monthly parents' income in € in log-terms, confidence in parents' income, an indicator that is equal to 1 if parents are separated, an indicator for being a half-orphan, an indicator for believing that parents are relatively poor, migration background[20], potential civil servant status of parents and parents'

[17] The 18 fields are: Agricultural Sciences, Construction and Architecture, Biology and Chemistry, Electrical Engineering, Geosciences and Physics, Health Sciences, Medicine, Art, Mathematics and Computer Science, Mechanical Engineering, Pedagogy, Psychology, Law, Social Sciences, Linguistics and Cultural Sciences, Industrial Engineering, Economic Sciences, No clear allocation possible.

[18] Students in the first and second semester.

[19] Students in the third and fourth semester.

[20] Migration background is 0 if both the student and their parents were born in Germany, 1 if one out the three was born outside of Germany, 2 if two of them were born outside of Germany, and 3 if all were born

educational background[21]. The PCA yields that there is one principal component, which is used to construct the index. Using a cutoff of ± 0.3 for the factor loadings (Hair, 1998), the SES-Index comprises parents' income with the highest weight, followed by the belief that parents are relatively poor compared to other families, migration background, parents' education, and the half-orphan indicator. A higher SES-Index corresponds to a higher SES.

Analogously, I use PCA as a dimension reduction technique to comprise different reasons for non-take-up of student aid that students indicated on 5-point Likert scales. The PCA yields three components where the first captures reasons that are application or student aid program related. This index comprises the reasons "I do not want to be seen as a BAföG receiver", "I cannot provide the necessary certificate of performance", "I do not want to take on any debt", "The application is too time-consuming/complex", "My family situation is too complex for a BAföG application", "I do not wish to disclose any income information", and "I do not want to receive money from the state". The second index captures reasons that are related to their parents' income being too high for eligibility. The reasons are: "My parents have said that their income is too high", "I have realized myself that my parents' income is too high", and "I get enough financial support from my parents". The third index captures reasons that are related to the student's own financial situation. The reasons are: "My spouse's income is too high", "I have too much income myself", "I cannot receive BAföG due to previous training(s)", and "I have too many assets". The weights of the reasons that construct the three components are similarly high. Higher values in these components correspond to a higher agreement on the respective reasons why one has not applied for student aid so far. The two reasons "My application in the past was denied" and "The expected funding amount is positive but so low that it is not worth the effort" did not load on any of the three components and are therefore included separately in the analysis. The SES-Index and the three components of non-take-up reasons are used for the causal forest analysis instead of the variables they comprise.

A.3 Assumptions for Estimating the LATE

To estimate the local average treatment effect (LATE), five assumptions must be fulfilled (Angrist, Imbens & Rubin, 1996). These assumptions are discussed in the following. All assumptions are fulfilled.

1. **SUTVA**[22]: An individual's outcome is not affected by the treatment assigned to others.

outside of Germany.

[21] Parents' education is 0 if both parents do not have a university degree, 1 if one of them has a university degree, and 2 if both have a university degree.

[22] Stable unit treatment value assumption.

As explained in Section 4.2 and shown in Table C.13, I do not find any evidence for spillovers of the treatment, so the SUTVA holds.

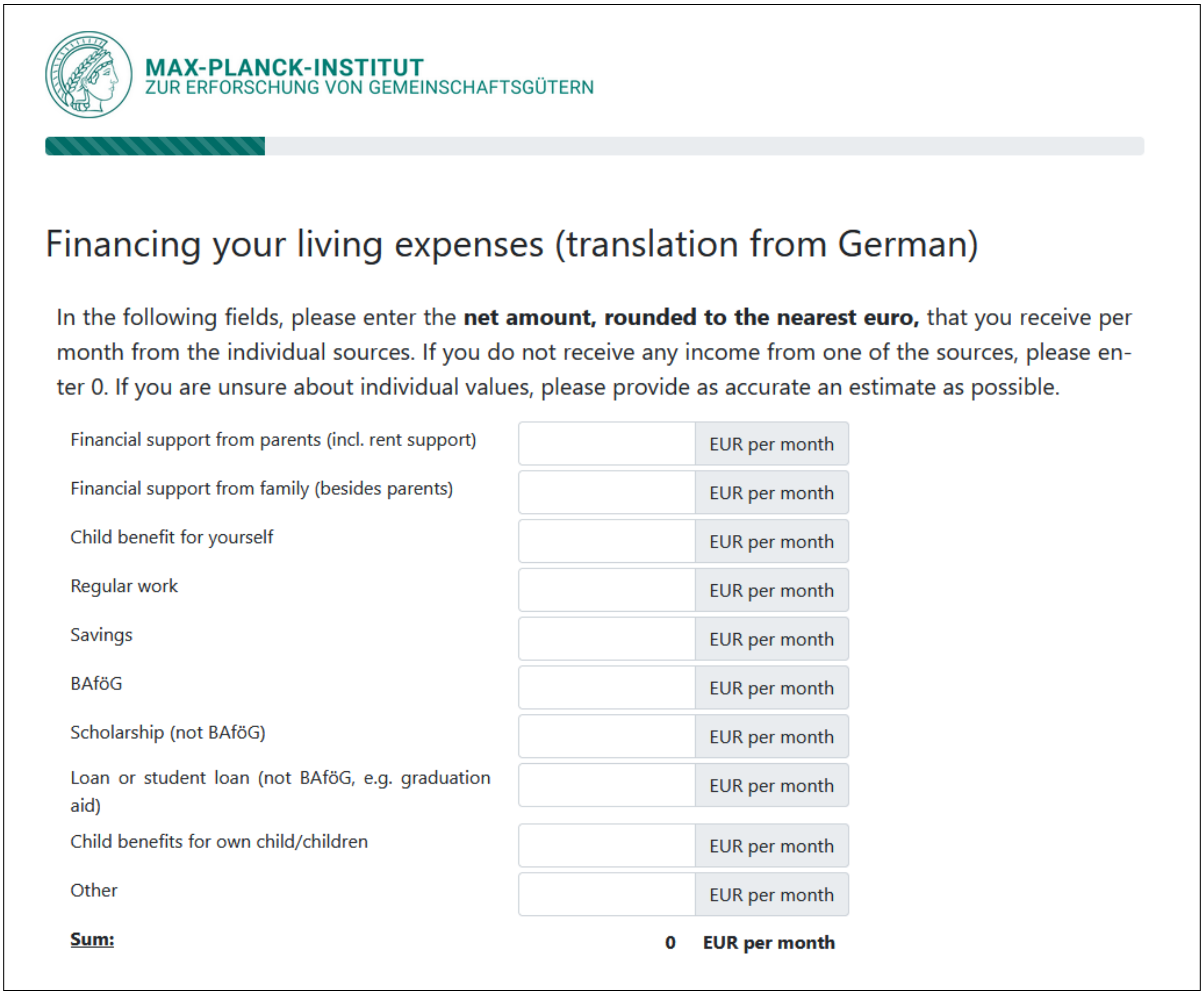
2. **Independence**: Random assignment of the treatment.
   The information intervention was stratified, and the control and treatment group are balanced, shown in Table 1.

3. **Exclusion Restriction**: Treatment only affects take-up by correcting misperceptions.
   Given that the treatment is an information intervention that aims to correct misperceptions, it is unlikely that it increases take-up any other way than by correcting misperceptions about the student aid conditions and own eligibility.

4. **First Stage**: The intervention significantly corrects misperceptions.
   As shown in Tables 2 and 3, the intervention significantly corrects misperceptions both on student aid conditions and individual eligibility.

5. **Monotonicity**: The intervention only corrects misperceptions and does not worsen them.
   This is true by design for eligibility misperceptions, as I only look at misperceivers in the first place. For misperceptions about student aid conditions, we see a positive effect on corrections for underestimators and no effect for overestimators. Yet, since the effect is positive or zero but not negative, monotonicity is fulfilled.

Appendix B: Survey Screenshots



MAX-PLANCK-INSTITUT
ZUR ERFORSCHUNG VON GEMEINSCHAFTSGÜTERN

Financing your living expenses (translation from German)

In the following fields, please enter the **net amount, rounded to the nearest euro,** that you receive per month from the individual sources. If you do not receive any income from one of the sources, please enter 0. If you are unsure about individual values, please provide as accurate an estimate as possible.

Financial support from parents (incl. rent support)		EUR per month
Financial support from family (besides parents)		EUR per month
Child benefit for yourself		EUR per month
Regular work		EUR per month
Savings		EUR per month
BAföG		EUR per month
Scholarship (not BAföG)		EUR per month
Loan or student loan (not BAföG, e.g. graduation aid)		EUR per month
Child benefits for own child/children		EUR per month
Other		EUR per month
Sum:	**0**	**EUR per month**

Figure B.1: Question on student's income per month.

