Survey-Guided Policy Design: An Economic Approach to Student Aid and Beyond

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Chapter 1

Introduction

In this dissertation, I use research-guided surveys to systematically analyze the problems or barriers that prevent individuals from acting in line with a policy goal. Based on this evidence, I design policy interventions that address these problems to achieve behavioral change. These interventions are tested in randomized controlled trials to evaluate their effectiveness. By using survey methods to identify problems and design interventions before evaluating their effectiveness, I show how survey evidence can help to inform policy ex-ante.

In Chapter 2, together with Matthias Sutter and Sebastian Tonke, we look at the task of designing effective interventions from a general point of view. To facilitate ex-ante predictions about which policy intervention potentially works best, pre-intervention diagnostics are necessary to identify the underlying problems causing individuals not to act in line with a policy goal. We propose a systematic, parsimonious, and generalizable survey tool – anamnesis – to identify these underlying problems. Guided by economic and psychologic theory, our anamnesis classifies the underlying type of problem along three fundamental diagnoses: awareness, intention, and implementation problems. We test whether we can use our anamnesis results to predict the effectiveness of three typical interventions (reminders, incentives, simplifications) across three different settings in an online experiment with 7,500 subjects. Participants worked on a real-effort task where we manipulated the settings to induce one of the three types of problems. The interventions aimed at reducing the number of mistakes within the task. We find high context-specificity of the interventions across the three different settings, which can be predicted by our anamnesis results. When we predict ex-ante that an intervention will solve 100% of mistakes made in a given setting, the intervention actually reduced the mistakes by 89.2%, on average. Choosing an intervention based on our diagnosis increases the effect size by around 58% compared to randomly choosing one of the tested interventions. Our results show that a systematic survey tool can facilitate designing effective interventions ex-ante without the need to test different interventions first.

In Chapter 3, together with Sascha Strobl, we turn towards the applied case of student aid take-up. Across Western countries, many students do not take up student aid despite being eligible (Bettinger et al., 2012; Booij, Leuven & Oosterbeek, 2012; Callender & Wilkinson, 2013; Dynarski et al., 2021; Herber & Kalinowski, 2019). Yet, it is unclear how many students do not take up aid because they misperceive their eligibility (eligibility barrier), and how many do not apply for student aid despite knowing they are eligible (application barrier). These two groups, however, likely have different reasons for non-take-up. To design an effective intervention that aims to increase take-up, it is necessary to identify these groups and analyze their non-take-up reasons separately. In the context of student aid in Germany, we conducted a structured survey among 22,222 enrolled students to elicit perceived eligibility for student aid, perceptions about the terms of student aid, and reasons for non-take-up. Using the students' sociodemographic and economic background, we calculate their entitlement to determine actual eligibility. We find an eligibility barrier of 82.2% of students who do not believe they are eligible, and an application barrier of 13.2% of students who know about their eligibility but still do not take up aid. Both barriers are largely driven by misperceptions. Among the students at the eligibility barrier, 59% think they will not pass the means-test, but 62% of these underestimate the eligibility conditions. Among the students at the application barrier, 62% do not take up aid due to debt aversion, but 76% of these overestimate the repayment. The results show that misperceptions are a main inhibitor of student aid take-up at both barriers.

In Chapter 4, I address these misperceptions with an information intervention. At the end of the survey (that was the basis for Chapter 3), a randomized subsample of students received additional information about the terms of student aid and, if possible based on the eligibility calculation, about their own entitlement. This is the treatment group. The control group did not receive additional information. Both groups are contacted again 6 months and 1 year after the initial survey to elicit perceptions about the terms of student aid again, and to elicit take-up of student aid. In the sample of 6,225 students who were part of this experiment and did not receive student aid at the initial survey, I find that 63% of the students systematically underestimate the financial value of student aid, and 86% misperceive their eligibility. The information intervention significantly corrects the misperceptions about student aid by 32% and about one's eligibility by 57%. Additionally, the intervention increases take-up of student aid by 1.1 pp (43%). Combining the two effects, I find that 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 effectively change take-up and thereby reduce social inequality by alleviating financial concerns among disadvantaged students and their parents.

Chapter 2

Designing Effective Interventions

joint with Matthias Sutter & Sebastian Tonke[*]

2.1 Introduction

A core objective of economics is to design effective interventions. Unfortunately, many interventions fail to meet their target or show high context-specificity. White (2019) summarizes that around 80% of interventions across several domains show weak or no positive effects. Other studies show that the effectiveness of interventions drops dramatically when scaled up or implemented elsewhere (Allcott, 2015; DellaVigna & Linos, 2022; List, 2022; Vivalt, 2020).[1] The conditions under which interventions work are barely identified (Bryan, Tipton & Yeager, 2021; Szaszi et al., 2022).

In this paper, we argue that the failure to design effective interventions is rooted in a lack of understanding of the underlying problem. Although effective interventions hinge on understanding the underlying problem (Datta & Mullainathan, 2014; Rockenbach, Tonke & Weiss, 2023; Rodrik, 2010; Tonke, 2024), a systematic, generalizable framework to elicit and diagnose problems is missing. To fill this gap, we propose a framework to diagnose the type and to quantify the extent of the underlying problem, which in turn allows us to predict the effectiveness of interventions. Our approach is analogous to a medical consultation. Before an intervention is implemented, the target subjects provide self-reported answers to a simple questionnaire (anamnesis). The anamnesis helps to identify the underlying problem (diagnosis). The diagnosis is used to predict the effectiveness of different intervention types and to recommend the most effective one (prescription).

Policymakers typically design and implement interventions when an individual's performance on some generic task does not match a stated policy goal. This can occur with respect to labor productivity, healthy nutrition, pro-environmental consumption, driving within the speed limit, saving for retirement, or paying taxes. An intervention then typically aims to narrow the gap between a policy goal and the performance of individuals on that task. We argue that in order to design effective interventions, it is crucial

[*]The idea and the experimental design for this study emerged from joint discussions between Sebastian Tonke, Matthias Sutter, and myself. I implemented and conducted the experiment via Prolific. I analyzed the data with input from Sebastian Tonke. I wrote the first manuscript, which we jointly revised multiple times.

[1]Maier et al. (2022) and Mertens et al. (2022) even argue that there is little evidence for the effectiveness of nudging after adjusting for publication bias.

to understand why people fail to act in line with the policy goal.[2] We propose three fundamental problems. The first problem is an *awareness problem*: Subjects are unaware that their actual performance deviates from their believed performance, i.e., their performance is worse than they thought. The second problem is an *intention problem*: Individuals might not intend to meet the policy goal. The third problem is an *implementation problem*: Individuals fail to implement their intentions. We show that the prevalence of these three problems can be measured through a parsimonious and generalizable set of anamnesis questions. The answers from the anamnesis then result in diagnoses, which we use to predict the effectiveness of different intervention types and make prescriptions for the most effective intervention.

To empirically validate the value of our framework, we have to show that the same intervention type can succeed or fail to change behavior, depending on the underlying problem. To do so, we designed three experimental settings that exogenously induce (i) awareness, (ii) intention, or (iii) implementation problems while holding important other parameters constant. Within each of these three settings, we implement three intervention types in addition to a baseline condition, resulting in a total of 12 experimental conditions. To resolve the attention problem, we use reminders; to resolve the intention problem, we increase the monetary incentives; and to resolve implementation problems, we use simplifications. We measure task performance through a modified version of the real-effort task by Toussaert (2018) in an online experiment with over 7,500 subjects. Participants have to remember 3-digit numbers appearing on their screen in short intervals. Upon request, they have to enter the last displayed number into an input field. Each participant receives 50 such queries. The stated policy goal is to answer all 50 queries correctly. This set-up provides us with the necessary experimental control to induce an awareness, intention, and implementation problem separately, as well as high statistical power given our 12 experimental conditions.

We conduct our anamnesis among participants in each baseline condition of the three settings after participants have worked on the task. The anamnesis consists of only two questions, measuring intentions to meet the policy goal and beliefs about their performance. We elicit intentions by asking how many of the 50 queries the individual initially planned to answer correctly (0-50). We measure beliefs by asking how many of the 50 queries they think they answered correctly (0-50). The extent of an awareness problem is then measured by the difference between the beliefs about one's performance and actual performance. Intention problems are measured by the difference between one's stated intention to answer a certain number of queries correctly and the policy goal. Implementation problems are measured by the difference between one's stated intention and one's believed actual performance.

As pre-registered and intended, we find that the effectiveness of the three interventions strongly varies across settings. Reminders reduce incorrectly answered queries between 24% to 54% compared to the baseline. The effect size of incentives ranges from 5% to 29%, and the effect size of simplifications ranges from 25% to 66%. Accordingly, we diagnose a high level of

[2] The policy goal can be chosen by oneself, a policymaker, an employer, or some other entity that aims to change behavior.

awareness, intention, or implementation problems in the setting where we induced the respective problem. We use the diagnoses to make point predictions regarding the effectiveness of the intervention types. As hypothesized, we find that treatment effects are largest in the setting where our framework predicts the highest effect. That is, reminders are most effective where we induce an awareness problem, incentives are most effective where we induce an intention problem, and simplifications are most effective where we induce an implementation problem. Our point predictions match actual intervention effects with high precision. Aggregating across settings and intervention types, we find that a predicted effectiveness of 100% ex-ante translates into an actual effectiveness of an intervention of 89.2%. Choosing an intervention based on our framework increases the treatment effect size by around 58% compared to randomly choosing one of the three interventions.

Our study makes two main contributions. First, rigorous impact evaluations with the goal to find out "what works" have been conducted across a broad range of economic fields.[3] These rigorous impact evaluations have uncovered that the majority of interventions seem to fail when scaled up or implemented elsewhere (Allcott, 2015; DellaVigna & Linos, 2022; List, 2022; Vivalt, 2020; White, 2019). This finding calls for novel approaches that can find out "what works when". To fill this gap, we develop and test a simple questionnaire that can predict the effectiveness of interventions across settings. Our anamnesis is easy to implement, generalizable, and succeeds in predicting which intervention types will be most effective.

We are aware of two alternative approaches to dealing with and predicting context dependency. The first approach is expert predictions. The literature, however, suggests that it is difficult to predict ex-ante which intervention type will be most effective in a given setting. For example, individual-level predictions of layman, professors, and practitioners poorly predict intervention effects in a given setting (DellaVigna & Linos, 2022; DellaVigna & Pope, 2018a; Milkman et al., 2021b). The second approach is to hand-pick a set of interventions and to compare them empirically. This can be done simultaneously in so-called mega-studies (Milkman et al., 2021a,b), by testing several different bundles of interventions (Banerjee et al., 2025), by targeting interventions for a specific group through machine learning (Opitz et al., 2024) or in sequentially conducted adaptive experiments (Kasy & Sautmann, 2021). Testing many interventions is, however, often not possible because of time, financial, and ethical constraints. Further, these approaches still require hand-picking a set of interventions ex-ante, potentially without systematic knowledge of the underlying fundamental problem.

Second, our paper adds and builds on reviews of intervention types and taxonomies that link intervention types to underlying problems or models of behavior (Benartzi et al., 2017; Datta & Mullainathan, 2014; Engl & Sgaier, 2020; Gneezy, Meier & Rey-Biel, 2011; Löfgren & Nordblom, 2020; Michie, Stralen & West, 2011; Münscher, Vetter & Scheuerle, 2015; Szaszi et al., 2017).[4] In contrast to this literature, our goal is not to classify interventions, catalog

[3] E.g. development economics (Demeritt & Hoff, 2018), education economics (e.g. Hoxby & Turner, 2015; Jensen, 2010), behavioral finance (e.g. Bhargava & Manoli, 2015; Duflo & Saez, 2003), and environmental economics (e.g. Bruelisauer et al., 2020; Newell & Siikamäki, 2014).

[4] For a review on defaults, see Jachimowicz et al. (2019). For a review on commitment devices, see Bryan, Karlan & Nelson (2010).

them, understand why they work, or provide overviews. Our contribution is to provide a practicable approach to diagnose the underlying fundamental problem along a systematic framework and to empirically show this approach's value in choosing the right intervention type.

This chapter is structured as follows: we first explain our framework and the prediction measures in more detail in Section 2.2. Then, we outline our experimental design and data used to validate our framework in Section 2.3. We present our results in Section 2.4 and conclude in Section 2.5.

2.2 The Framework

2.2.1 Diagnosis - Awareness, Intention, and Implementation Problems

Our diagnosis relies on a conceptual framework that categorizes deviations from the policy goal into three types of fundamental problems. The first fundamental problem is the awareness problem, which we define as being unaware that actual performance deviates from believed performance. For example, individuals might be unaware that their current work performance is worse than they think, or they might be unaware that they are driving faster than they think. Such unawareness can stem from a lack of salience (Bordalo, Gennaioli & Shleifer, 2022), forgetfulness, or limited attention (Gabaix, 2019). If unawareness is the underlying fundamental problem, then the prescription would be an intervention that makes individuals aware of their deviation from their believed performance, for example, by giving them feedback about their own behavior or by sending reminders.