MAX-PLANCK-INSTITUT
ZUR ERFORSCHUNG VON GEMEINSCHAFTSGÜTERN

Information about your family background (translation from German)

Please estimate approximately what **net income** your parent 1 has in total per month.

- No income
- Up to 500€
- Over 500€ to 1000€
- Over 1000€ to 1500€
- Over 1500€ to 2000€
- Over 2000€ to 2500€
- Over 2500€ to 3000€
- Over 3000€ to 4000€
- Over 4000€ to 5000€
- Over 5000€ to 6000€
- Over 6000€
- I cannot estimate this

How sure are you about this answer?
Please click on the bar to select.

0 % 100 %

Figure B.2: Question on parent's income and confidence.

MAX-PLANCK-INSTITUT
ZUR ERFORSCHUNG VON GEMEINSCHAFTSGÜTERN

Reasons against BAföG-application (translated from German)

Please enter the reasons why you so far did not apply for BAföG this semester/study year.
Tick the extent to which the reasons apply to you or not. You can select several reasons that apply to you.
If you are taking part in the survey from your smartphone, please use the landscape format for this question.

	Applies	**Rather applies**	**Rather does not apply**	**Does not apply**	**Cannot make a clear statement**
I get enough financial support from my parents					
My spouse's income is too high					
I cannot provide the necessary certificate of performance					
The expected funding amount is positive but so low that it is not worth it					
My family situation is too complex for a BAföG application					
I do not want to receive money from the state					
I have too many assets (e.g. car/savings account)					
I have realized myself that my parents' income is too high					
My application in the past was declined					
I cannot receive BAföG due to previous training(s)					
My parents have said that their income is too high					
I have too much income myself (through work and/or scholarship)					
Application process is too time-consuming / application is too complex					
I do not want to take on any debt					
I do not want to be seen as a BAföG receiver					
I do not wish to disclose any income information about myself and/or my parents to the BAföG office					
Other:					

Figure B.3: Question on reasons against applying for student aid.

MAX-PLANCK-INSTITUT
ZUR ERFORSCHUNG VON GEMEINSCHAFTSGÜTERN

Information about BAföG (translated from German)

Have you ever looked into BAföG?
A married couple with 2 children can earn up to €120,000 gross per year and the children can still receive BAföG!

Here is some information about BAföG:

- **Parents' income thresholds:**
  Parents or single parents with two children may earn up to approx. €120,000 gross per year for the children's BAföG entitlement. For one child, the income threshold is around €85,000 gross per year. Your own BAföG entitlement increases with each sibling in training.
- **BAföG amount:**
  Last winter semester, the maximum BAföG rate was raised to €934 per month. With parental health insurance, the maximum rate is €812 per month.
- **Repayment:**
  Only half of the BAföG must be repaid, but never more than €10,010. The repayment can be spread over several years to avoid financial hardship or can be repaid in one lump sum with a discount of up to 21%.
- **Assets and own income:**
  Assets up to €15,000 (e.g. account balance) are not taken into account. Anything above this amount reduces the BAföG entitlement, but does not invalidate it.
  The same applies to your own income. A €520 job is exactly at the limit and is not taken into account. Any net income in excess of this is deducted from the BAföG entitlement.
- **Age:**
  Any student who is not older than 45 at the start of their studies can apply for BAföG.

Even if the application process can be time-consuming, it's worth it!
Click here for the official homepage and here for the BAföG-application.
Most student representative bodies at universities offer free BAföG counseling. If you would like more information or are interested in an informal exchange, you can also contact us via e-mail at an bafoeg@coll.mpg.de.

Figure B.4: Screenshot of the information intervention page 1.

MAX-PLANCK-INSTITUT
ZUR ERFORSCHUNG VON GEMEINSCHAFTSGÜTERN

Information about BAföG (translated from German)

ATTENTION:

With the information you have provided in this study, you could receive between **X€** and **Y€** BAföG per month!
[In case the student fulfills requirements]
You could also be eligible for parent-independent BAföG due to your age and/or completed initial training.

[In case of non-positive student aid estimation] Unfortunately, we were unable to determine an individual BAföG estimate based on your information.
However, your parents can have a combined income of X€ to Y€ **net** per month without you losing your possible BAföG entitlement.
[In case the student fulfills requirements]
You could also be eligible for parent-independent BAföG due to your age and/or completed initial training.

This information is without guarantee!
The actual amount of BAföG depends on the individual case and is based on the actual income and family situation.

In order to secure a possible BAföG entitlement for May, you must submit an informal application to your BAföG office this month, as no BAföG is paid retroactively for the period before the first application.

If you would like more information or are interested in an informal exchange, you can also contact us via e-mail at bafoeg@coll.mpg.de.

Figure B.5: Screenshot of the information intervention page 2.

Appendix C: Additional Results

Table C.1: Correct Values of Misperception Elicitation Scenarios

Scenario	Correct Value in €
Amounts of Student Aid	
Basis	762
Mother's income €20,000	341
Assets of €18,000	512
Income Thresholds for Parents	
Basis	50,000
Studying sister	74,000
Repayment Amounts	
Basis	4,500
Total aid of €30,000	10,010
Repayment in one sum	3,960

Notes: The table shows the correct values of each question asked for the misperception elicitation using hypothetical scenarios.

Table C.2: Summary Statistics - Participants after First Wave

	Non-Receivers (N=12296)		Receivers (N=9573)		Diff. t-test
Variable	Mean	SD	Mean	SD	p-value
Age	24.300	3.940	24.949	4.322	0.000
Female (=1)	0.628	0.483	0.657	0.475	0.000
Monthly Income in €	1047.316	558.276	1119.176	508.326	0.000
Monthly Student Aid in €	0.000	0.000	497.283	359.016	0.000
Single (=1)	0.962	0.191	0.961	0.194	0.611
Study year	3.601	1.912	3.528	1.912	0.005
Lives with parents (=1)	0.161	0.367	0.113	0.316	0.000
East Germany (=1)	0.186	0.389	0.264	0.441	0.000
Consent for Recontact (=1)	0.787	0.410	0.832	0.374	0.000
Misperception Area (in €)					
Amounts of Student Aid	-261.865	228.496	-256.251	213.452	0.061
Income Thresholds for Parents	-16678.78	24253.94	-13298.53	24635.19	0.000
Repayment Amounts	2867.914	4457.681	1456.999	3059.225	0.000

Notes: The table shows the summary statistics of the student aid receivers and non-receivers after the first wave of data collection. The last column shows the p-value corresponding to two-sided t-tests of the means of each group, respectively.

Table C.3: Differences between Potential and Experimental Sample

	Potential Sample (N=9216)		Experimental Sample (N=6225)		Diff. t-test
Variable	Mean	SD	Mean	SD	p-value
Info-Intervention (=1)	0.474	0.499	0.476	0.499	0.820
Age	24.314	3.910	24.300	3.948	0.830
Female (=1)	0.622	0.485	0.623	0.485	0.889
Monthly Income in Wave 1 in €	1043.772	498.171	1046.862	496.079	0.706
Single (=1)	0.962	0.192	0.964	0.185	0.345
Study year	3.616	1.912	3.645	1.905	0.346
Lives with parents (=1)	0.164	0.370	0.161	0.368	0.732
East Germany (=1)	0.182	0.386	0.180	0.384	0.753
Believes to be eligible (=1)	0.099	0.299	0.089	0.284	0.024
Misperception Area (in €)					
Amounts of Student Aid	-261.176	226.052	-264.599	220.249	0.349
Income Thresholds for Parents	-16581.55	24274.65	-15413.89	23920.48	0.003
Repayment Amounts	2843.178	4406.360	2827.063	4237.863	0.820

Notes: The table shows the summary statistics of the potential sample of non-receivers who participated in wave 1 and those who participated again in wave 2 and, therefore, comprise the experimental sample. Only non-receivers could participate in the experiment since they did not apply for student aid before the survey. The last column shows the p-value corresponding to two-sided t-tests of the means of each group, respectively.

Table C.4: Representativeness of Experimental Sample

	Experimental Sample (N=6,225)		Representative Data (N=163,272)		Diff. t-test
Variable	Mean	SD	Mean	SD	p-value
Age	24.300	3.948	24.594	3.845	0.000
Female (=1)	0.623	0.485	0.500	0.500	0.000
Monthly Income in €	1046.862	494.083	1057.148	1206.954	0.201
Migration Background (=1)	0.204	0.403	0.195	0.396	0.109
Single (=1)	0.964	0.185	0.900	0.299	0.000
Study year	3.645	1.905	3.309	1.961	0.000
Lives with parents (=1)	0.161	0.368	0.263	0.440	0.000
East Germany (=1)	0.180	0.384	0.180	0.384	0.993

Notes: The table shows the summary statistics of the experimental sample and representative data for students in Germany in 2021 (Becker et al., 2024). The representative data were constructed the same way as the experimental sample: student aid receivers and students ineligible for student aid for administrative reasons were dropped. The last column shows the p-value corresponding to two-sided t-tests of the means of each group, respectively.

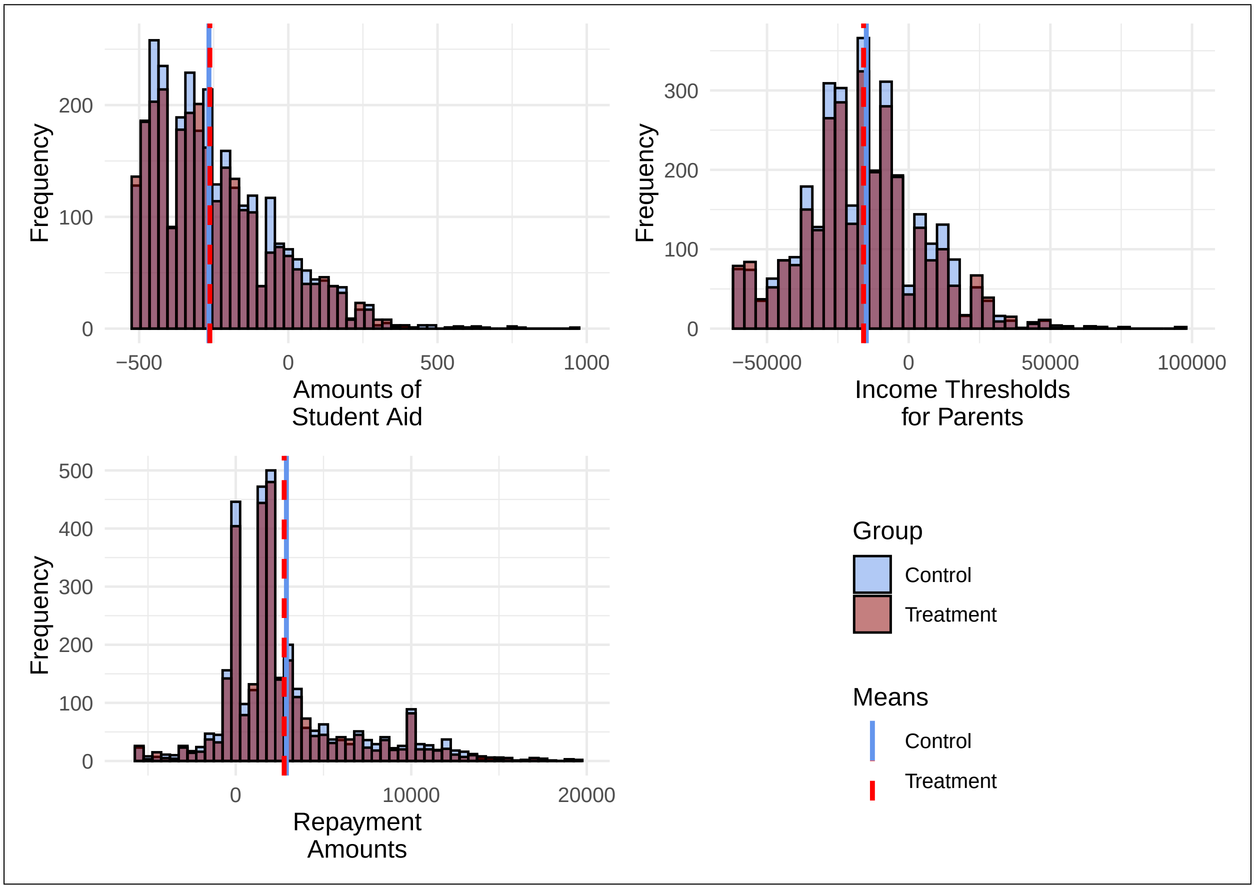


Figure C.1: Distribution of Average Misperceptions per Area.

Table C.5: Intervention Effect on Difference in Misperceptions from 1st to 2nd Wave

	Correction of Misperceptions (in %)				
	Amounts of Student Aid	Income Thresh. for Parents	Repayment Amounts	Pooled Domains	Total Number
	(1)	(2)	(3)	(4)	(5)
Info-Intervention (=1)	0.037***	0.013	0.144***	0.058**	0.040***
	(0.008)	(0.009)	(0.042)	(0.024)	(0.010)
Intervention X Overest. Financial Value of Student Aid W1 (=1)	−0.034**	−0.027*	−0.138***	−0.036	−0.019*
	(0.016)	(0.014)	(0.048)	(0.025)	(0.011)
Overestimated Financial Value of Student Aid W1 (=1)	0.046***	0.018*	0.260***	0.072***	0.046***
	(0.014)	(0.010)	(0.039)	(0.017)	(0.008)
Mean (Control Group Underest.)	0.091	0.085	0.254	0.180	0.113
Observations	6,225	6,225	6,225	6,225	6,225
R^2	0.373	0.493	0.391	0.370	0.354
F Statistic	25.323***	41.360***	27.286***	24.966***	23.332***

Notes: The table shows the intervention effects on the correction of misperceptions from the first to the second wave. Misperceptions are measured as the absolute deviation from the correct values in the elicitation scenarios, divided by these correct values to determine the misperceptions in %. Misperceptions are averaged per area for columns (1)-(3), and over all areas for column (4). Column (5) uses the total number of misperceptions, measured as the number of answers to the elicitation scenarios outside the incentivized interval as explained in Section 3.2. The outcome is the correction in misperceptions, calculated as first-wave minus second-wave misperceptions, such that positive coefficients show a stronger correction of misperceptions. *Overestimated* is equal to 1 if the participant overestimated the correct value of the misperception elicitation for at least one question per elicitation scenario. For the area "repayment amounts", the variable equals 1 if the participant underestimated at least one correct value, respectively. The positive coefficients in row 1 show that the intervention reduced misperceptions for the participants who underestimated the financial value of student aid significantly.
I control for misperceptions in the first wave, all sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table C.6: Intervention Effect on Correction of Misperceptions from 1st to 2nd Wave (Avg.

	Correction of Misperceptions (in %)				
	Amounts of Student Aid	Income Thresh. for Parents	Repayment Amounts	Pooled	Total Number
	(1)	(2)	(3)	(4)	(5)
Info-Intervention (=1)	0.027***	0.012	0.053***	0.027***	0.029***
	(0.008)	(0.007)	(0.018)	(0.009)	(0.005)
Intervention X Overest. Financial Value of Student Aid W1 (=1)	−0.023	−0.050***	−0.078	−0.003	−0.016**
	(0.025)	(0.016)	(0.058)	(0.016)	(0.008)
Overestimated Financial Value of Student Aid W1 (=1)	0.047**	0.025**	0.006	-0.002	0.015***
	(0.020)	(0.011)	(0.037)	(0.012)	(0.006)
Mean Outcome Reference	0.042	0.029	0.033	0.045	0.040
Observations	6,225	6,225	6,225	6,225	6,225
R^2	0.373	0.493	0.383	0.368	0.352
F Statistic	25.254***	41.425***	26.376***	24.718***	23.077***

Notes: The table shows the intervention effects on the correction of misperceptions from the first to the second wave. Misperceptions are measured as the absolute deviation from the correct values in the elicitation scenarios, divided by these correct values to determine the misperceptions in %. Misperceptions are averaged per area for columns (1)-(3), and over all areas for column (4). Column (5) uses the total number of misperceptions, measured as the number of answers to the elicitation scenarios outside the incentivized interval as explained in Section 3.2. The outcome is the correction in misperceptions, calculated as first-wave minus second-wave misperceptions, such that positive coefficients show a stronger correction of misperceptions. *Overestimated* is equal to 1 if the participant on average overestimated the correct value of the respective misperception elicitation scenario. For the area "repayment amounts", the variable is equal to 1 if the participant underestimated the average correct value, respectively. The positive coefficients in row 1 show that the intervention reduced misperceptions for the participants who underestimated the financial value of student aid significantly.
I control for misperceptions in the first wave, all sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table C.7: Intervention Effect on Misperceptions About Own Eligibility