The second fundamental problem is the intention problem, which we define as the lack of intention to meet the policy goal after considering the available and perceived costs, benefits, and constraints. That is, whether and to which degree individuals intend to match a policy goal depends on the utility they derive from doing so. If matching the policy goal does not increase their utility, individuals do not form the intention to meet the policy goal. For example, individuals might not intend to match their employer's performance expectation, or might not want to drive within the speed limit. If individuals have an intention problem, the prescription is to change the perceived costs, benefits, or constraints. For example, a policy maker might consider increasing the monetary payoff or correcting a common misperception about the costs of acting in line with a policy goal.

The third fundamental problem is the implementation problem. Individuals might fail to implement their intentions due to a lack of self-control (Thaler & Shefrin, 1981), procrastination (Laibson, 1997), unexpected complexity of the task, or other psychological factors (Ajzen, 1985, 1991). For example, task complexity might inhibit workers from implementing their intended productivity, or drivers might lack self-control to resist the temptation to speed. If individuals have implementation problems, potential policy interventions could be reducing the implementation costs through simplification, the removal of temptations, or commitment devices.

2.2.2 Anamnesis - Identifying and Quantifying the Problem

Borrowing terminology from medical consultation, we call the process of coming to a diagnosis the anamnesis. Anamnesis describes the process of eliciting and analyzing individuals' self-reported behavior to diagnose problems. The approach is pragmatic. Self-reported answers can be imperfect or biased, yet we will show that our anamnesis questions are highly valuable nonetheless. The approach is not exclusive. Our initial anamnesis can be easily extended through deeper examinations into the reasons for having awareness, intention, or implementation problems.

Two anamnesis questions suffice to make a diagnosis when the policy maker observes individuals' past performance (α) and when the policy goal (γ) is known to the target population. The first question measures beliefs about one's performance (β) by asking, e.g., "How many tasks do you think you solved correctly?". The second question measures intention (i) by asking, e.g., "How many of the tasks did you initially intend to solve correctly?".

The awareness problem is measured as the difference between beliefs and actual performance. For example, one might believe to have solved 40 tasks correctly while only 20 were actually solved correctly. We diagnose an awareness problem of 20 tasks.

The intention problem is measured as the difference between the policy goal and intended performance. For example, one might have intended to solve 40 tasks correctly, while the policy goal was to solve 50 tasks correctly. Here, we diagnose an intention problem of 10 tasks.

The implementation problem is measured as the difference between one's intention and beliefs about one's performance. Here, the individuals know they failed to implement their original intention. For example, one might report having planned to solve 40 tasks but believes to have solved only 10 tasks. We diagnose an implementation problem of 30 tasks.[5]

It is, of course, possible for individuals to have multiple problems simultaneously. We discuss how multiple concurrent diagnoses affect the predictability of our framework in Section 2.2.3. An overview of the framework is displayed in Table 2.1.

2.2.3 From Diagnosis to Prescription

We now discuss how our diagnoses lead to predictions and prescriptions. First, we use our diagnoses to make predictions about the effectiveness of different interventions. Based on these predictions, we then recommend the intervention with the highest predicted effectiveness. This recommendation is called a prescription.

We need to consider the following things when making predictions. For now, and as pre-registered, we assume that our intervention only addresses one single problem. We use reminders for awareness problems, incentives

[5]One might wonder why the anamnesis relies on the difference between the believed performance and not on the actual performance. This is because the difference between intention and actual performance can consist of two components: awareness problems and implementation problems. Since we want to measure awareness and implementation problems separately, we rely on the difference between intention and beliefs to measure the extent of the implementation problem and the difference between the belief and actual behavior to measure the extent of an awareness problem. Without an awareness problem, believed and actual performance are equal and can be used interchangeably.

TABLE 2.1: The Framework: Definition and Anamnesis of Fundamental Problems

Diagnosis	Anamnesis	Prescription (Intervention)
Awareness problem Actual performance differs from believed performance.	**Belief (β) - action (α)**	• Reminders • Feedback about behavior • ...
Intention problem No intention to match policy goal after considering perceived costs, benefits, and constraints.	**Policy goal (γ) - intention (i)**	• Change incentives or costs • Correct misperceptions of costs and benefits (e.g. information) • ...
Implementation problem Failing to implement the intention.	**Intention (i) - belief (β)**	• Commitment devices • Reduction of implementation cost (simplification, planning prompts) • ...

Notes: The table shows the three fundamental problems of our framework. We use two anamnesis questions (belief and action) and the policy goal as shown in column 2 for the diagnosis of a fundamental problem, explained in column 1. Typical interventions that are prescribed to solve the problems are provided in the last column.

for intention problems, and simplifications for implementation problems. Further, we assume that the intervention will resolve 100% of that specific problem. For example, after a reminder is implemented, we assume that there are no awareness problems anymore. Later, we will show that this is an overly optimistic assumption but that our predictions are already quite precise nevertheless. They become even better once we empirically adjust these assumptions.

In addition, we assume that an intervention is ineffective for individuals who have "negative diagnosis values". For example, an individual who believes to perform even better than originally intended would have a negative score for the implementation problem. In such cases, there is no implementation problem, and hence, the respective prediction is set to zero.

The next important consideration is that individuals can have multiple problems simultaneously. For instance, someone might have both an intention and implementation problem. In such cases, resolving the intention problem alone does not fully translate into behavioral change, as individuals still fail to implement a part of their intentions. As a consequence, only an adjusted share of the intention problem will cause mistakes that can be fixed through interventions. Below, we explain how our predictions can be adjusted to deal with concurrent problems.

Predicting Effectiveness of Interventions that Target Awareness Problems:

The predicted effect (PE) of an intervention that addresses 100% of the awareness problem is equal to the diagnosed extent of the awareness problem, which is quantified as the gap between believed (β) and actual performance (α) as seen in Formula 2.1. An adjustment for concurrent problems is not necessary, as concurrent implementation and intention problems lower believed performance and are therefore already captured by lower values of (β). The intervention will therefore fully resolve the awareness problem and translate into behavioral change.

$$PE(awareness) = max\{\beta - \alpha, 0\} \tag{2.1}$$

Predicting Effectiveness of Interventions that Target Intention Problems: To predict the effectiveness of an intention-tackling intervention, we need to consider whether there are concurrent awareness or implementation problems. If there are none, the gap between the policy goal and the intention can be fully closed by an intervention that addresses 100% of the intention problem. If individuals have concurrent awareness or implementation problems, however, an intervention that only addresses the intention problem will be less effective. Here, we have to take into account that, despite higher intentions, the individuals will still fail to implement a fraction of the tasks they intended to do. This could happen if the intention is not implemented into believed performance, a concurrent implementation problem, or if the believed performance is distorted and does not match actual performance, a concurrent awareness problem. For example, assume that someone only manages to convert 50% of the intended performance into actual performance due to concurrent problems. If an intervention now increases the intention and motivates an individual to attempt 10 additional tasks, we would argue that only 5 additional tasks will be eventually completed by the individual. Formula 2.2 shows how we adjust our prediction accordingly.

$$PE(intention) = max\{\gamma - i, 0\} \cdot (1 - |\frac{min\{\alpha - i, 0\}}{i}|) \tag{2.2}$$

The first factor shows the diagnosed extent of the intention problem, quantified by the gap between the policy goal γ and the intended performance i. The second factor becomes relevant in the presence of concurrent problems. It calculates which fraction of the intended tasks an individual manages to convert into actually completed tasks. If α equals i, for example, there are no concurrent problems, and subjects convert all of their intentions into actual behavior. If α is smaller than i, there are concurrent problems as the actual performance is smaller than the intended performance. These concurrent problems decrease the second factor, meaning that the fraction of intention that is being converted is also becoming smaller. Thus, the predicted effectiveness of an intention-addressing intervention decreases.

Predicting Effectiveness of Interventions that Target Implementation Problems: The extent of the implementation problem is quantified by the gap between the intended performance and the believed performance. Individuals are aware that they intended to solve more tasks than they believe they did, suggesting an implementation problem. To predict the effectiveness of an intervention that tackles 100% of the implementation problem, we need to consider whether there is a concurrent awareness problem. If there is not, the gap between the beliefs and the intention can be fully closed by an intervention addressing the implementation problem. If people have a concurrent awareness problem, however, the same intervention will be less effective. Beyond what the individual knowingly failed to implement, unawareness will cause the individual to still make mistakes. For example, assume that someone has an awareness problem, which leads to only completing 80% of the tasks that they believed to have solved. If an intervention now solves the implementation problem and the individual believes to have solved 10 additional tasks, we would argue that only 8 additional of those tasks will actually be solved due to the awareness problem. Formula 2.3 shows how we adjust our prediction accordingly.

$$PE(implementation) = max\{i - \beta, 0\} \cdot (1 - |\frac{min\{\alpha - \beta, 0\}}{\beta}|) \tag{2.3}$$

The first factor measures the diagnosed extent of the implementation problem, which is quantified as the difference between intention i and the believed performance β on a task. The second factor accounts for potential awareness problems. It measures the fraction of tasks that the individual believes they have completed that fall prey to the awareness problem. If α equals β, for example, there is no awareness problem. If α is smaller than β, there is a concurrent awareness problem, as actual performance is smaller than the believed performance. This decreases the second factor, meaning that unawareness causes a lower fraction of tasks to be solved. As a consequence, the overall predicted effectiveness of an implementation problem-addressing intervention decreases.

Formulas 2.2 and 2.3 use a notation with absolute values so that the formula is applicable in settings where less is better (negative values) and more is better (positive values). For the former settings with negative values, the maximum functions in all formulas turn into minimum functions and vice versa to maintain applicability across settings. A graphical illustration and description of how the fundamental problems are intertwined is given in Figure A.1.

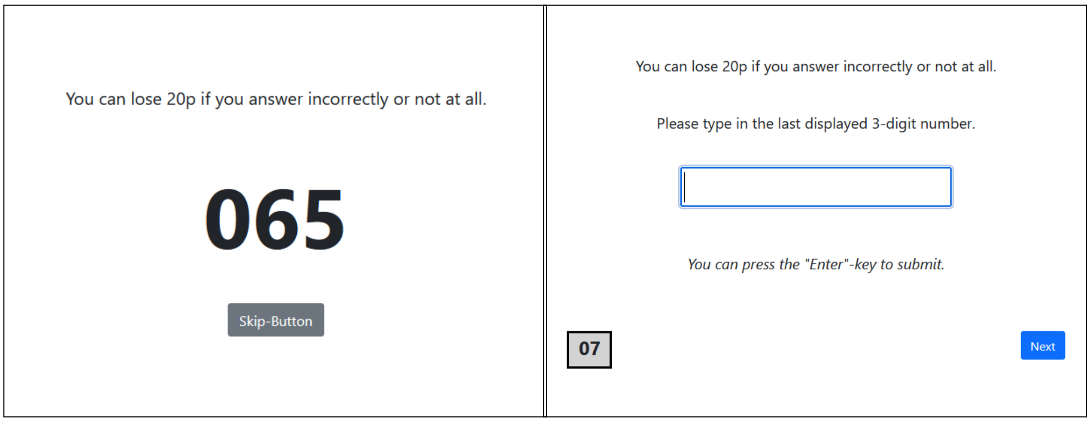
2.3 Experimental Test of the Framework

We designed an online experiment with two purposes: First, to show that the effectiveness of an intervention hinges on the underlying fundamental problem, leading to heterogeneous treatment effects across settings. Second, to show that we can use our anamnesis to diagnose the fundamental problem and predict the effectiveness of interventions. With these goals in mind, we generated three settings in which we exogenously induce either awareness, intention, or implementation problems. Within each setting, we then tested three interventions (reminders, incentives, simplifications) against a baseline condition, leading to a total of 12 experimental conditions.[6]

[6]This experiment was preregistered at the AEA registry (AEARCTR-0009809). IRB approval was obtained from the University of Cologne (220006MS).

FIGURE 2.1: Example Screens RET - Baseline

A: Numbers displayed during task B: Query to enter last displayed number



Notes: The figure shows screenshots of the real-effort task from the baseline group of Setting AWA. Panel A shows the screen of the changing numbers with the potential loss for a mistake above the number and the skip button below. Panel B shows the screen of the query to type in the last displayed number where the potential loss is displayed as well. The structure of the screen and task is identical for all experimental conditions.

2.3.1 The Real-Effort Task

Performance on a task is measured through a modified version of the real-effort task (RET) by Toussaert (2018). Participants see a three-digit number on their screen that changes every 1.2 seconds. In random intervals, they are asked to enter the last displayed number into an input field within 7 seconds. Example screens can be found in Figure 2.1. The task has an exception rule: If the last displayed number includes the digit "3", participants have to enter only "0" into the input field to provide a correct answer. This exception is explained to participants in a salient manner in the instructions. The task lasts for 10 minutes, during which participants are queried 50 times. The queries contain the digit "3" 20 times, which is explained to participants. The instructions state that the goal is to answer all 50 queries correctly.