	Correction of Eligibility Misperceptions (Intensive, in %)			
	Eligibility calculation: without own income		Eligibility calculation: with own income	
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.035***	0.031**	0.046***	0.047***
	(0.012)	(0.012)	(0.014)	(0.014)
Constant	0.058***	0.194	0.062***	0.211
	(0.008)	(0.160)	(0.010)	(0.196)
Study Field FE	No	Yes	No	Yes
University FE	No	Yes	No	Yes
Observations	2,361	2,361	1,786	1,786
R^2	0.004	0.092	0.007	0.101
F Statistic	9.472***	1.746***	12.324***	1.483***

Notes: The table shows the intervention effects on the correction of misperceptions about the participants' own eligibility for student aid from the first to the second wave. Only participants are considered who are classified as eligible for student aid and misperceive this eligibility in wave 1, so participants that do not believe to be eligible, hence answer the Likert scale question on perceived eligibility in wave 1 with "Rather No", "Definitely No", or "Cannot give a clear answer". The difference between answers to the perceived eligibility question from the first to the second wave is used as outcome, divided by 4 to represent percentage terms. Every student who applied is assumed to definitely think they are eligible. That is, a student who answered "Definitely No" in the first wave and "Definitely Yes" in the second wave or applied for student aid has a correction of 1. To determine eligibility, the student's sociodemographic and economic situation excluding their own income is used for columns (1) and (2), and including their income for columns (3) and (4).
I control for all misperceptions from the scenarios, confidence in these answers, sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table C.8: Intervention Effect on Student Aid Take-Up

	Take-Up of Student Aid (=1)		
	(1)	(2)	(3)
Info-Intervention (=1)	0.011^{***}	0.010^{***}	0.010^{***}
	(0.004)	(0.004)	(0.004)
Constant	0.024^{***}	0.196^{**}	0.181^{***}
	(0.002)	(0.058)	(0.061)
Controls	No	Yes	Yes
Study Field FE	No	No	Yes
University FE	No	No	Yes
Observations	6,225	6,225	6,225
R^2	0.001	0.068	0.079
F Statistic	6.580^{**}	7.568^{***}	3.855^{***}

Notes: The table shows the intervention effect on take-up rates. Every student who indicated to receive student aid in wave 2 or wave 3 or with a successful application is considered for take-up. Additionally, students classified as eligible based on their sociodemographic and economic situation, excluding their own income, who applied for student aid but did not have the final decision in wave 2 and did not participate in wave 3 were imputed to take up. The positive coefficients in row 1 show that the intervention led to significantly higher application rates by 1.0-1.1 pp.

I control for misperceptions per area in the first wave, all sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects. Coefficients for these variables are presented in Table C.9. Clustered standard errors are in parentheses.

*p<0.1; **p<0.05; ***p<0.01

Table C.9: Intervention Effect on Student Aid Take-Up (extended)

	Take-Up of Student Aid (=1)			
	Imputed		Non-Imputed	
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.010***	0.010***	0.007**	0.008**
	(0.004)	(0.004)	(0.003)	(0.003)
Misp. Amounts of Student Aid	−0.007	−0.007	−0.003	−0.003
in W1 (in %)	(0.009)	(0.008)	(0.008)	(0.008)
Confidence Misp. Amounts of Student Aid	−0.002	−0.003	0.001	0.0002
	(0.014)	(0.014)	(0.014)	(0.014)
Misp. Income Thresholds for Parents	0.008	0.008	0.006	0.006
in W1 (in %)	(0.006)	(0.007)	(0.006)	(0.006)
Confidence Misp. Income Thresholds	0.001	0.004	−0.007	−0.003
for Parents	(0.017)	(0.017)	(0.017)	(0.018)
Misp. Repayment Amounts in W1 (in %)	−0.0003	−0.001	0.0001	−0.0002
	(0.004)	(0.004)	(0.004)	(0.004)
Confindence Misp. Repayment Amounts	0.022**	0.021**	0.021**	0.019*
	(0.011)	(0.011)	(0.010)	(0.010)
Age	0.001	0.001	0.0002	0.0001
	(0.001)	(0.001)	(0.001)	(0.001)
Female (=1)	0.0001	−0.002	0.001	−0.002
	(0.004)	(0.004)	(0.005)	(0.005)
Married (=1)	−0.008	−0.011	−0.0001	−0.003
	(0.016)	(0.015)	(0.015)	(0.015)
Lives with parents (=1)	−0.015	−0.017	−0.011	−0.014
	(0.011)	(0.010)	(0.011)	(0.010)
East Germany (=1)	−0.006	0.006	−0.006	−0.002
	(0.006)	(0.015)	(0.005)	(0.015)
Master (=1)	0.003	0.009	−0.00003	0.005
	(0.007)	(0.008)	(0.007)	(0.008)
Second training (=1)	0.005	0.003	0.005	0.003
	(0.007)	(0.007)	(0.006)	(0.006)
Log(Monthly Income in Wave 1 in €)	−0.021***	−0.022***	−0.017***	−0.019***
	(0.007)	(0.007)	(0.006)	(0.006)
Study Field FE	No	Yes	No	Yes
University FE	No	Yes	No	Yes
Observations	6,225	6,225	6,225	6,225
R^2	0.068	0.079	0.061	0.073
F Statistic	7.568***	3.855***	6.814***	3.548***

Notes: Continued on next page.

Table C.9: Intervention Effect on Student Aid Take-Up (extended) (contd.)

	Take-Up of Student Aid (=1)			
	Imputed		Non-Imputed	
	(1)	(2)	(3)	(4)
Log(Parents' monthly net income in €)	0.0003	−0.001	0.001	0.0003
	(0.005)	(0.005)	(0.005)	(0.005)
Confidence parents' Income	−0.0003	−0.002	−0.001	−0.003
	(0.011)	(0.011)	(0.010)	(0.010)
Parents handle finances (=1)	0.015	0.017	0.002	0.005
	(0.015)	(0.015)	(0.011)	(0.012)
Parents separate (=1)	−0.0001	−0.0004	−0.001	−0.001
	(0.005)	(0.005)	(0.005)	(0.005)
Half-orphan (=1)	0.008	0.007	0.013	0.013
	(0.013)	(0.013)	(0.013)	(0.013)
Knows receivers (=1)	−0.004	−0.003	−0.004	−0.003
	(0.004)	(0.004)	(0.004)	(0.004)
Believes parents are poor (=1)	0.014	0.014	0.014	0.014
	(0.010)	(0.010)	(0.010)	(0.010)
Num. of siblings	0.001	0.0004	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Study year	−0.004**	−0.005**	−0.003	−0.003*
	(0.002)	(0.002)	(0.002)	(0.002)
Moves out from parents (=1)	0.041**	0.045**	0.042**	0.046**
	(0.019)	(0.019)	(0.019)	(0.019)
Moves in to parents (=1)	−0.035***	−0.034***	−0.031***	−0.031***
	(0.006)	(0.006)	(0.005)	(0.005)
GPA	−0.0004	−0.00000	−0.002	−0.001
	(0.003)	(0.004)	(0.003)	(0.004)
Born outside Germany (=1)	−0.021*	−0.020	−0.029**	−0.028**
	(0.013)	(0.013)	(0.013)	(0.013)
Both parents born outside Germany (=1)	0.022*	0.023*	0.016	0.018
	(0.012)	(0.012)	(0.012)	(0.012)
Some parent born outside Germany (=1)	−0.004	−0.004	−0.001	−0.002
	(0.008)	(0.008)	(0.008)	(0.008)
Both parents civil servants (=1)	−0.003	−0.003	−0.001	−0.0003
	(0.008)	(0.008)	(0.008)	(0.008)
Some parent civil servant (=1)	−0.002	−0.003	−0.003	−0.004
	(0.005)	(0.005)	(0.004)	(0.005)
Study Field FE	No	Yes	No	Yes
University FE	No	Yes	No	Yes
Observations	6,225	6,225	6,225	6,225
R^2	0.068	0.079	0.061	0.073
F Statistic	7.568***	3.855***	6.814***	3.548***

Notes: Continued on next page.

Table C.9: Intervention Effect on Student Aid Take-Up (extended) (contd.)

	Take-Up of Student Aid (=1)			
	Imputed		Non-Imputed	
	(1)	(2)	(3)	(4)
Both parents college degree (=1)	−0.002	−0.001	−0.002	−0.001
	(0.004)	(0.004)	(0.004)	(0.004)
Some parent college degree (=1)	−0.014**	−0.013**	−0.014**	−0.013**
	(0.006)	(0.006)	(0.006)	(0.006)
No longer student (=1)	0.002	0.004	0.003	0.004
	(0.006)	(0.006)	(0.006)	(0.006)
Believes to be eligible (=1)	0.100***	0.100***	0.090***	0.090***
	(0.013)	(0.013)	(0.013)	(0.012)
Reason: Stigma (=1)	−0.007	−0.006	−0.003	−0.002
	(0.012)	(0.013)	(0.012)	(0.012)
Reason: Parents said so (=1)	−0.012**	−0.011**	−0.011**	−0.010**
	(0.005)	(0.005)	(0.005)	(0.005)
Reason: Found out myself (=1)	0.001	−0.0002	0.002	0.001
	(0.005)	(0.005)	(0.004)	(0.004)
Reason: Partners' income (=1)	−0.003	−0.004	−0.006	−0.007
	(0.012)	(0.012)	(0.010)	(0.010)
Reason: Not enough ECTS (=1)	−0.016**	−0.015**	−0.016**	−0.015**
	(0.007)	(0.007)	(0.007)	(0.007)
Reason: Debt aversion (=1)	−0.016***	−0.016***	−0.015***	−0.015***
	(0.005)	(0.005)	(0.005)	(0.005)
Reason: Own income (=1)	−0.007	−0.007	−0.008	−0.008
	(0.006)	(0.006)	(0.006)	(0.006)
Reason: Complexity (=1)	0.001	0.001	0.004	0.004
	(0.005)	(0.005)	(0.004)	(0.004)
Reason: Application denied (=1)	−0.002	−0.001	−0.005	−0.004
	(0.007)	(0.007)	(0.006)	(0.007)
Reason: Second training (=1)	−0.009	−0.008	−0.009	−0.009
	(0.008)	(0.008)	(0.008)	(0.008)
Reason: Amount too small (=1)	−0.0003	−0.0002	−0.001	−0.001
	(0.006)	(0.006)	(0.006)	(0.006)
Reason: Family situation (=1)	0.002	0.002	0.002	0.003
	(0.007)	(0.007)	(0.007)	(0.007)
Study Field FE	No	Yes	No	Yes
University FE	No	Yes	No	Yes
Observations	6,225	6,225	6,225	6,225
R^2	0.068	0.079	0.061	0.073
F Statistic	7.568***	3.855***	6.814***	3.548***

Notes: Continued on next page.

Table C.9: Intervention Effect on Student Aid Take-Up (extended) (contd.)

	Take-Up of Student Aid (=1)			
	Imputed		Non-Imputed	
	(1)	(2)	(3)	(4)
Reason: Privacy issues (=1)	−0.004	−0.004	−0.006	−0.005
	(0.007)	(0.007)	(0.006)	(0.006)
Reason: Enough support parents (=1)	−0.010	−0.009	-0.012^{**}	-0.011^{*}
	(0.006)	(0.006)	(0.005)	(0.006)
Reason: No money from state (=1)	−0.009	−0.010	−0.009	−0.011
	(0.007)	(0.007)	(0.007)	(0.007)
Reason: Wealth (=1)	−0.001	−0.001	−0.002	−0.002
	(0.004)	(0.004)	(0.004)	(0.004)
Reason: Other (=1)	0.010	0.010	0.008	0.008
	(0.010)	(0.010)	(0.010)	(0.010)
Patience	−0.0005	−0.001	−0.001	−0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Impulsiveness	0.001	0.001	0.0001	0.0002
	(0.001)	(0.001)	(0.001)	(0.001)
Debt Aversion	−0.001	−0.001	−0.0003	−0.0003
	(0.001)	(0.001)	(0.001)	(0.001)
Constant	0.196^{***}	0.181^{***}	0.162^{***}	0.148^{**}
	(0.058)	(0.061)	(0.054)	(0.058)
Study Field FE	No	Yes	No	Yes
University FE	No	Yes	No	Yes
Observations	6,225	6,225	6,225	6,225
R^2	0.068	0.079	0.061	0.073
F Statistic	7.568^{***}	3.855^{***}	6.814^{***}	3.548^{***}

Notes: The table shows the intervention effect on take-up rates. Every student who indicated to receive student aid in wave 2 or 3 or with a successful application is considered for take-up. For columns 1 and 2, also students are considered for take-up who are classified as eligible based on their sociodemographic and economic situation without considering their own income and who indicated to have applied for student aid but did not have the final decision in wave 2 and did not participate in wave 3.
The table shows the regression coefficient of all misperception, sociodemographic, reasons for non-take-up, and preference variables not displayed in columns 2 and 3 of Table C.8. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table C.10: Intervention Effect on Take-Up of Student Aid - Eligible Students (without own income)

	Take-Up of Student Aid (=1)			
	Imputed		Non-Imputed	
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.023***	0.022***	0.017**	0.017**
	(0.007)	(0.007)	(0.007)	(0.007)
Constant	0.035***	0.355***	0.032***	0.322***
	(0.005)	(0.137)	(0.005)	(0.117)
Controls	No	Yes	No	Yes
Study Field FE	No	Yes	No	Yes
University FE	No	Yes	No	Yes
Observations	2,718	2,718	2,718	2,718
R^2	0.003	0.121	0.002	0.111
F Statistic	8.379***	2.774***	4.941**	2.519***

Notes: The table shows the intervention effect on take-up rates for students who are classified as eligible for student aid based on their sociodemographic and economic situation without considering their own income. Every student who indicated to receive student aid in wave 2 or 3 or with a successful application is considered for take-up. For columns 1 and 2, all students who were classified as eligible (excluding their income) and indicated to have applied for student aid but did not participate in the third wave were imputed to take up. I control for all sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table C.11: Intervention Effect on Take-Up of Student Aid - Eligible Students (with own income)

	Take-Up of Student Aid (=1)			
	Imputed		Non-Imputed	
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.027***	0.025***	0.018**	0.018**
	(0.009)	(0.009)	(0.008)	(0.009)
Constant	0.040***	0.412**	0.036***	0.405***
	(0.006)	(0.163)	(0.006)	(0.137)
Controls	No	Yes	No	Yes
Study Field FE	No	Yes	No	Yes
University FE	No	Yes	No	Yes
Observations	2,072	2,072	2,072	2,072
R^2	0.004	0.136	0.002	0.126
F Statistic	7.383***	2.494***	3.956**	2.274***

Notes: The table shows the intervention effect on take-up rates for students who are classified as eligible for student aid based on their sociodemographic and economic situation including income. Every student who indicated to receive student aid in wave 2 or 3 or with a successful application is considered for take-up. For columns 1 and 2, all students who were classified as eligible and indicated to have applied for student aid but did not participate in the third wave were imputed to take up.

I control for all sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects. Clustered standard errors are in parentheses.

*p<0.1; **p<0.05; ***p<0.01

Table C.12: Information and Awareness Intervention Effects on Student Aid Applications

	Take-Up of Student Aid (=1)			
	Imputed		Non-Imputed	
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.011***	0.010***	0.007**	0.007**
	(0.004)	(0.004)	(0.004)	(0.003)
Awareness-Intervention (=1)	0.028	0.013	0.012	−0.001
	(0.021)	(0.020)	(0.017)	(0.017)
Info X Awareness	−0.005	0.001	0.016	0.021
	(0.028)	(0.027)	(0.026)	(0.025)
Constant	0.023***	0.176***	0.022***	0.145**
	(0.003)	(0.060)	(0.003)	(0.058)
Controls	No	Yes	No	Yes
Study Field FE	No	Yes	No	Yes
University FE	No	Yes	No	Yes
Observations	6,225	6,225	6,225	6,225
R^2	0.002	0.080	0.001	0.074
F Statistic	4.006***	3.809***	2.668**	3.511***

Notes: The table shows the effect of both the information and the cross-randomized awareness intervention on student aid applications. The awareness intervention was distributed to 200 students from both the control and treatment groups of the information intervention. Students were informed in an email that they could receive a positive amount of student aid if they apply. For columns 1 and 2, all students who were classified as eligible (excluding their income) and indicated to have applied for student aid but did not participate in the third wave were imputed to take up.
I control for all sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table C.13: Intervention Effect on University Level Specifications

Specification	Number of Universities	Number of Students	Weighted ATE on Uni Level	p-value
University level	37	5779	0.0146	0.6235
Universities with N < 50	14	317	0.0502	0.4546
Universities with N >= 50	23	5462	0.0125	0.4884
>10% Students in City	16	2398	0.0116	0.8832
<=10% Students in City	21	3064	0.0133	0.3986
Enrolled > 10,000	25	4919	0.0117	0.7517
Enrolled <= 10,000	12	543	0.0199	0.1776
Citysize > 100,000	29	5115	0.0124	0.5572
Citysize <= 100,000	8	347	0.0143	0.5870

Notes: The table shows the intervention effect on university level. For each specification, the average treatment effect is calculated where each university is used as one observation with weights for the number of students per university. The p-values show if these ATEs are significantly different from the overall treatment effect of 1.1 pp in the increase of take-up through the intervention based on weighted two-sided t-tests. All t-tests are insignificant.