Participants can skip the task and watch a video instead. If they choose to do so, any remaining queries are counted as incorrect. The skip button is placed directly below the changing 3-digit number, which directs participants to the video. Participants are informed that skipping the task has no consequences other than influencing their payments, as the remaining queries are counted as incorrect. Participants' performance is incentivized using a loss framing. They receive an endowment of $2.68[7]. From all 50 queries, 5 are selected randomly to be payoff-relevant. For an incorrect answer, participants lose $0.23 such that they can lose up to $1.15 but end up with at least $1.53. We chose a loss framing as it has been shown to effectively incentivize performance in online settings (DellaVigna & Pope, 2018b) and in the field (Fryer et al., 2022; Hossain & List, 2012).

[7] Participants were paid in £ as standard currency in Prolific. All values were multiplied by 1.14 based on the average exchange rate to US$ during data collection from September 22 to November 28, 2022.

After the task or the video, participants answer the anamnesis questions. In addition, we elicit sociodemographic information[8] and measure economic preferences[9]. After the survey, participants are informed about their payments, and are redirected to Prolific to finish the experiment. The experiment was programmed in oTree (Chen, Schonger & Wickens, 2016).

2.3.2 Three Settings

Our objective is to generate three distinct settings that exogenously induce awareness, intention, and implementation problems. To achieve this, we modified the real-effort task described above for each setting. Screenshots of the tasks for each experimental group are shown in Appendix A.3. We end up with the following three settings that are used as baseline conditions:

- **Setting AWA**: Lower salience of the exception rule.
  To induce awareness problems, we made the exception rule less salient in the instructions. Instead of being highlighted in its own paragraph, it was embedded within another paragraph alongside other important features of the real-effort task. We hypothesized that participants would be less likely to read the exception rule carefully and more likely to forget to apply it during the task.
- **Setting INT**: Flat fee for performance.
  To induce intention problems, we removed the piece-rate incentives. Participants received a flat fee of $1.54 for participation and were explicitly informed that the number of correct answers would not affect their payment.
- **Setting IMP**: Increase task complexity and temptation to skip RET.
  To induce implementation problems, we increased the implementation costs by making two adjustments. First, we increased the numbers displayed in the task from three to five digits, making memorizing the last displayed number more tedious. Second, we made skipping the tasks more tempting, as participants were given the option to skip the task entirely without having to watch a video for the remaining time.[10] This made skipping the task altogether more tempting to participants.

Interventions: We test three types of interventions – reminders, incentives, and simplifications – against the baseline of each setting. This results in a total of 12 experimental conditions. The goal is to demonstrate that the effectiveness of the three interventions depends on the underlying problem and that our framework can predict the intervention's success. For each setting, we therefore have the baseline condition and the following three intervention groups:

[8] We elicit age, gender, educational background, income level, occupational status, household size, size of resident city, and political preference.

[9] Following the preference survey module by Falk et al. (2023), we elicit risk, time, trust, altruism, and positive reciprocity preferences. Additionally, we include a question on competition preferences using the same format. The order of the preference questions is randomized.

[10] The skip button immediately led to the anamnesis questions.

- **Reminders**: Reminder of the exception rule.
  To address an awareness problem, we remind participants to apply the exception rule. The reminder to enter "0" if the last displayed number contained a "3" is placed on the screen immediately before the task begins and remains visible on top of the screen. This aims to increase participants' awareness of the exception rule and prevents them from forgetting it during the task.

- **Incentives**: Increased pay for performance.
  To address an intention problem, we increased the monetary incentives. The initial endowment is set to \$3.25, and participants now lose \$0.34 for each incorrect answer among the five randomly chosen pay-off relevant queries. This aims to increase the intended performance.

- **Simplifications**: Reduce task complexity and temptation to skip RET.
  To address an implementation problem, we simplified the task by requiring participants to memorize only two digits and eliminated the option to skip the task to remove any temptation. These modifications aim to reduce the implementation costs and help participants answer as many queries correctly as intended.

2.3.3 Anamnesis

In our set-up, we observe actual behavior and the policy goal is communicated to participants in the instructions. Under these conditions, our anamnesis relies on two questions. To elicit intention, we asked participants "Please be honest. Think back to the beginning of the study. How many of the 50 queries did you plan to answer correctly after reading the instructions?". Participants answered on a 0-50 scale. We elicited performance beliefs by asking "How many of the 50 queries do you think you answered correctly?" (0-50 scale). The belief elicitation was incentivized. Participants received \$0.11 for a correct guess, \$0.06 for a deviation of 1, and \$0.02 for a deviation of 2 queries. From the answers of participants in the baseline conditions, we diagnose the extent of the problems and make predictions about the effectiveness of the interventions, as explained in section 2.2.3.

We do not allow participants to have "negative problems", i.e., participants who are aware that they might have answered fewer queries correctly than they actually did are not considered to have an awareness problem. Similarly, someone who believes to have answered more queries correctly than intended is not considered to have an implementation problem. In these cases, the extent of the problem is set to 0. Stating an intention to solve more than 50 queries was impossible.

2.3.4 Sample, Randomization, and Balance

We collected data on the platform Prolific between September and November 2022. The sample consists of US participants fluent in English and with an approval rate of $\geq$95%. Participation from mobile devices was not allowed to maintain the functionality of the real-effort task. We use several measures such as Captchas, honey pots, and attention checks to prevent computer-generated answers in our experiment.[11]

Of the 8,312 participants who started the experiment, 106 were screened out or dropped out before being assigned to a treatment group, leaving 8,206 participants who were assigned to a specific treatment. Another 689 participants dropped out before finishing the experiment, and 17 failed the attention check, resulting in the pre-registered 7,500 participants who completed the experiment. We stratify treatment assignments by age, gender[12], and college education. The experimental groups are balanced. The balance table for the intention-to-treat (ITT) sample (including drop-outs) is displayed in Table A.2. Table A.1 shows the balance of the participants who finished the experiment. There is a slightly higher rate of dropouts in the implementation-problem groups. To show that this does not substantially confound our results, we will use our framework to predict average treatment effects for those who finished the experiment, as well as for the intent-to-treat sample in the Appendix. Figure A.2 displays a flowchart of the sampling process in detail. The average payment was $2.37 for 12:49 minutes, equivalent to $11.09 per hour.

2.4 Hypotheses and Results

This section describes the heterogeneity of treatment effects in reducing mistakes in the real-effort task across settings and whether our framework can predict it. We first discuss the results from the Setting AWA, then from the Setting INT, and finally from the Setting IMP. The analysis is structured along our preregistered hypotheses (AEARCTR-0009809).[13]

2.4.1 Awareness Intervention

H1a *Reminders are most effective at reducing mistakes in Setting AWA.*

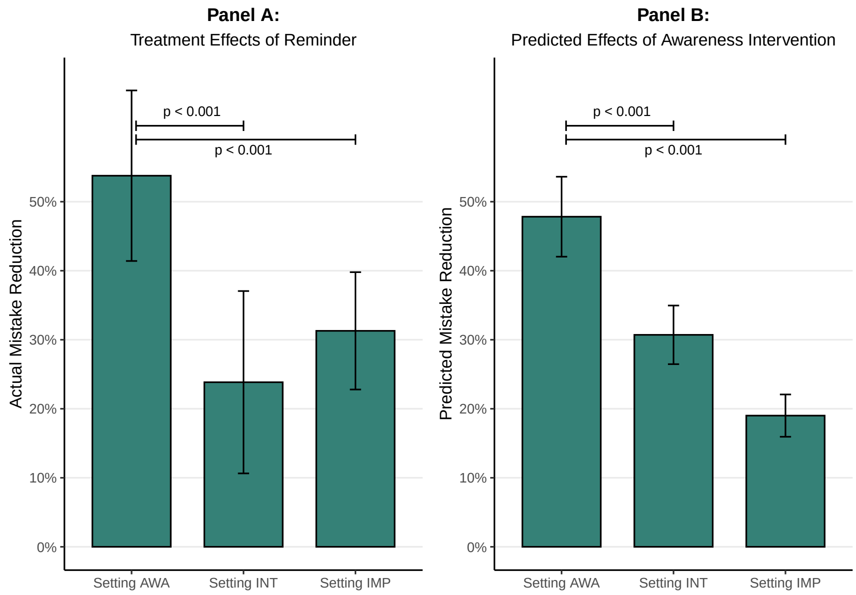
Panel A of Figure 2.2 displays the treatment effects of reminders in comparison to the baseline across settings. We normalize effect sizes by the respective baseline groups' average mistakes for comparability. That is, Panel A shows the percentage of baseline group mistakes that were resolved through the reminders. As hypothesized, we find that the effectiveness of reminders varies strongly across settings. The reminders in the awareness setting reduce 54% of the baseline group's mistakes from 10.12 to 4.68, which is larger than in

[11] Participants were required to complete a Captcha. Those who failed the initial Captcha were given a second attempt. If they failed again, they were excluded from the study. Only one participant was excluded due to failure to solve both Captchas. Subsequently, participants were prompted to enter their Prolific ID. We identified and excluded 64 cases of individuals attempting to participate multiple times. To further safeguard against non-human participants, a hidden honeypot question – requesting the participant's name but invisible to human respondents – was employed. As anticipated, no responses were recorded for this question, indicating that bot interference is negligible in our experiment. Finally, an attention check was embedded in the post-task survey. This check required participants to select a specific response on a Likert scale.

[12] We additionally balance by gender through the option offered by Prolific.

[13] In the preregistration, the hypotheses were structured by effectiveness and prediction of the interventions. Here, we structure them by the type of fundamental problem and intervention.

FIGURE 2.2: Treatment Effects and Predictions of an Awareness-Addressing Intervention



Notes: Panel A shows the treatment effects of reminders across the three settings. Panel B shows the predicted effects of an intervention that tackles the awareness problem, calculated as described in section 2.2. All effects are reported relative to the mistakes in the baseline groups per setting. The whiskers show the 95%-confidence intervals of the means. The p-values in Panel A correspond to Wald-tests, comparing the size of the respective treatment effect coefficients from an OLS model. The p-values in Panel B correspond to two-sided t-tests, comparing the average effect sizes of the predictions.

the intention and implementation setting (Wald-tests: $p<0.001$, $p<0.001$). Reminders are also effective in the other two settings, yet their effect size is substantially lower. In Setting INT, the actual treatment effect is 24%, reducing mistakes from 12.59 to 9.58. In Setting IMP, the actual treatment effect is 30%, reducing mistakes from 19.61 to 13.47. The corresponding regression statistics are shown in columns 1 and 2 of Table A.3. We find similar patterns when using the ITT sample (Figure A.3).

H1b *A higher predicted effectiveness of reminders is associated with a higher actual effectiveness.*[14]

We now evaluate whether our framework can predict effect sizes across settings. Panel B of Figure 2.2 shows predicted effect sizes (see section 2.2.3). As with the actual effect sizes in Panel A, the predicted effect sizes are normalized by mistakes in the respective baseline group. Overall, we find strong empirical support in line with H1b. We predict that an intervention tackling unawareness would resolve 48% of the mistakes in the awareness problem setting, which is close to the actual effect size of 54%. As hypothesized, the predicted effect in Setting AWA is significantly larger than in the other two settings (t-tests: $p<0.001$, $p<0.001$). In Setting INT, we predict that reminders could reduce mistakes by 30%, while the actual treatment effect is 24%. In

[14] The predicted effectiveness is the share of awareness-problem-driven mistakes that can be reduced by resolving the awareness problem alone (see Section 2.2.3). In other words, due to concurrent problems, only a share of the awareness problem will cause mistakes that can be fixed through interventions. In our preregistration, we write that "a higher share of diagnosed awareness barriers predicts higher effectiveness", which is less precise.

TABLE 2.2: Prediction of Awareness-Intervention Effects by the Framework

	Predicted Mistake Reduction (standardized)		
	Individual Level	Aggregate Level	Aggregate Level - No Intercept
Dependent Variable:	(1)	(2)	(3)
Actual Mistake Reduction (standardized)	0.034*** (0.005)	0.851*** (0.211)	1.085*** (0.082)

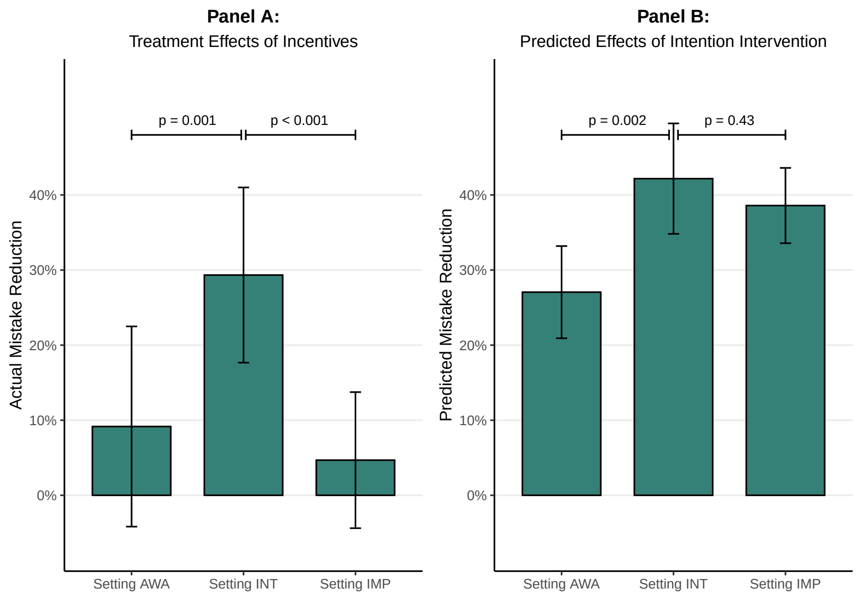
Notes: This table shows OLS results of the predicted mistake reduction in percent on the actual mistake reduction due to reminders in percent. The predictions are based on our framework as explained in section 2.2.3. The actual mistake reduction corresponds to the average intervention effects compared to the baseline group in each respective setting. Both mistake reductions are standardized by dividing them by the baseline mistakes of the respective setting. Column 1 uses individual-level predictions as the independent variable and reports robust standard errors in parentheses. Standard errors for the aggregate-level predictions in columns 2 and 3 are obtained via bootstrapping with 1000 repetitions. For each repetition, resample the original sample, calculate the mean predicted and actual effects, and perform the OLS analysis with and without allowing for an intercept. The standard deviations of these bootstrapped coefficients are used as standard errors. *p<0.1; **p<0.05; ***p<0.01

Setting IMP, we predict an effectiveness of 19%, while the actual treatment effect is 30%. Note that while we did not intend to induce awareness problems in Settings INT and IMP, our framework nevertheless accurately predicts reminders to be effective in those settings.

In Table 2.2, we use OLS regressions to evaluate whether there is a significant correlation between predicted and actual treatment effects. We use two approaches: First, we regress the individual-level predictions on the three average treatment effects, displayed in column 1. That is, we use the 1,874 individual predictions in the three baseline conditions as explanatory variable. The outcome variable is the corresponding average treatment effects of reminders, as we do not observe individual-level treatment effects. Column 1 shows that individual predictions significantly correlate with the average treatment effect ($p<0.001$). We find similar results using predictions from the uncorrected diagnosis values and the ITT sample, displayed in Tables A.5 and A.6. Estimated coefficients on the individual level are quite small, which can be explained by the fact that we do not observe individual-level treatment effects. Correlations coefficients are much larger when we regress the average predictions on the average treatment effects, which is presented in column 2. Here, we find that a 1 percentage point reduction in predicted effectiveness translates to a 0.851 percentage point reduction ($p<0.001$, bootstrapped s.e.) in actual effectiveness. These findings establish strong evidence that our framework can successfully predict the treatment effects of the reminders.

Finally, one might be interested in how effective our predictions are from an ex-ante perspective. Here, the intercept with the y-axis would be unknown. Column 3 regresses the average predictions on the average treatment effect but without estimating an intercept (i.e., forcing the prediction of zero to go through the null point). We find that a predicted mistake reduction of 100% translates into an actual mistake reduction of 108.5%, which is remarkably accurate.

FIGURE 2.3: Treatment Effects and Predictions of an Intention-Addressing Intervention



Notes: Panel A shows the average treatment effects of increased incentives across the three settings. Panel B shows the predicted effects of an intervention that tackles the intention problem, calculated as described in section 2.2. All effects are reported relative to the mistakes in the baseline groups per setting. The whiskers show the 95%-confidence intervals of the means. The p-values in Panel A correspond to Wald-tests, comparing the size of the respective treatment effect coefficients from an OLS model. The p-values in Panel B correspond to two-sided t-tests, comparing the average effect sizes of the predictions.

2.4.2 Intention Intervention

H2a *Incentives are most effective at reducing mistakes in Setting INT.*

Panel A of Figure 2.3 shows the treatment effects of increased incentives across the three settings. As with reminders, incentives show a high level of heterogeneity across settings. Higher monetary incentives are most effective in Setting INT, where they reduce mistakes by 29% from 12.59 to 8.89. In line with our pre-registration, treatment effects are significantly smaller in the other two settings (Wald-tests: p=0.001, p<0.001). The treatment effects for incentives are not statistically different from zero in Setting AWA and IMP. The corresponding regression statistics are shown in columns 3 and 4 of Table A.3. We find similar patterns when using the ITT effects (Figure A.4).

H2b *A higher predicted effectiveness of incentives is associated with a higher actual effectiveness.*

Panel B shows the predicted treatment effects. In line with actual treatment effects, predictions are the largest for Setting INT, with a predicted reduction of mistakes of 42%. This is significantly larger than the predicted effect for Setting AWA (t-test: p=0.002), but not Setting IMP (t-test: p=0.43). While the rank ordering of predictions aligns with the actual effects, the predictions seem to overshoot. We predict an effectiveness of increased incentives in Setting AWA of 27% and of 39% in Setting IMP, whereas actual treatment effects are insignificantly different from zero. An explanation for this overshooting

TABLE 2.3: Prediction of Intention-Intervention Effects by the Framework

	Predicted Mistake Reduction (standardized)		
	Individual Level	Aggregate Level	Aggregate Level - No Intercept
Dependent Variable:	(1)	(2)	(3)
Actual Mistake Reduction (standardized)	0.006* (0.003)	0.912 (0.740)	0.416*** (0.079)

Notes: This table shows OLS results of the predicted mistake reduction in percent on the actual mistake reduction due to incentives in percent. The predictions are based on our framework as explained in section 2.2.3. The actual mistake reduction corresponds to the average treatment effects compared to the baseline in each respective setting. Both mistake reductions are standardized by dividing them by the baseline mistakes of the respective setting. For column 1, we use the predictions on the individual level as independent variable with robust standard errors in parentheses. To obtain standard errors for the aggregate levels in columns 2 and 3, we used bootstrapping analogous to Table 2.2.
*p<0.1; **p<0.05; ***p<0.01

is that the incentives were not large enough to address intention problems, which we discuss in detail in subsection 2.4.4.

Although predictions seem too high, we find that there is a significant correlation between individual-level predicted effects and the actual treatment effects. Column 1 of Table 2.3 shows that individual-level predictions correlate with the average treatment effect (p=0.055). Tables A.5 and A.6 provide estimations for predictions using the uncorrected diagnosis values and the ITT sample. When using the aggregated predictions, we find a high correlation coefficient of 0.912 percentage point reduction in actual effectiveness (see column 2), yet this coefficient is statistically insignificant from zero.

As previously, column 3 shows ex-ante prediction when the intercept with the y-axis is unknown. We find that a predicted mistake reduction of 100% translates into an actual mistake reduction of 41.6%, which corroborates that our predictions overshoot the actual effectiveness of the incentives.

2.4.3 Implementation Intervention

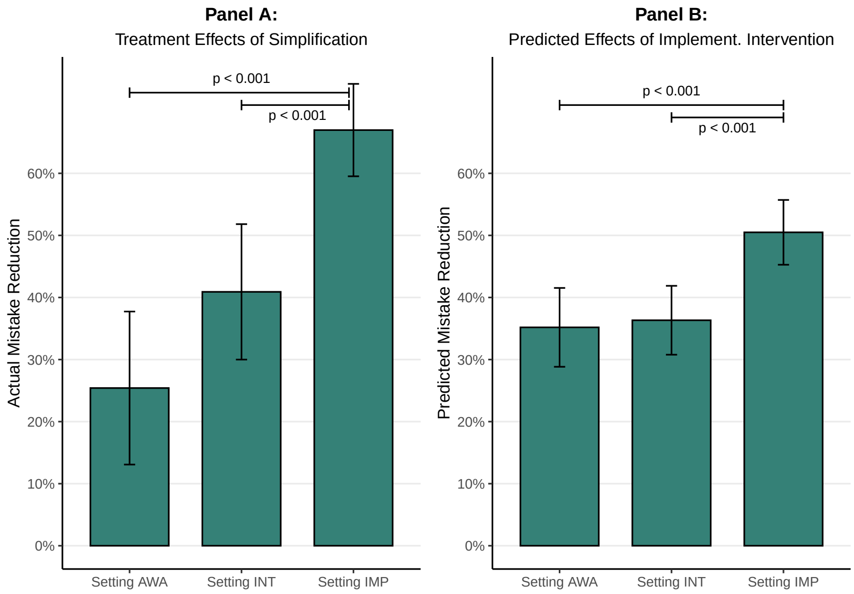
H3a *Simplifications are most effective at reducing mistakes in Setting IMP.*

Panel A of Figure 2.4 shows treatment effects of the simplification across settings. As hypothesized, simplifications show a high level of setting specificity as well. In the Setting IMP, they reduce mistakes by 67% from 19.61 to 6.48. This effect is significantly larger than in the other settings (Wald-tests: p<0.001, p<0.001), where mistakes are reduced by 25% from 10.12 to 7.55 in Setting AWA, and by 41% from 12.59 to 7.44 in Setting INT. The corresponding regression statistics are shown in columns 5 and 6 of Table A.3 of the Appendix. We find similar patterns when using the ITT effects (Figure A.5).

H3b *A higher predicted effectiveness of simplifications is associated with a higher actual effectiveness.*

Panel B shows the predicted effect of an implementation-tackling intervention on mistakes. In line with the actual treatment effects, we find that predictions are the largest for the implementation problem setting. In Setting IMP, we predict a reduction of 51%, which is significantly larger compared to the

FIGURE 2.4: Treatment Effects and Predictions of an Implementation-Addressing Intervention



Notes: Panel A shows the average treatment effects of simplifications across the three settings. Panel B shows the predicted effects of an intervention that tackles the implementation problem, calculated as described in section 2.2. All effects are reported relative to the mistakes in the baseline groups per setting. The whiskers show the 95%-confidence intervals of the means. The p-values in Panel A correspond to Wald-tests, comparing the size of the respective treatment effect coefficients from an OLS model. The p-values in Panel B correspond to two-sided t-tests, comparing the average effect sizes of the predictions.

other two settings (t-tests: $p<0.001$, $p<0.001$), where we predict reductions of 35% and 36%.

Again, we find that there is a significant correlation between individual-level predicted effects and the actual treatment effects. Column 1 of Table 2.4 shows that individual predictions significantly correlate with the average treatment effect ($p<0.001$). This is robust to using the uncorrected diagnosis values and the ITT effects (see Tables A.5 and A.6). Column 2 shows a high correlation coefficient between aggregated predictions and actual effectiveness. A 1 percentage point reduction in predicted effectiveness translates to a 2.345 percentage point ($p<0.001$) reduction in actual effectiveness.

Column 3 shows ex-ante prediction. We find that a predicted mistake reduction of 100% translates into an actual mistake reduction of 112.8%. Hence, our predictions for the simplification are close to 1, yet slightly undershoot the actual effects.

In sum, our results show that treatment effects are highly context-specific, and that our framework can predict this context-specificity. When aggregating across all settings, we find that c.p. a 1 percentage point increase in the predicted effectiveness corresponds to a 1.2 percentage point increase in actual effectiveness, as shown in column 2 of Table A.7. As column 3 shows, predicting an effectiveness of 100% ex-ante translates into an actual effectiveness of 89.2%, which underlines the accuracy of our prediction based on the simple anamnesis.

TABLE 2.4: Prediction of Implementation-Intervention Effects by the Framework

	Predicted Mistake Reduction (standardized)		
	Individual Level	Aggregate Level	Aggregate Level - No Intercept
Dependent Variable:	(1)	(2)	(3)
Actual Mistake Reduction (standardized)	0.020*** (0.006)	2.345*** (0.664)	1.128*** (0.062)

Notes: This table shows OLS results of the predicted mistake reduction in percent on the actual mistake reduction due to simplifications in percent. The predictions are based on our framework as explained in section 2.2.3. The actual mistake reduction corresponds to the average treatment effects compared to the baseline in each respective setting. Both mistake reductions are standardized by dividing them by the baseline mistakes of the respective setting. For column 1, we use the predictions on the individual level as independent variable with robust standard errors in parentheses. To obtain standard errors for the aggregate levels in columns 2 and 3, we used bootstrapping analogous to Table 2.2.
*p<0.1; **p<0.05; ***p<0.01

The average effect of the interventions with the highest predicted effectiveness per setting is 50.02% while the average effect of all interventions in our settings is 31.71%. That means that prescribing an intervention based on our framework increases the effect size by around 58% compared to randomly choosing one of the tested interventions, which highlights the value of conducting a diagnosis before choosing an intervention.