Table C.14: Reasons Why Receivers in the Intervention Group Reacted to the Intervention

Reasons that motivated me to apply (=1)	Take-Up (N=42)
I became more specifically aware of BAföG through the first survey	0.571
The information that I could possibly expect a positive BAföG	0.905
The information about the BAföG amount per month	0.548
The information about the amount of parental income	0.524
The information about the amount of my own income	0.286
The information about the amount of my own assets	0.476
The information about the repayment amount of BAföG	0.357
Other	0.190

Notes: The table shows the fraction of how many students indicated which reason why they applied due to the information intervention. Only students who previously stated that their participation in the first wave survey lead them to apply for student aid are included. The reasons are measured on a 5-point Likert scale. If a student indicated for a specific reason that it applies or rather applies to them, they are represented in the fraction of indicating the specific reason, respectively.

Table C.15: Heterogeneous Intervention Effects on Student Aid Take-Up (Non-Imputed)

	Take-Up of Student Aid (=1)			
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	-0.002	-0.001	0.002	-0.009^{**}
	(0.003)	(0.004)	(0.003)	(0.004)
SES-Index	-0.007^{***}			-0.002
	(0.002)			(0.003)
Intervention X	0.025^{***}			0.019^{**}
Low Quintiles SES (=1)	(0.008)			(0.009)
Monthly Income (in %)		-0.007^{*}		-0.009^{**}
		(0.004)		(0.004)
Intervention X		0.020^{***}		0.016^{**}
Low Quintiles Income (=1)		(0.007)		(0.007)
Reasons: Parents' Income (Index)			-0.011^{***}	-0.009^{***}
			(0.002)	(0.002)
Intervention X			0.013^{*}	0.005
Low Quintiles Reasons: P. Income (=1)			(0.007)	(0.008)
Calculated Entitlement (in 100€)	0.004^{***}	0.006^{***}	0.005^{***}	0.004^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
Mean Take-Up - High Quintiles Control	0.015	0.019	0.011	0.007
Mean Take-Up - Low Quintiles Control	0.034	0.028	0.041	0.052
Observations	6,225	6,225	6,225	6,225
R^2	0.033	0.028	0.036	0.040
F Statistic	2.557^{***}	2.170^{***}	2.782^{***}	3.003^{***}

Notes: The table shows results from OLS estimation of the heterogeneity driving variables and interaction terms on a dummy variable equal to 1 if the participant took up student aid since wave 1. Explanatory variables were selected through causal random forest estimation. The SES-Index gives the socioeconomic status of students constructed using PCA. The index for reasons for non-take-up is constructed using PCA, where the more the students indicated that their parents' income was why they did not apply, the higher the index. The individual income is divided by the average income of the whole sample to show effects in %. Low Quintiles dummies are equal to 1 if the participant ranked below the 40%-quantile on the SES-, the Reasons-Index, or income, respectively. I control for the calculated student aid entitlements. Study field and university fixed effects are included. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table C.16: Heterogeneous Intervention Effects on Student Aid Take-Up - Eligible Students (without own income)

	Take-Up of Student Aid (=1)			
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.007	0.008	0.006	−0.009
	(0.008)	(0.009)	(0.008)	(0.009)
SES-Index	−0.008**			−0.002
	(0.004)			(0.004)
Intervention X	0.034**			0.017
Low Quintiles SES (=1)	(0.017)			(0.018)
Monthly Income (in %)		−0.026***		−0.031***
		(0.009)		(0.009)
Intervention X		0.033**		0.026*
Low Quintiles Income (=1)		(0.015)		(0.015)
Reasons: Parents' Income (Index)			−0.013***	−0.011***
			(0.003)	(0.003)
Intervention X			0.035**	0.028
Low Quintiles Reasons: P. Income (=1)			(0.017)	(0.018)
Calculated Entitlement (in 100€)	0.006***	0.006***	0.006***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
Mean Take-Up - High Quintiles Control	0.023	0.029	0.023	0.013
Mean Take-Up - Low Quintiles Control	0.052	0.043	0.043	0.08
Observations	2,718	2,718	2,718	2,718
R^2	0.047	0.047	0.054	0.064
F Statistic	1.761***	1.780***	2.039***	2.297***

Notes: The table shows results from OLS estimation of the heterogeneity driving variables and interaction terms on a dummy variable equal to 1 if the participant took up student aid since wave 1. The sample is restricted to students who were classified as eligible for student aid based on their sociodemographic and economic situation without considering their own income. All students who were classified as eligible and indicated to have applied for student aid but did not participate in the third wave were imputed to take up. Explanatory variables were selected through causal random forest estimation. The SES-Index gives the socioeconomic status of students constructed using PCA. The index for reasons for non-take-up is constructed using PCA, where the more the students indicated that their parents' income was why they did not apply, the higher the index. The individual income is divided by the average income of the whole sample to show effects in %. Low Quintiles dummies are equal to 1 if the participant ranked below the 40%-quantile on the SES-, the Reasons-Index, or income, respectively. I control for the calculated student aid entitlements. Study field and university fixed effects are included. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table C.17: Heterogeneous Intervention Effects on Student Aid Take-Up (Non-imputed) - Eligible Students (without own income)

	Take-Up of Student Aid (=1)			
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.007	0.001	0.004	−0.010
	(0.008)	(0.008)	(0.007)	(0.009)
SES-Index	-0.007^{**}			−0.003
	(0.004)			(0.004)
Intervention X	0.021			0.007
Low Quintiles SES (=1)	(0.015)			(0.017)
Monthly Income (in %)		-0.021^{**}		-0.025^{***}
		(0.008)		(0.009)
Intervention X		0.038^{***}		0.032^{**}
Low Quintiles Income (=1)		(0.013)		(0.013)
Reasons: Parents' Income (Index)			-0.012^{***}	-0.011^{***}
			(0.003)	(0.003)
Intervention X			0.025^{*}	0.022
Low Quintiles Reasons: P. Income (=1)			(0.014)	(0.016)
Calculated Entitlement (in 100€)	0.005^{***}	0.005^{***}	0.005^{***}	0.003^{**}
	(0.001)	(0.001)	(0.001)	(0.001)
Mean Take-Up - High Quintiles Control	0.023	0.027	0.019	0.013
Mean Take-Up - Low Quintiles Control	0.045	0.04	0.053	0.08
Observations	2,718	2,718	2,718	2,718
R^2	0.040	0.043	0.047	0.056
F Statistic	1.497^{***}	1.618^{***}	1.762^{***}	2.018^{***}

Notes: The table shows results from OLS estimation of the heterogeneity driving variables and interaction terms on a dummy variable equal to 1 if the participant took up student aid since wave 1. The sample is restricted to students who were classified as eligible for student aid based on their sociodemographic and economic situation without considering their own income. Explanatory variables were selected through causal random forest estimation. The SES-Index gives the socioeconomic status of students constructed using PCA. The index for reasons for non-take-up is constructed using PCA, where the more the students indicated that their parents' income was why they did not apply, the higher the index. The individual income is divided by the average income of the whole sample to show effects in %. Low Quintiles dummies are equal to 1 if the participant ranked below the 40%-quantile on the SES-, the Reasons-Index, or income, respectively. I control for the calculated student aid entitlements. Study field and university fixed effects are included. Clustered standard errors are in parentheses.

*p<0.1; **p<0.05; ***p<0.01

Table C.18: Heterogeneous Intervention Effects on Student Aid Take-Up - Eligible Students (with own income)

	Take-Up of Student Aid (=1)			
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.010	0.008	0.009	−0.007
	(0.009)	(0.011)	(0.010)	(0.010)
SES-Index	−0.009**			−0.002
	(0.005)			(0.005)
Intervention X	0.035			0.020
Low Quintiles SES (=1)	(0.022)			(0.024)
Monthly Income (in %)		−0.041***		−0.040***
		(0.011)		(0.011)
Intervention X		0.040**		0.033*
Low Quintiles Income (=1)		(0.018)		(0.018)
Reasons: Parents' Income (Index)			−0.017***	−0.014***
			(0.004)	(0.004)
Intervention X			0.031	0.021
Low Quintiles Reasons: P. Income (=1)			(0.021)	(0.023)
Calculated Entitlement (in 100€)	0.005***	0.007***	0.005***	0.003**
	(0.002)	(0.002)	(0.002)	(0.002)
Mean Take-Up - High Quintiles Control	0.026	0.033	0.024	0.013
Mean Take-Up - Low Quintiles Control	0.06	0.049	0.065	0.088
Observations	2,072	2,072	2,072	2,072
R^2	0.048	0.054	0.056	0.069
F Statistic	1.500***	1.674***	1.761***	2.066***