2.4.4 Improving Predictability by Refining the Assumptions

While our predictions are quite precise overall, they perform better in some settings than in others. Most notably, we find that the predictions for the monetary incentives were too high compared to the actual treatment effects. A key explanation for this is that our assumptions regarding the effectiveness of the interventions were too optimistic. In other words, the additional monetary incentive may not have resolved 100% of the intention problem. Our framework allows us to analyze the degree to which the interventions affected the underlying problem by comparing differences in diagnoses between the baseline and the reminder group, which is shown in Table 2.5. If our assumptions about the effectiveness of our interventions were true (see Section 2.2.3), we would expect that the reminders completely solve the awareness problem but not the intention and implementation problems. That is, in Panel A, column 1 would show values of -1, and columns 2 and 3 would show 0. Equivalently, for Panel B, we would expect values of -1 in column 2 and 0 otherwise. For Panel C, we would expect values of -1 in column 3 and 0 otherwise.

Column 1 of Panel A shows to which extent the reminders actually reduce the awareness problem. In Setting AWA, reminders reduce the diagnosed awareness problems by 81%, reducing its diagnosed extent from 4.84 to 0.93 mistakes. In Settings INT and IMP, reminders reduce the awareness problem by 56% and 53%. Columns 2 and 3 show that reminders did not significantly affect the diagnoses of intention and implementation problems. We conclude that our predictions for reminders are quite precise because our reminders largely work as assumed: They consistently reduce awareness problems without impacting the other two problems.

TABLE 2.5: Relative Intervention Effects on Diagnosed Problems

	Change in Diagnosed Problems		
Experimental Group	Awareness (1)	Intention (2)	Implementation (3)
Panel A: Reminders			
Setting AWA	-0.81***	-0.20	0.00
Setting INT	-0.56***	-0.07	0.03
Setting IMP	-0.53***	-0.09	-0.12
Panel B: Incentives			
Setting AWA	-0.08	0.08	-0.12
Setting INT	0.22**	-0.53***	-0.31**
Setting IMP	-0.03	-0.03	-0.07
Panel C: Simplification			
Setting AWA	0.05	-0.49***	-0.42***
Setting INT	0.01	-0.62***	-0.32***
Setting IMP	0.06	-0.79***	-0.77***

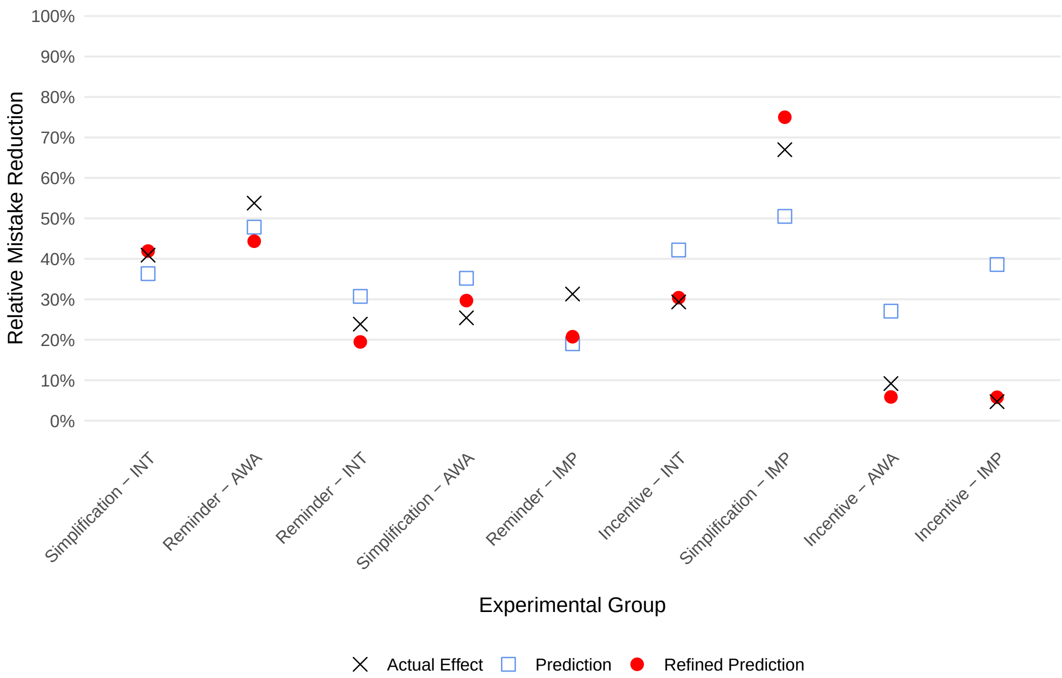
Notes: The table shows the intervention effects on the extent of diagnosed problems for each experimental setting. All values show the change of diagnosed problems relative to the baseline group's problems of the respective setting. Negative values indicate that the extent of the underlying problem was reduced.

The assumptions regarding the incentives were too optimistic. As shown in Panel B, column 2, the incentives in Setting INT solved half of the intention problem (53%), reducing its diagnosed extent from 6.33 to 2.95 mistakes. Moreover, we find that the monetary incentive slightly increased awareness problems by 22% (column 1) and reduced implementation problems by 31% (column 3). In the other two settings, the intention problem was not resolved at all, and neither of the other problems was affected. Hence, substantial intention problems remain in those two settings, as the extra monetary incentive may have been insufficient to tackle the intention problem. Why were incentives in setting INT more effective in addressing the intention problem? In Setting INT, the relative increase in payments was larger. Participants in the baseline group received no pay-for-performance, whereas the treatment group received a potential bonus of \$1.71, compared to the other two settings where the bonus was just increased from a possible \$1.14 to \$1.71. The marginal increase of the monetary incentive in the other two settings seems to have been too small to resolve the intention problem.

Panel C evaluates our assumption regarding simplifications. As intended, simplifications reduced implementation problems across all settings (column 3). For example, in Setting IMP, simplifications solved 77% of the implementation problem, reducing its diagnosed extent from 11.09 to 2.53 mistakes. Awareness problems are not affected by simplifications (column 1). We did not anticipate, however, the degree to which the simplification also addresses intention problems. Column 2 shows that simplifications substantially reduced intention problems between 49% and 79%. As a result, actual treatment effects overshoot their predictions as simplifications not only resolve implementation problems but also intention problems.

Could our predictions be improved if we had a better knowledge of the extent to which treatments address the underlying problems? To see whether refined assumptions also lead to better predictions, we use the values from

FIGURE 2.5: Precision of the Intervention Effect Prediction



Notes: The figure shows the prediction of intervention effects on the reduction of mistakes compared to the actual effects found for all interventions and settings. We show both the predicted effects and the refined predictions. The experimental groups on the x-axis are ordered from left to right by the precision of the predicted effects.

Table 2.5 instead of assuming that an intervention addresses only one type of problem and to 100%. Understanding whether refined assumptions improve predictability is important as it would not only underscore the potential of our framework but also because policymakers and researchers might learn to form (or already have) more accurate assumptions about how treatments affect the underlying problem types.

To make the refined adjustments, we simply multiply our predicted effects (Section 2.2.3) by the fraction of how well an intervention actually resolves the respective fundamental problem in each setting. Furthermore, we relax the assumption that treatment may only affect one of the underlying problems. Here, the predictions for affecting the awareness, intention, and implementation problems are simply added per intervention (discounted by the degree to which they actually reduce the underlying problem).

Figure 2.5 plots standard and refined predictions as well as actual treatment effects across our nine interventions. Effects and predictions are ordered by their precision from left to right. Blue squares indicate our standard assumptions, red dots indicate the refined assumptions, and black crosses the actual treatment effects. Figure 2.5 mirrors the findings from the results section. There exists substantial heterogeneity of treatment effects across settings. This heterogeneity is already quite precisely predicted under our standard assumptions. Strikingly, we find that we can strongly improve predictability when we use the refined predictions. This becomes most evident as we move to the right of the panel, where the standard predictions become

TABLE 2.6: Refined Prediction of Intervention Effects by the Framework

	Actual Mistake Reduction (standardized)			
	Reminders (1)	Incentives (2)	Simplification (3)	Pooled (4)
Panel A: Aggregate Level				
Predicted Mistake Reduction	1.090***	0.913***	0.890***	0.883***
(refined, standardized)	(0.267)	(0.266)	(0.122)	(0.055)
Panel B: Aggregate Level - No Intercept				
Predicted Mistake Reduction	1.260***	0.981***	0.907***	0.994***
(refined, standardized)	(0.104)	(0.170)	(0.042)	(0.051)

Notes: This table shows OLS results of the refined predicted mistake reduction based on our framework on the actual mistake reduction due to the intervention. We use the diagnosis of the baseline settings in each of the three settings for the prediction of how many mistakes are reduced due to one respective intervention. The actual reduction of mistakes per intervention and setting is used as the outcome variable. Both the predicted and the actual mistake reduction are standardized by the average mistakes of the baseline groups in each setting. Regression coefficients are in percentage terms. To obtain standard errors, we used bootstrapping to resample the original sample 1000 times, calculate the mean intervention and refined predicted effects for each setting, and perform the OLS analysis on the aggregate data with and without allowing for an intercept. The standard deviations of these bootstrapped coefficients are used as standard errors and reported in parentheses.
*p<0.1; **p<0.05; ***p<0.01

less precise. Here, the refined predictions, in contrast, are very close to the actual treatment effects. Table 2.6 provides corresponding regression statistics. When pooling across all treatments (column 4), we find that a 1 percentage point increase in the refined predicted effect translates into a 0.883 percentage point increase in the actual effect (Panel A). For the ex-ante prediction without intercept, we even find that a 100% predicted effectiveness translates into an actual effectiveness of 99.4% (Panel B). These results highlight the potential of our framework to predict treatment effects with high accuracy.

2.5 Conclusion

The critical role of diagnosing underlying fundamental problems prior to designing interventions has been recognized in previous literature. Yet, surprisingly few papers offer transparency regarding the rationale behind their choice of intervention within their specific setting. This lack of transparency is notable, especially given the large number of ineffective interventions documented in the literature. We argue that ineffective interventions and the lack of a systematic diagnosis are intertwined. In order to design effective interventions, we need a systematic diagnosis. Hitherto, however, the literature lacks a systematic, generalizable, and parsimonious framework to diagnose fundamental problems and predict intervention effectiveness. Our paper contributes to this gap by introducing an empirically validated framework.

Our approach prioritizes both practicality and parsimony, which ensures broad applicability. Inevitably, this can introduce measurement imperfections due to its reliance on subjects' self-reported recall of past intentions and beliefs. We view our framework as an initial tool to broadly identify the fundamental problem. Our framework is not restrictive; where appropriate, it can be easily extended, and potential biases can be rigorously tested using other methodologies. Despite its simplicity, we find that our framework performs well in predicting intervention effects. On average, an ex-ante predicted effectiveness of 100% translates into an actual effectiveness of interventions of 89.2%. Experts, in comparison, tend to largely overestimate treatment effects (DellaVigna & Linos, 2022; Milkman et al., 2021b).

While our framework can help to identify the type and extent of the fundamental problem, it is not directly informative about which type of intervention to use to address a certain problem. That is, there are multiple ways to address awareness, intention, and implementation problems. We believe that cost-effectiveness can be a guiding principle when considering intervention design. For example, when individuals do not plan to work more because of misperceptions regarding their net wage, correcting the misperceptions may be more cost-effective than increasing the actual net wage. For this to be effective, one would have to understand how widespread those misperceptions are. Another related point is that one can imagine that extremely high incentives could solve any problem. For example, offering someone a million dollars to eat an apple a day would likely resolve any related awareness, intention, and implementation problems. However, such an intervention would be far too costly when a simple reminder could also solve the awareness problem to a large extent.

The implications of our findings are far-reaching, particularly in terms of the selection and targeting of interventions. By understanding the underlying fundamental problems, policymakers, researchers, and practitioners can design more effective interventions to address the most pressing challenges of our time.

Chapter 3

Identifying the Barriers of Student Aid Take-Up: The Role of Misperceptions

*joint with Sascha Strobl**

3.1 Introduction

Social benefit programs aim to help people in need. Yet, non-take-up of entitlements is a persisting problem. Most programs across the world are taken up by only half or less of the eligible people (Ko & Moffitt, 2022). The main reasons are lack of information and too high transaction costs of applying (Currie, 2006; Eurofound, 2015). Both of these reasons describe different take-up barriers, however. At the eligibility barrier, people might not take up their entitlement because they do not think they are eligible. At the application barrier, some people might not take up their entitlement even though they know they are eligible. This raises the question of whether people know about their eligibility and, if so, whether they make an informed decision against take-up. To effectively increase take-up, it is necessary to understand the extent of each barrier and address the reasons for non-take-up accordingly.

As a tool to reduce social inequality in access to education, student aid programs face the same problems as general social benefits. Take-up falls short of eligibility rates both in the US and Europe (for US, see: Bettinger et al., 2012; Bird et al., 2021; Castleman & Page, 2016; Dynarski et al., 2021; Kofoed, 2017; for Europe, see: Booij, Leuven & Oosterbeek, 2012; Callender & Wilkinson, 2013; Fidan & Manger, 2021; Herber & Kalinowski, 2019). Providing information alone is often insufficient to increase take-up, which suggests students are aware of student aid programs (Bettinger et al., 2012; Bird et al., 2021; Booij, Leuven & Oosterbeek, 2012; Marx & Turner, 2020). Given that student aid is means-tested and often needs to be repaid at least partly, there is room for misperceptions, however. Students may know about the program but not about their own eligibility for it due to misperceptions about the eligibility conditions, posing a problem at the eligibility barrier. Additionally, students might not apply for student aid even though they know they are

*The idea for this study emerged from joint discussions between Sascha Strobl and me based on my idea for the experiment for Chapter 4. We designed the survey together and I collected the data. Sascha Strobl performed the microsimulation and the sample reweighting and we jointly analyzed the data. We wrote the first manuscript together and revised it multiple times.

eligible because of debt aversion, application complexity, or stigma, posing a problem at the application barrier. The question for this latter group is first if they understand the financial terms of student aid correctly and made an informed decision against take-up, or if they misperceive these terms and potentially also the extent of their entitlement.

Up to now, the extent of the barriers remains unclear as it is necessary to know the perceived eligibility. To close this gap, we conducted an online survey among 22,222 enrolled university students across Germany. Since universities do not charge tuition fees, Germany offers a unique context to study non-take-up as student aid is used to cover living expenses and, therefore, is closely related to general social benefit programs. We determine which students are eligible for student aid using a microsimulation model based on the students' sociodemographic and economic situation. By additionally eliciting perceived eligibility, we can identify the students who are eligible but do not believe they are, the eligibility barrier, and who know they are eligible but still do not take up aid, the application barrier. Additionally, we elicit misperceptions about student aid conditions through hypothetical scenarios and reasons for non-take-up. This unique set-up allows us to determine the specific drivers of non-take-up at both barriers.

To estimate the overall non-take-up rate, we use weights to adjust our sample so that it is representative of the general student body with respect to sociodemographic characteristics. We find that 70% of eligible students do not take up student aid. These students forgo €492 per month, on average. Among these eligible non-receivers, we identify an eligibility barrier of 82.2% who do not believe they are eligible. These *non-believers* have systematic misperceptions about the student aid conditions. We find that they significantly underestimate the monthly amounts of student aid and the eligibility thresholds for parents' income. Additionally, they overestimate the repayment amounts of student aid.

A main reason for non-take-up is that 59% do not think they are eligible because of their parents' income. The majority of them, however, underestimate the income thresholds for parents by at least 20%. Their misperceptions about the student aid conditions potentially led them to think they were not eligible even though they were. Correcting these misperceptions could, therefore, potentially increase take-up.

At the application barrier, we find that 13.2% of eligible non-receivers believe they are eligible for student aid but still do not take it up. On average, these *believers* know the eligibility conditions of student aid as well as the receivers, which suggests that they make an informed decision against student aid. Yet, they significantly overestimate the repayment amounts, similar to the non-believers. At the same time, 62% state they do not want to incur debt as a reason for non-take-up. Connecting the misperceptions and this reason, it is likely that some students do not take up student aid because of debt aversion while they misperceive the actual repayment conditions.

Additionally, when asked about the minimum amount of student aid they would apply for, 66% of the students indicate an amount smaller than their simulated entitlement. A back-of-the-envelope calculation shows that these students have an average reservation wage of €792 per hour for applying. Compared to a regular student wage of €15 per hour, this reservation wage underlines the role of misperceptions in student aid non-take-up.

While misperceptions impact both barriers, they also drive non-take-up overall. Comparing the student aid receivers to the eligible non-receivers, we find that students who have stronger misperceptions about the student aid conditions are less likely to receive student aid. Additionally, first-generation immigrants and students who do not know any student aid receiver are less likely to take up aid. This suggests that the network matters for take-up (Bertrand, Luttmer & Mullainathan, 2000). Students benefit from the knowledge of their family and friends to notice their eligibility and take up aid. Especially the students who do not have access to this knowledge suffer from the lack of information to tackle misperceptions.

We contribute to several strands of the literature. First, there is a large literature on the determinants of non-take-up of social benefits (e.g. Aizer, 2007; Bhargava & Manoli, 2015; Daponte, Sanders & Taylor, 1999; Finkelstein & Notowidigdo, 2019; Gray, 2019). In their extensive collection of benefit programs, Ko & Moffitt (2022) show that most programs across the world only have take-up rates around 50%. The most common drivers of non-take-up are lack of information about the programs and the entitlement, followed by the high transaction cost of applying for the benefits and stigma (Currie, 2006; Eurofound, 2015). Yet, to the best of our knowledge, no paper quantifies the eligibility and application barrier to take-up while connecting them to other drivers of non-take-up. This paper addresses this gap by identifying both eligible non-receivers who believe they are eligible and who do not believe they are eligible. Furthermore, we separately analyze their misperceptions about the conditions of the aid program and connect them to their stated reasons for non-take-up.

Second, we contribute to the literature on understanding the non-take-up of student aid. Using third-party observational data (Bettinger et al., 2019; Cadena & Keys, 2013; Erwin & Binder, 2020; Fidan & Manger, 2021; Herber & Kalinowski, 2019; Kofoed, 2017) and field experiments (Bettinger et al., 2012; Bird et al., 2021; Booij, Leuven & Oosterbeek, 2012; Castleman & Page, 2016; Dynarski et al., 2021; Hoxby & Turner, 2015; Marx & Turner, 2020), the existing literature identified complexity of the application as an important determinant of non-take-up, while evidence on other determinants such as the lack of information or debt aversion is mixed. Yet, no study can identify the group of eligible non-receivers of student aid while eliciting their perceived eligibility. This is necessary to provide evidence for the determinants of non-take-up separately at the eligibility and application barrier. We close this gap by using a large-scale online survey where we identify the eligibility and application barrier to take-up and show that the determinants of non-take-up differ between these groups, especially with respect to misperceptions about student aid.

Third, we contribute to the literature that studies misperceptions in decision-making. Misperceptions have been studied in the realm of schooling (Jensen, 2010; Kaufmann, 2014; Reuben, Wiswall & Zafar, 2017), COVID-vaccinations (Bartoš et al., 2022), investment behavior (Haaland & Næss, 2023), insurance demand (Domurat, Menashe & Yin, 2021), and also student aid (Booij, Leuven & Oosterbeek, 2012; Riedmiller, 2025). We add to this literature by showing how different misperceptions about student aid relate to actively deciding for non-take-up and non-take-up due to perceived ineligibility.

The remainder of this Chapter is organized as follows. We first explain the institutional context of student aid in Germany in Section 3.2 before we describe our data collection and sample in Section 3.3. Results for the determinants of non-take-up and misperceptions are discussed in Section 3.4, and the paper is concluded in Section 3.5.

3.2 Institutional Context

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). Yet, take-up is low. Only 11% of students receive federal student aid with a decreasing tendency (Destatis, 2023), and microsimulations show that at least 40% of the eligible students do not take up their entitlement (Herber & Kalinowski, 2019). It is unclear, however, how many do not take up the aid because of perceived ineligibility (eligibility barrier) or because they actively decide against it even though they know about their eligibility (application barrier).

To be eligible for student aid, one has to fulfill certain institutional requirements. Students can only receive student aid if they are German or have a permanent residence permit. Additionally, one has to start studying before turning 45 to be eligible. Some study programs are excluded from student aid, such as PhD programs or part-time programs. For the other program, eligibility is restricted to the standard period of study, which is five years for most programs. Students who study longer than this are institutionally ineligible. Even when we only consider institutionally eligible students, only 14.9% of students receive federal student aid (Deutscher Bundestag, 2023).

Perceived ineligibility might stem from underestimating the income thresholds for parents. A married couple with one child can have an annual gross household income of up to €85,000. With two children, parents can earn up to €120,000 annually until the child is no longer eligible for student aid. The administration uses this income to calculate how much parents can contribute to the cost of living of the student. The maximum amount a student can receive is €934 per month[1], comparable to a combination of a Pell Grant and a Direct Subsidized Loan in the US. The student's own income beyond €520 per month is deducted from their monthly entitlement.[2] Given their extent, students potentially misperceive the income thresholds and, consequentially, their own eligibility for student aid.

If students know about their eligibility, they may misperceive the repayment conditions of student aid. Repayment starts five years after the funding period, so usually when the students have already entered the labor market. Only half of the cumulative student aid amount is a loan that must be repaid, while the other half is a scholarship. In contrast to student aid in other countries like the US and the UK, the loan remains interest-free until repaid, so

[1]If the student is covered by their parents' health insurance, the maximum amount is €812.

[2]Having a student job on the side is allowed without deductions. The standard wage for a tax-free so-called "mini-job" in Germany is €520.

the debt does not increase over time. Additionally, it is capped at €10,010, so the debt never exceeds this amount, irrespective of the actual aid received. If the student repays their debt in one sum, another discount of up to 21% on the loan applies. The repayment can be deferred if the debtor has a net income below €1,605 per month, and completely abated after 20 years, similar to the income-driven repayment in the US. Students potentially misperceive the debt accumulation from student aid and therefore decide against take-up despite eligibility.

Determining how many students do not claim their entitlement due to perceived ineligibility or despite known eligibility is necessary to effectively increase take-up. From a welfare perspective, increasing take-up is beneficial as 76.1% of students in Germany who live alone or with other students are at risk of poverty (Destatis, 2022b), and increasing take-up helps them as it reduces their financial burden (Riedmiller, 2025). Since German universities do not charge tuition, student aid directly affects income and is used to cover the students' cost of living. Additionally, take-up can improve academic performance as it leads to higher persistence and graduation rates (Bettinger et al., 2019; Castleman & Page, 2016; Denning, 2019; Fack & Grenet, 2015; Glocker, 2011; Murphy & Wyness, 2023; Nguyen, Kramer & Evans, 2019), and decreases the need for paid labor while studying (Denning, 2019; Herber & Kalinowski, 2019; Kofoed, 2022; Park & Scott-Clayton, 2018). The German context is ideal to quantify the eligibility and application barriers and their relation to other non-take-up determinants as it allows us to look at a nationwide program with low take-up and room for misperceptions about both the eligibility conditions and repayment amounts.

3.3 Data and Sample Preparation

3.3.1 Data Collection

We use an incentivized online survey distributed among enrolled students at all 83 public universities in Germany. The survey was conducted in two waves six months apart.[3] In the first wave, collected from May 2 to May 31, 2023, 22,222 students participated, of which 17,636 gave consent to be contacted again. In the second wave, collected from November 2 to December 15, 2023, 12,096 participated again. In both waves, students could participate in a lottery with 100 prizes of €25 in the first wave and 200 prizes of €50 in the second wave. Median participation took approximately 15 minutes in the first and 12 minutes in the second wave.

In both survey waves, students were asked about their income by entering how much money they receive from different sources, as depicted in Figure B.1. One of the sources was student aid. A positive income from student aid is classified as take-up. In the first wave, participants were additionally asked about their parents' monthly net income thresholds in increments of €500. This question was asked for both parents separately. Additionally, students had to indicate their confidence in these income levels using a slider

[3] The data collection was split into two waves due to the evaluation of a randomly assigned information intervention among the non-receivers. The experiment is described and analyzed in Chapter 4.

from 0-100%, as shown in Figure B.2. We also asked for the students' age, pursued degree, semester, marital status, number of siblings, housing situation, previous secondary training, and their parents' marital status. This information is part of the student aid's means-test.

Furthermore, we elicited reasons for non-take-up among the non-receivers by asking students to indicate which reasons apply to them on a 5-point Likert scale. Some reasons indicate perceived ineligibility, such as "I have realized myself that my parents' income is too high". Others point towards an active decision against take-up, such as "I do not want to take on any debt". Figure B.3 shows the complete list of reasons as elicited in the survey. Which reasons belong to which category is presented in Table 3.4 and discussed further as part of the results in Section 3.4.4.

We elicited potential misperceptions about student aid conditions in two ways. First, we asked non-receivers if they think they would receive student aid if they applied. Answers were given on a 5-point Likert scale ranging from "Definitely no" to "Definitely yes". This allows us to understand if students knowingly decide against receiving student aid in case they are eligible or if they misperceive their individual eligibility. Additionally, we use three hypothetical case scenarios of student aid receivers to elicit how well participants assess (i) the amount of student aid one can receive per month, (ii) the possible income thresholds for parents for a given entitlement, and (iii) the repayment amount. In total, we asked 8 questions to elicit potential misperceptions. Each question had an underlying true value. Participants were incentivized to give a correct answer with increased chances to win one of the prizes. An answer was counted as correct if it was within a certain interval around the underlying true value. For each question, participants indicated their confidence in their answers on a slider from 0-100%, where 100% means that they are certain their answer is correct. The scenarios and questions are presented in Appendix B.1.1. A detailed description of the misperception elicitation is provided in Chapter 4.

In addition, we ask students if someone in their close circle receives student aid in the first wave. In the second wave, we also elicited the students' debt aversion, impulsiveness, and patience as potential determinants of non-take-up. For this, we asked students to indicate on a 10-point Likert scale how they see themselves: as a person who is willing to take on debt or tries to avoid debt, as an example for debt aversion. All three elicitation questions are shown in Appendix B.1.2. Furthermore, we asked students about their migration background and if one or both of their parents have a higher educational degree. We also asked all non-receivers at which minimum entitlement they would take up student aid.