Notes: The table shows results from OLS estimation of the heterogeneity driving variables and interaction terms on a dummy variable equal to 1 if the participant took up student aid since wave 1. The sample is restricted to students who were classified as eligible for student aid based on their sociodemographic and economic situation, including their income. All students who were classified as eligible and indicated to have applied for student aid but did not participate in the third wave were imputed to take up. Explanatory variables were selected through causal random forest estimation. The SES-Index gives the socioeconomic status of students constructed using PCA. The index for reasons for non-take-up is constructed using PCA, where the more the students indicated that their parents' income was why they did not apply, the higher the index. The individual income is divided by the average income of the whole sample to show effects in %. Low Quintiles dummies are equal to 1 if the participant ranked below the 40%-quantile on the SES-, the Reasons-Index, or income, respectively. I control for the calculated student aid entitlements. Study field and university fixed effects are included. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table C.19: Heterogeneous Intervention Effects on Student Aid Take-Up (Non-imputed) - Eligible Students (with own income)

	Take-Up of Student Aid (=1)			
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.007	−0.004	0.007	−0.012
	(0.009)	(0.010)	(0.009)	(0.009)
SES-Index	−0.009*			−0.002
	(0.004)			(0.005)
Intervention X	0.022			0.010
Low Quintiles SES (=1)	(0.020)			(0.022)
Monthly Income (in %)		−0.031***		−0.030***
		(0.010)		(0.010)
Intervention X		0.051***		0.046***
Low Quintiles Income (=1)		(0.016)		(0.016)
Reasons: Parents' Income (Index)			−0.016***	−0.013***
			(0.004)	(0.004)
Intervention X			0.019	0.012
Low Quintiles Reasons: P. Income (=1)			(0.017)	(0.019)
Calculated Entitlement (in 100€)	0.004**	0.005***	0.004**	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Mean Take-Up - High Quintiles Control	0.026	0.03	0.019	0.013
Mean Take-Up - Low Quintiles Control	0.051	0.045	0.063	0.088
Observations	2,072	2,072	2,072	2,072
R^2	0.041	0.050	0.049	0.063
F Statistic	1.267*	1.565***	1.507***	1.851***

Notes: The table shows results from OLS estimation of the heterogeneity driving variables and interaction terms on a dummy variable equal to 1 if the participant took up student aid since wave 1. The sample is restricted to students who were classified as eligible for student aid based on their sociodemographic and economic situation, including their income. Explanatory variables were selected through causal random forest estimation. The SES-Index gives the socioeconomic status of students constructed using PCA. The index for reasons for non-take-up is constructed using PCA, where the more the students indicated that their parents' income was why they did not apply, the higher the index. The individual income is divided by the average income of the whole sample to show effects in %. Low Quintiles dummies are equal to 1 if the participant ranked below the 40%-quantile on the SES-, the Reasons-Index, or income, respectively. I control for the calculated student aid entitlements. Study field and university fixed effects are included. Clustered standard errors are in parentheses.

*p<0.1; **p<0.05; ***p<0.01

Table C.20: Relative Changes in Income of Eligible Students over Time

	Relative Income (in %)					
	Total		from Work		from Parents	
	(1)	(2)	(3)	(4)	(5)	(6)
Take-Up (=1)	-0.162^{***}	-0.151^{***}	-0.239^{**}	-0.201^{*}	-0.148^{**}	-0.177^{***}
	(0.050)	(0.050)	(0.110)	(0.110)	(0.061)	(0.061)
Wave 2 (=1)	0.043^{***}	0.061^{***}	0.148^{***}	0.269^{***}	-0.039^{***}	-0.049^{***}
	(0.008)	(0.008)	(0.020)	(0.020)	(0.011)	(0.011)
Wave 3 (=1)	0.127^{***}	0.161^{***}	0.350^{***}	0.551^{***}	-0.022	-0.033^{**}
	(0.011)	(0.011)	(0.027)	(0.027)	(0.014)	(0.014)
Take-Up (=1) X Wave 2 (=1)	0.080^{*}	0.101^{**}	-0.139^{*}	-0.206^{**}	-0.081^{*}	-0.093^{*}
	(0.046)	(0.046)	(0.083)	(0.083)	(0.049)	(0.049)
Take-Up (=1) X Wave 3 (=1)	0.121^{**}	0.195^{***}	-0.446^{***}	-0.449^{***}	-0.158^{***}	-0.171^{***}
	(0.053)	(0.053)	(0.124)	(0.124)	(0.052)	(0.052)
Mean Non-Take-Up W1	1024.79	936.28	379.87	280.43	497.67	524.58
Eligible Students	w/o inc	with inc	w/o inc	with inc	w/o inc	with inc
Observations	4,665	3,639	4,755	3,708	4,755	3,708
R^2	0.111	0.140	0.113	0.140	0.164	0.194
F Statistic	578.324^{***}	588.960^{***}	600.770^{***}	597.847^{***}	927.483^{***}	886.349^{***}

Notes: The table shows results from an OLS panel regression with individual-level random effects of relative income over time for students who are classified as eligible for student aid in wave 1. For each regression, I determine eligibility excluding students' income in the first column and including it in the second column, respectively. Income is measured as the absolute income that participants report in each wave divided by the average income in wave 1 of participants who do not take up student aid over time to measure the relative change compared to this reference group. *Wave 2* and *Wave 3* are equal to one for the respective period, and *Take-Up* is one for all participants who take up student aid in wave 2 or wave 3. I control for sociodemographic characteristics. Robust standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Appendix D: Probit Regressions on Take-Up

Table D.1: Intervention Effect on Student Aid Take-Up

	Take-Up of Student Aid (=1)					
	Imputed			Non-Imputed		
	(1)	(2)	(3)	(4)	(5)	(6)
Info-Intervention (=1)	0.166***	0.166**	0.198***	0.132**	0.125*	0.149**
	(0.056)	(0.065)	(0.072)	(0.058)	(0.067)	(0.075)
Constant	−1.985***	0.113	−7.587***	−2.007***	−0.111	−7.743***
	(0.044)	(0.873)	(0.989)	(0.046)	(0.913)	(1.021)
Controls	No	Yes	Yes	No	Yes	Yes
Study Field FE	No	No	Yes	No	No	Yes
University FE	No	No	Yes	No	No	Yes
Observations	6,225	6,225	6,225	6,225	6,225	6,225

Notes: The table shows the results of Table C.8 using Probit estimation instead of OLS.
I control for misperceptions per area in the first wave, all sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table D.2: Intervention Effect on Take-Up of Student Aid - Eligible Students (without own income)

	Take-Up of Student Aid (=1)			
	Imputed		Non-Imputed	
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.243***	0.311***	0.194**	0.241**
	(0.076)	(0.110)	(0.082)	(0.117)
Constant	−1.812***	−7.527***	−1.850***	−7.429***
	(0.062)	(1.567)	(0.065)	(1.548)
Controls	No	Yes	No	Yes
Study Field FE	No	Yes	No	Yes
University FE	No	Yes	No	Yes
Observations	2,718	2,718	2,718	2,718

Notes: The table shows the results of Table C.10 using Probit estimation instead of OLS.
I control for all sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table D.3: Intervention Effect on Take-Up of Student Aid - Eligible Students (with own income)

	Take-Up of Student Aid (=1)			
	Imputed		Non-Imputed	
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.250***	0.301**	0.193**	0.219
	(0.084)	(0.124)	(0.088)	(0.134)
Constant	−1.756***	−3.893**	−1.801***	−3.313*
	(0.068)	(1.793)	(0.071)	(1.778)
Controls	No	Yes	No	Yes
Study Field FE	No	Yes	No	Yes
University FE	No	Yes	No	Yes
Observations	2,072	2,072	2,072	2,072

Notes: The table shows the results of Table C.11 using Probit estimation instead of OLS.
I control for all sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table D.4: Causal Effect of Correcting Misperceptions on Student Aid Take-Up (LATE)

	Take-Up of Student Aid (=1)					
	Eligible Students: without own income		Eligible Students: with own income		Scenarios	
	Binary	*Likert*	*Binary*	*Likert*	*Pooled*	*Total*
	(1)	(2)	(3)	(4)	(5)	(6)
Correction of Misperceptions (in %)	12.903**	12.511**	8.559***	9.554***	7.769***	8.577***
	(5.039)	(4.886)	(3.275)	(3.655)	(2.772)	(3.060)
Study Field FE	Yes	Yes	Yes	Yes	Yes	Yes
University FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,361	2,361	1,786	1,786	6,225	6,225
1st stage F Statistic	4.330	6.487	9.642	11.597	14.475	24.503