3.3.2 Non-Take-Up Simulation

Non-take-up is defined as not applying for student aid even though one would receive a positive amount. Since the student aid entitlement is not observable without an application, we need to estimate each student's potential aid amount based on their sociodemographic and economic situation. We use a microsimulation model that rebuilds the means-test performed within the student aid application process. For this, we first identify students who fulfill the institutional eligibility requirements. Second, we only include students who have never applied for student aid in the past, as this is the relevant target group for measuring non-take-up. Lastly, only observations with complete information to perform the means-test are included, i.e., students need to report their parents' income. For the resulting sample, we simulate the aid amount the students would receive in case of an application.

To test the validity of the microsimulation model, we use the students who receive student aid to compare the simulated aid amounts from the model to the actual aid amounts they receive. That is, we calculate the beta error rate (BER), which is the number of student aid receivers simulated as ineligible divided by the total student aid receivers (Frick & Groh-Samberg, 2007; Harnisch, 2019; Herber & Kalinowski, 2019), as shown in formula 3.1.

$$\text{Beta Error Rate (BER)} = \frac{\text{Take-Up} = 1 \cap \text{Eligibility} = 0}{\text{Take-Up} = 1} \tag{3.1}$$

The BER shows us how well the microsimulation fits the actual take-up data. In case of a high BER, students likely misreported their take-up or their parents' income. The former should not be a problem as the survey is completely anonymous and participants have no incentive to report a positive amount of student aid if they do not receive any. However, some students might receive student aid even though they initially seem ineligible. Exceptional cases such as having a disabled sibling increase potential eligibility but were not part of the survey. Misreporting parents' income might cause a high BER if students do not know how much their parents earn. Therefore, students in our survey were asked for monthly income thresholds in €500 increments to avoid point estimates. Additionally, we asked for the net income as it is more tangible for students than gross income (Anderson & Holt, 2017), and students had to indicate their confidence in their parents' income reports.

3.3.3 Sample Selection and Simulation

To set up the sample, we apply the steps outlined above. Starting from 22,222 students who participated in the online survey, we first dropped all institutionally ineligible students. This includes internationals, long-term students, students who exceed the age restriction for federal student aid, students enrolled in a study course or degree invalid for receiving student aid, and students who receive other social benefits that are mutually exclusive to student aid.[4] Then, we dropped all non-receivers who had already applied for student aid at some point and who had incomplete information to perform the student aid calculation. This reduced the number of observations to 16,023. Additionally, we excluded 119 participants who gave invalid answers to the misperception elicitation questions.[5] The sample therefore consists of 15,904 students who participated in the first wave of data collection. Based on this sample, we apply the eligibility simulation. In total, 11,688 students either

[4] If students receive social benefits such as federal rent support, they first have to show that they are institutionally ineligible for student aid.

[5] Participants who indicate a student aid amount over €10,000 per month in all three questions in the first scenario, income thresholds for parents over €300,000 for both questions in the second scenario, or repayment amounts over €100,000 for all three questions in the third scenario.

already receive student aid or are eligible for it. To include migration background and parents' education, we restrict the sample to the 6,665 receivers or eligible students who participated in the second wave of data collection. A detailed flowchart of our sample construction is presented in Figure B.4.

Some variables that are part of the means-test but not of the survey were imputed or selected systematically where necessary. This includes the students' wealth, whether they are covered by their parents' health insurance, and their siblings' educational stage. The insurance is imputed via the students' age, as adults older than 25 are no longer eligible to be covered by parental health insurance in Germany. The other imputed values were chosen to calculate the lower bound of the simulated student aid entitlement.

To test our estimation, we calculate the beta error. In our restricted sample, we find that 4,256 receive student aid, of which we calculate 317 to have no student aid entitlement. That is, we have a BER of 7.4%, which is similar to or better than earlier findings from the literature (Frick & Groh-Samberg, 2007; Harnisch, 2019; Herber & Kalinowski, 2019). In the full sample, we have 7,101 student aid receivers of which 516 have a calculated entitlement of 0, which amounts to a BER of 7.3%. Based on this estimation, we calculate a representative non-take-up rate in the next section.

3.4 Results

3.4.1 Non-Take-Up Rate and Barriers to Take-Up

Among the 6,665 eligible students in the restricted sample, 2,409 did not take up student aid. That is, we find a non-take-up rate of 36.1%, which is in line with earlier findings by Herber & Kalinowski (2019). In the full sample, the non-take-up rate is 39.3%, with 11,688 eligible students and 4,587 non-receivers. Even though non-receivers and receivers are similar to a representative sample when compared separately, as shown in Tables B.4 and B.5, receivers are overrepresented in our sample. That is, this non-take-up rate is lower than the actual representative rate.

To estimate a more precise non-take-up rate, we reweight our sample to represent the general student body. We use the algorithm by Merz (1985) to adjust our data based on gender, studying in East Germany, age, pursued degree, migration background, educational background of parents, and the fraction of receivers and eligible students. A detailed description is provided in Appendix B.1.3. Using these weights, we find a non-take-up rate of 70.4% with the current eligibility calculation and regulations. Even for the most conservative case for calculating eligibility, we find a lower bound of the representative non-take-up rate of 56%. That is, more than every second student who is eligible for student aid does not take up their entitlement. All non-take-up rates using the restricted sample and the full sample are presented in Table B.1.

Using the simulated eligibility and the elicited perceived eligibility, we can identify two subgroups among the eligible non-receivers: the students who do not think they are eligible, the *non-believers*, and the students who believe they are eligible, the *believers*. These two groups comprise the two barriers to take-up. As non-believers do not take up student aid because

TABLE 3.1: Summary Statistics by Take-Up and Eligibility Beliefs

	Non-Eligible	Eligible Non-Receivers			Receivers
	All (N=2,263)	All (N=2,409)	Non-Believers (N=1,979)	Believers (N=318)	All (N=4,256)
Sociodem. Background (=1)					
Age (in years)	23.07	24.65	24.61	24.09	24.52
Female	0.59	0.64	0.66	0.59	0.66
Studies in East Germany	0.17	0.17	0.16	0.19	0.28
Knows student aid receiver	0.48	0.42	0.43	0.47	0.66
Born outside Germany	0.03	0.14	0.09	0.30	0.06
Parents born outside Germany	0.14	0.25	0.19	0.47	0.29
First-gen student	0.21	0.37	0.35	0.48	0.63
Economic Background (in €)					
Student aid entitlement	0.00	491.83	462.09	599.75	650.88
Income from work	331.87	431.12	443.10	368.28	241.12
Support from parents	580.12	405.91	431.19	310.40	125.63
Total income	1,047.90	1,020.03	1,051.71	901.03	1,143.12
Average Misperception (in €)					
Student aid amount	-253.90	-265.54	-281.20	-187.94	-247.35
Income thresh. for parents	-14,156.73	-16,933.69	-16,527.94	-16,006.04	-11,299.56
Repayment amount	2,981.97	2,922.45	2,897.84	2,795.47	1,293.98

Notes: The table shows the summary statistics for the restricted sample, as explained in Section 3.3.3. Column 1 shows students who are not eligible for student based on our calculated entitlement. Column 2 shows students who do not take up student aid but are eligible for it. Columns 3 and 4 further separate this group into students who do not believe they are eligible (Non-Believers) and who believe they are eligible (Believers). Summary statistics for student aid receivers are presented in column 5. The entitlements are calculated by our microsimulation for columns 1-4. The actual student aid amount is used for column 5. Based on the group sizes, we find that 36.1% of eligible students do not take up student aid. Additionally, 82.2% of the non-take-up group do not believe they are eligible for student aid.

they do not know about their eligibility, they are at the eligibility barrier. Believers, on the other hand, already suspect that they are eligible but still do not apply for student aid. Thus, they are at the application barrier. Summary statistics for these groups, the ineligible non-receivers and the receivers are presented in Table 3.1. Summary statistics for the full sample are presented in Table B.2.[6]

At the eligibility barrier, 82.2% of the eligible non-receivers do not believe they are eligible. At the application barrier, 13.2% of the eligible non-receivers believe to be eligible.[7] On average, the non-believers forgo €462 of student aid per month, while believers forgo €600, as displayed in columns 3 and 4 of Table 3.1. The two groups also differ in their economic background, as shown by their work income and the financial support from their parents. Non-believers earn €443 per month and receive €431 from their parents. Believers earn only €368 and receive €310 from their parents. In total, non-believers have €1,052 available per month, and believers have €901. Regarding their sociodemographic background, we see that believers are more

[6]The differences between the two samples based on two-sided t-tests are presented in Table B.3.

[7]The remaining 4.6% answer the question on perceived eligibility with "Cannot make a clear statement" and were not categorized in one of the two groups.

likely to be male, study in East Germany, have a migration background, and be first-gen students.

As shown in the last column of Table 3.1, student aid receivers get, on average, €651 aid and €126 from their parents per month. In total, they have €1,143 available per month. Compared to the believers, they have a similar student aid entitlement, yet for receivers, this entitlement is realized income. This explains that while receivers have less financial support from their parents, they have a higher total income compared to eligible non-receivers. With respect to the sociodemographic background, we see that receivers are more likely to study in East Germany, know another student aid receiver, and be first-gen students compared to non-receivers. For migration background, we see that receivers are more likely to have parents not born in Germany (second-generation immigrants) but are less likely to be born outside of Germany themselves (first-generation immigrants) than non-receivers.

Misperceptions of all groups are shown in the last three rows of Table 3.1. We find significant misperceptions regarding all three domains. On average, non-receivers underestimate the amount of student aid from the hypothetical scenarios by €266 and the income thresholds for parents by €16,934. Additionally, they overestimate the repayment amounts by €2,922. Believers and non-believers differ in these misperceptions, however. Receivers, on the other hand, underestimate the monthly aid amount by €247, underestimate the income thresholds for parents by €11,300, and overestimate the repayment amounts by €1,294. In the following section, we analyze the determinants of both the eligibility and the application barrier to take-up with a closer look at the role of misperceptions.

3.4.2 Determinants of Known Eligibility and Take-Up

To systematically analyze how students at the eligibility and application barrier to take-up differ, we use a probit estimation where we compare believers, non-believers, and receivers separately. We first determine the differences between believers and non-believers to get more insights into the application barrier, so who knows to be eligible but still does not claim aid. Then, we estimate what determines the take-up of student aid. Since there might be differences in the eligibility and application barrier, we estimate three specifications: (i) the comparison of receivers to all eligible non-receivers, (ii) the comparison of receivers to eligible non-believers, and (iii) the comparison of receivers to eligible believers. The first specification tells us what drives non-take-up overall. The second specification allows us to look at the eligibility barrier since our non-take-up group are non-believers. The third specification generates insights into the application barrier to take-up as we look at believers as comparison group. The marginal effects from the probit estimation are presented in Table 3.2.

With respect to the student's sociodemographic background, we find that students who study in East Germany and know a student aid receiver are more likely to take up student aid in all three specifications from columns 2 to 4. Additionally, knowing about one's own eligibility is more likely if the student knows other student aid receivers, as shown in column 1. This

TABLE 3.2: Determinants of Take-Up and Belief to be Eligible

	Believer (=1)	Take-Up (=1)		
Comparison Group:	Non-Believers (1)	Non-Receivers (2)	Non-Believers (3)	Believers (4)
Sociodemographic Background				
Age (standardized)	-0.054***	-0.070***	-0.072***	0.005
	(0.008)	(0.009)	(0.009)	(0.003)
Female (=1)	-0.026*	0.001	-0.010	0.010*
	(0.015)	(0.016)	(0.015)	(0.006)
Studies in East Germany (=1)	0.018	0.106***	0.099***	0.012**
	(0.018)	(0.016)	(0.015)	(0.005)
Knows student aid receiver (=1)	0.049***	0.174***	0.171***	0.019***
	(0.014)	(0.014)	(0.014)	(0.006)
First generation student (=1)	0.027*	0.102***	0.092***	0.019***
	(0.015)	(0.017)	(0.016)	(0.006)
Born outside Germany (=1)	0.078**	-0.274***	-0.184***	-0.122***
	(0.033)	(0.032)	(0.037)	(0.025)
Parents born outside Germany (=1)	0.051**	0.101***	0.105***	0.005
	(0.021)	(0.019)	(0.017)	(0.006)
Economic Background (in €100)				
Student aid entitlement	0.017***	0.019***	0.020***	-0.002**
	(0.003)	(0.003)	(0.003)	(0.001)
Support from parents	-0.004**	-0.078***	-0.071***	-0.012***
	(0.002)	(0.004)	(0.003)	(0.001)
Misperceptions (in %)				
Absolute underestimation	-0.083***	-0.093***	-0.125***	0.020*
student aid amount	(0.027)	(0.030)	(0.029)	(0.010)
Absolute underestimation	-0.043	-0.132***	-0.130***	-0.008
income thresholds for parents	(0.031)	(0.037)	(0.035)	(0.012)
Absolute overestimation	-0.008	-0.111***	-0.096***	-0.018***
repayment amount	(0.011)	(0.019)	(0.019)	(0.006)
Observations	2,409	6,665	6,235	4,574
Pseudo R^2	0.162	0.391	0.414	0.297

Notes: The table shows the marginal effects from probit regressions on the dependent variables students' own belief about their aid eligibility (column 1) and take-up (columns 2-4). *Believer* equals 1 for students who believe they are eligible. *Take-Up* equals 1 for all students who receive student aid. The comparison group for each regression is shown below the dependent variable. In column 1, we compare all eligible believers with all eligible non-believers. In columns 2-4, we compare all receivers to eligible non-receivers. We differentiate between all eligible non-receivers (column 2), eligible students who believe they are eligible (column 3), and eligible students who believe they are ineligible (column 4). Marginal effects >0 relate to belief/take-up, and marginal effects <0 relate to non-belief/non-take-up, respectively. We include study field and university fixed effects, and dummies for leaving the online survey during the misperception elicitation, indicating parents' income thresholds ≤€10,000 in both respective misperception elicitation questions, and no confidence level for parents' income. The full set of coefficients is presented in Table B.9. Robust standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

suggests that the network matters, in line with Bertrand, Luttmer & Mullainathan (2000). Once students are confronted with student aid through other people in their network, they are more likely to realize that they are eligible and also more likely to claim student aid. Moreover, we find that increasing age is related to a lower likelihood of realizing one's eligibility. This translates into a lower likelihood of take-up for non-believers but not for believers, suggesting that older students who realize their eligibility also claim aid. Taken together, the sociodemographic background mostly influences the eligibility barrier to take-up as the marginal effects reveal that once the groups realize their eligibility, they also take up student aid.