Notes: The table shows the results of Table 4 using Probit estimation for the second stage instead of OLS. I control for all misperceptions from the scenarios, confidence in these answers, sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects in both stages. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table D.5: Information and Awareness Intervention Effects on Student Aid Take-Up

	Take-Up of Student Aid (=1)			
	Imputed		Non-Imputed	
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.172***	0.205***	0.124**	0.136*
	(0.058)	(0.075)	(0.059)	(0.076)
Awareness-Intervention (=1)	0.363*	0.211	0.185	0.021
	(0.205)	(0.254)	(0.232)	(0.300)
Info X Awareness	−0.116	−0.106	0.121	0.222
	(0.265)	(0.322)	(0.285)	(0.354)
Constant	−2.003***	−7.658***	−2.015***	−7.803***
	(0.048)	(0.997)	(0.048)	(1.029)
Controls	No	Yes	No	Yes
Study Field FE	No	Yes	No	Yes
University FE	No	Yes	No	Yes
Observations	6,225	6,225	6,225	6,225

Notes: The table shows the results of Table C.12 using Probit estimation for the second stage instead of OLS.
I control for all sociodemographic and other control variables mentioned in Section 3, as well as reasons for non-take-up and study field and university fixed effects. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table D.6: Heterogeneous Intervention Effects on Student Aid Take-Up

	Take-Up of Student Aid (=1)			
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	-0.118	0.049	0.042	-0.198
	(0.095)	(0.082)	(0.101)	(0.128)
SES-Index	-0.096^{***}			-0.022
	(0.024)			(0.030)
Intervention X	0.463^{***}			0.388^{***}
Low Quintiles SES (=1)	(0.116)			(0.140)
Monthly Income (in %)		-0.197^{*}		-0.227^{**}
		(0.109)		(0.099)
Intervention X		0.230^{**}		0.171
Low Quintiles Income (=1)		(0.101)		(0.113)
Reasons: Parents' Income (Index)			-0.191^{***}	-0.178^{***}
			(0.030)	(0.035)
Intervention X			0.183	0.035
Low Quintiles Reasons: P. Income (=1)			(0.122)	(0.142)
Calculated Entitlement (in 100€)	0.064^{***}	0.086^{***}	0.067^{***}	0.050^{***}
	(0.011)	(0.010)	(0.010)	(0.011)
Observations	6,225	6,225	6,225	6,225

Notes: The table shows the results of Table 5 using Probit estimation for the second stage instead of OLS. Study field and university fixed effects are included. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table D.7: Heterogeneous Intervention Effects on Student Aid Take-Up (Non-Imputed)

	Take-Up of Student Aid (=1)			
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	-0.127	-0.018	0.015	-0.243^{*}
	(0.098)	(0.088)	(0.103)	(0.132)
SES-Index	-0.097^{***}			-0.023
	(0.025)			(0.032)
Intervention X	0.426^{***}			0.347^{**}
Low Quintiles SES (=1)	(0.122)			(0.145)
Monthly Income (in %)		-0.157		-0.188^{*}
		(0.107)		(0.097)
Intervention X		0.291^{***}		0.243^{**}
Low Quintiles Income (=1)		(0.105)		(0.115)
Reasons: Parents' Income (Index)			-0.190^{***}	-0.178^{***}
			(0.031)	(0.036)
Intervention X			0.172	0.037
Low Quintiles Reasons: P. Income (=1)			(0.125)	(0.143)
Calculated Entitlement (in 100€)	0.054^{***}	0.074^{***}	0.056^{***}	0.039^{***}
	(0.011)	(0.011)	(0.011)	(0.011)
Observations	6,225	6,225	6,225	6,225

Notes: The table shows the results of Table C.15 using Probit estimation for the second stage instead of OLS. Study field and university fixed effects are included. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table D.8: Heterogeneous Intervention Effects on Student Aid Take-Up - Eligible Students (without own income)

	Take-Up of Student Aid (=1)			
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.074	0.149	0.083	−0.046
	(0.116)	(0.130)	(0.132)	(0.168)
SES-Index	−0.075**			−0.022
	(0.033)			(0.038)
Intervention X	0.296*			0.142
Low Quintiles SES (=1)	(0.158)			(0.178)
Monthly Income (in %)		−0.436**		−0.459***
		(0.188)		(0.171)
Intervention X		0.176		0.140
Low Quintiles Income (=1)		(0.153)		(0.163)
Reasons: Parents' Income (Index)			−0.169***	−0.163***
			(0.042)	(0.046)
Intervention X			0.232	0.182
Low Quintiles Reasons: P. Income (=1)			(0.167)	(0.187)
Calculated Entitlement (in 100€)	0.059***	0.060***	0.062***	0.039***
	(0.013)	(0.013)	(0.012)	(0.014)
Observations	2,718	2,718	2,718	2,718

Notes: The table shows the results of Table C.16 using Probit estimation for the second stage instead of OLS. Study field and university fixed effects are included. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table D.9: Heterogeneous Intervention Effects on Student Aid Take-Up (Non-imputed) - Eligible Students (without own income)

	Take-Up of Student Aid (=1)			
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.066	0.033	0.058	-0.127
	(0.123)	(0.142)	(0.141)	(0.183)
SES-Index	-0.079^{**}			-0.027
	(0.036)			(0.040)
Intervention X	0.230			0.079
Low Quintiles SES (=1)	(0.170)			(0.193)
Monthly Income (in %)		-0.355^{*}		-0.386^{**}
		(0.185)		(0.169)
Intervention X		0.309^{*}		0.287^{*}
Low Quintiles Income (=1)		(0.159)		(0.170)
Reasons: Parents' Income (Index)			-0.172^{***}	-0.166^{***}
			(0.046)	(0.050)
Intervention X			0.196	0.169
Low Quintiles Reasons: P. Income (=1)			(0.169)	(0.194)
Calculated Entitlement (in 100€)	0.052^{***}	0.051^{***}	0.054^{***}	0.030^{**}
	(0.014)	(0.014)	(0.013)	(0.015)
Observations	2,718	2,718	2,718	2,718

Notes: The table shows the results of Table C.17 using Probit estimation for the second stage instead of OLS. Study field and university fixed effects are included. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table D.10: Heterogeneous Intervention Effects on Student Aid Take-Up - Eligible Students (with own income)

	Take-Up of Student Aid (=1)			
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.111	0.123	0.149	-0.009
	(0.116)	(0.143)	(0.143)	(0.164)
SES-Index	-0.087^{**}			-0.021
	(0.038)			(0.044)
Intervention X	0.236			0.151
Low Quintiles SES (=1)	(0.180)			(0.203)
Monthly Income (in %)		-0.572^{***}		-0.519^{***}
		(0.169)		(0.155)
Intervention X		0.219		0.211
Low Quintiles Income (=1)		(0.164)		(0.171)
Reasons: Parents' Income (Index)			-0.207^{***}	-0.189^{***}
			(0.047)	(0.051)
Intervention X			0.121	0.058
Low Quintiles Reasons: P. Income (=1)			(0.179)	(0.200)
Calculated Entitlement (in 100€)	0.046^{***}	0.067^{***}	0.045^{***}	0.036^{**}
	(0.015)	(0.015)	(0.015)	(0.017)
Observations	2,072	2,072	2,072	2,072

Notes: The table shows the results of Table C.18 using Probit estimation for the second stage instead of OLS. Study field and university fixed effects are included. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table D.11: Heterogeneous Intervention Effects on Student Aid Take-Up (Non-imputed) - Eligible Students (with own income)

	Take-Up of Student Aid (=1)			
	(1)	(2)	(3)	(4)
Info-Intervention (=1)	0.091	−0.051	0.124	−0.147
	(0.131)	(0.161)	(0.153)	(0.191)
SES-Index	-0.090^{**}			−0.024
	(0.042)			(0.048)
Intervention X	0.174			0.116
Low Quintiles SES (=1)	(0.189)			(0.209)
Monthly Income (in %)		-0.464^{***}		-0.414^{***}
		(0.173)		(0.158)
Intervention X		0.434^{**}		0.442^{**}
Low Quintiles Income (=1)		(0.175)		(0.181)
Reasons: Parents' Income (Index)			-0.210^{***}	-0.195^{***}
			(0.053)	(0.057)
Intervention X			0.073	0.013
Low Quintiles Reasons: P. Income (=1)			(0.179)	(0.196)
Calculated Entitlement (in 100€)	0.042^{**}	0.060^{***}	0.038^{**}	0.029
	(0.017)	(0.016)	(0.016)	(0.019)
Observations	2,072	2,072	2,072	2,072

Notes: The table shows the results of Table C.19 using Probit estimation for the second stage instead of OLS. Study field and university fixed effects are included. Clustered standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01