The picture for parental background is more nuanced. First, we find that first-gen students are 2.7 pp (20.4%) more likely to realize their eligibility, as shown in column 1. Additionally, they are 10.2 pp (15.9%) more likely to take up aid overall, as shown in column 2. The positive marginal effects for both non-believers and believers in columns 3 and 4 suggest that once first-gen students realize they are eligible, they are also applying for student aid. This is in line with the influence on the eligibility barrier described above.

For migration background, we find a counteracting relation. While both first-generation and second-generation immigrants are more likely to realize they are eligible, the take-up relation is different. First-generation immigrants are 17.3 pp (27.0%)[8] less likely to take up student aid, which is significant in all three specifications. This suggests that they do not claim their entitlements even though they realize they are eligible. Second-generation immigrants, on the other hand, are 10.1 pp (15.8%) more likely to take up student aid. Yet, this association is only significant for non-believers. This suggests that while they realize they are eligible, some still do not claim student aid. Otherwise, we should find a significantly positive marginal effect in favor of take-up for the believers in column 3. This reveals that migration background influences both the eligibility barrier and the application barrier to take-up, as students are not claiming aid even after they realize they are eligible.

Regarding their economic background, we find that students with a higher entitlement based on our estimation and lower financial support from their parents are more likely to take up aid. A €100 higher entitlement is associated with a 1.9 pp higher likelihood to claim student aid. Receiving €100 less from your parents per month is associated with a 7.8 pp higher probability of take-up. Additionally, these students are also more likely to realize they are eligible. We find similar results for the full sample, presented in Table B.6. The effects suggest that students in more need of financial support are more likely to realize their eligibility for student aid and more likely to claim aid, affecting the eligibility barrier to take-up.

Looking at misperceptions elicited by the hypothetical scenarios, we find that higher misperceptions are associated with a significantly lower likelihood of take-up, as shown in column 2. Yet, only for the overestimation of repayment amounts, we find a significant non-take-up effect for both believers and non-believers. That is, students with higher misperceptions about the repayment are less likely to receive student aid, irrespective of knowing about their eligibility. This suggests that overestimating the repayment amount affects both the eligibility and application barrier to take-up, since even students who know about their eligibility are less likely to claim aid when they have higher misperceptions. For the eligibility conditions (student aid amount and income thresholds for parents), higher misperceptions

[8] The marginal effect for first-generation immigrants is the sum of the second and first-generation effect since all immigrated students who were not born in Germany also have parents who were not born in Germany.

are associated with non-take-up only for non-believers. For believers, higher misperceptions about the student aid amount even relate to a higher likelihood of take-up. This suggests that once students learn about their eligibility, misperceptions about the eligibility conditions do not matter for take-up. Thus, these misperceptions seem only to impact the eligibility barrier to take-up. This is corroborated by the finding that underestimating the student aid amount significantly decreases the likelihood of believing to be eligible, as shown in column 1. Taken together, the results suggest that while misperceptions about the repayment amounts seem to affect both the eligibility and application barrier to take-up, misperceptions about the eligibility conditions only influence the eligibility barrier. To understand the role of misperceptions further, we analyze how the misperceptions differ between the three groups in the following section.

3.4.3 Misperceptions about Student Aid and Take-Up

As misperceptions drive both the eligibility and application barrier to take-up, the question arises of how the misperceptions differ between the three groups: receivers, eligible believers, and eligible non-believers. That is, do students at the different barriers to take-up have varying misperceptions about student aid? To answer this question, we compare the three groups using an OLS estimation with absolute misperceptions in % as dependent variables and group indicators as independent variables. We use eligible believers as reference group as they link the eligibility and application barrier to take-up. Results for the estimation controlling for sociodemographic, parental, and economic background are presented in Table 3.3.

In line with the determinants of take-up in Table 3.2, we find significant differences between the misperceptions of receivers, eligible believers, and eligible non-believers. At the eligibility barrier to take-up, hence the comparison of believers and non-believers, we find that non-believers overestimate the eligibility conditions of student aid by 5.3 pp (9.9%) more, as shown in column 1. Disentangling these misperceptions reveals that this is mostly driven by the amount of student aid. Non-believers overestimate the amount of student aid by 9.0 pp (15.5%) more than believers, as shown in column 2. Yet, column 4 shows that these groups do not differ in their level of overestimating the repayment amounts.

At the application barrier, we find the opposite effects. Non-receivers who know they are eligible do not differ from receivers in their misperceptions about the eligibility conditions of student aid. Yet, receivers have 13.2 pp (21.0%) lower misperceptions about the repayment amounts. We find similar results for the full sample, presented in Table B.7. Corroborating the findings from Table 3.2, this suggests that tackling different misperceptions is necessary to address the eligibility and application barrier to take-up. First, students who do not know about their eligibility have significantly larger misperceptions about the eligibility conditions of student aid. That is, they overestimate how much student aid is paid out and how much parents can earn at a given entitlement. Reducing these misperceptions could help students to realize they are eligible and address the eligibility barrier to take-up. Second, eligible non-receivers who know they are eligible are similarly

TABLE 3.3: Misperceptions at the Eligibility and Application Barrier to Take-Up

	Misperceptions of Student Aid Terms (in %)			
	Eligibility Conditions	Student Aid Amount	Income Thresh. for Parents	Repayment Amount
	(1)	(2)	(3)	(4)
Eligible Non-Believers (=1)	0.053***	0.090***	0.023*	-0.004
	(0.013)	(0.016)	(0.013)	(0.044)
Receivers (=1)	0.010	0.027*	-0.009	-0.132***
	(0.013)	(0.016)	(0.013)	(0.040)
Mean Misperception Elig. Believers	0.536	0.582	0.434	0.629
Observations	6,665	6,665	6,665	6,665
R^2	0.230	0.111	0.336	0.120
F Statistic	21.11***	10.54***	170.50***	14.28***

Notes: The table presents OLS regression results on misperceptions, measured as the percentage deviation from the correct answer in the hypothetical scenarios. Column 1 pools misperceptions about eligibility conditions from the first two scenarios, while columns 2 and 3 separate them into misperceptions about student aid amounts (first scenario) and income thresholds for parents (second scenario). Column 4 covers misperceptions about repayment amounts (third scenario). Coefficients represent eligible non-receivers (=1) and student aid receivers (=1) who underestimate eligibility conditions or overestimate repayment. The reference group is the eligible believers, whose average underestimation (overestimation in column 4) is shown in the first row below the coefficients. Positive coefficients indicate a higher underestimation of student aid terms in percentage points. All coefficients are shown in Table B.10. We include study field and university fixed effects, and dummies for leaving the online survey during the misperception elicitation, indicating parents' income thresholds ≤€10,000 in both respective misperception elicitation questions, and no confidence level for parents' income. Robust standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

knowledgeable about the eligibility conditions as receivers but overestimate the repayment amounts significantly more. That is, they overestimate the potential debt from receiving student aid and potentially do not claim their entitlement for this reason. Reducing these repayment misperceptions could help students who know about their eligibility to realize that their prospective debt is lower than expected. This might encourage them to claim their entitlement, addressing the application barrier to take-up.

3.4.4 Reasons for Non-Take-Up

Since eligible believers and non-believers not only differ in knowing about their eligibility, it is likely that they have different reasons for non-take-up. For example, students at the eligibility barrier might indicate that they did not apply because of too high parental income, underlining their unawareness of their eligibility. At the application barrier, students could indicate that they did not apply because they do not want to incur debt, which could point towards an informed decision against take-up but also towards potential misperceptions about the repayment amount.

In Table 3.4, we show the fraction of stated reasons for non-take-up split between eligible believers and non-believers. The last column shows the p-values of two-sided t-tests comparing the means of each stated reason.

We find that especially non-believers state reasons indicating unawareness of one's own eligibility. While 59% of the non-believers state that their

TABLE 3.4: Reasons for Non-Take-Up by Eligibility Belief

Reason (=1)	Non-Believers (N=1,979) Mean	SD	Believers (N=318) Mean	SD	Diff. t-test p-val.
Unawareness of Eligibility					
I realized parents' income is too high	0.589	0.492	0.110	0.313	0.000
Parents said their income is too high	0.576	0.494	0.189	0.392	0.000
Spouse's income too high	0.039	0.193	0.044	0.205	0.671
Own income too high	0.258	0.437	0.189	0.392	0.008
Own assets too high	0.506	0.500	0.255	0.436	0.000
Active Decision against Take-Up					
Do not want to be seen as a student aid receiver	0.028	0.166	0.101	0.301	0.000
Do not want to get money from the state	0.079	0.270	0.167	0.373	0.000
Do not want to incur debt	0.446	0.497	0.619	0.486	0.000
Application effort too high	0.432	0.495	0.635	0.482	0.000
Family situation too complex	0.141	0.348	0.255	0.436	0.000
Expected funding amount too low	0.264	0.441	0.355	0.479	0.001
Other					
Sufficient support from parents	0.644	0.479	0.447	0.498	0.000
Do not want to disclose income information	0.109	0.312	0.164	0.370	0.005
Cannot provide certificate of performance	0.107	0.309	0.164	0.370	0.003

Notes: The table shows the fraction of how many students indicated which reasons for not applying for student aid. The reasons are measured on a 5-point Likert scale. The fractions show how many students stated that a specific reason "applies" or "rather applies" to them. The fractions are separately reported for eligible students who do not believe they are eligible and those who do. The last column shows p-values from two-sided t-tests between the means per group, respectively.

parents' income is too high, only 11% of the believers do.[9] Overall, significantly more non-believers state a reason related to unawareness of eligibility in four out of five cases. Note that the only insignificant comparison is "spouse's income is too high", which only applies to married students. Therefore, it is inapplicable to most students since only 4.4% of the sample is married.

For actively deciding against student aid, the distribution flips. Significantly more believers indicate reasons pointing towards an active decision against student aid in all six comparisons. While the literature states stigma or unwillingness to receive social aid as potential reasons for non-take-up (Currie, 2006; Eurofound, 2015), we see that this is not a major concern for our context. Even though significantly more believers indicate these reasons, the largest fraction is only 17%. The most common reasons are the application effort (43.2% vs. 63.5%) and debt aversion (44.6% vs. 61.9%). It is possible, however, that some students are more willing to exert effort and incur debt by claiming aid if they knew their entitlement is larger than they think. As 26% of non-believers and 36% of believers indicate that their expected funding amount is too low to be worth the application, correcting potential

[9]The 11% who believe to be eligible but state that their parents earn too much for eligibility likely know they are eligible for student aid unconditional on parents' income, e.g., due to previous relevant job trainings. For the receivers in our sample, 16% receive such unconditional student aid, which is within the same range.

misperceptions about the entitlement tackles both the eligibility and application barrier to take-up.

To this end, we asked students about the minimum amount of student aid they would need to apply. Interestingly, 68.0% of the believers and 65.3% of non-believers (65.8% in total) have a simulated entitlement above this number. That is, the large majority of the eligible non-receivers misperceive their own entitlement to the extent that they indicate they would apply if they knew about its amount. This underlines the role of misperceptions in both the eligibility and application barrier to take-up of student aid.

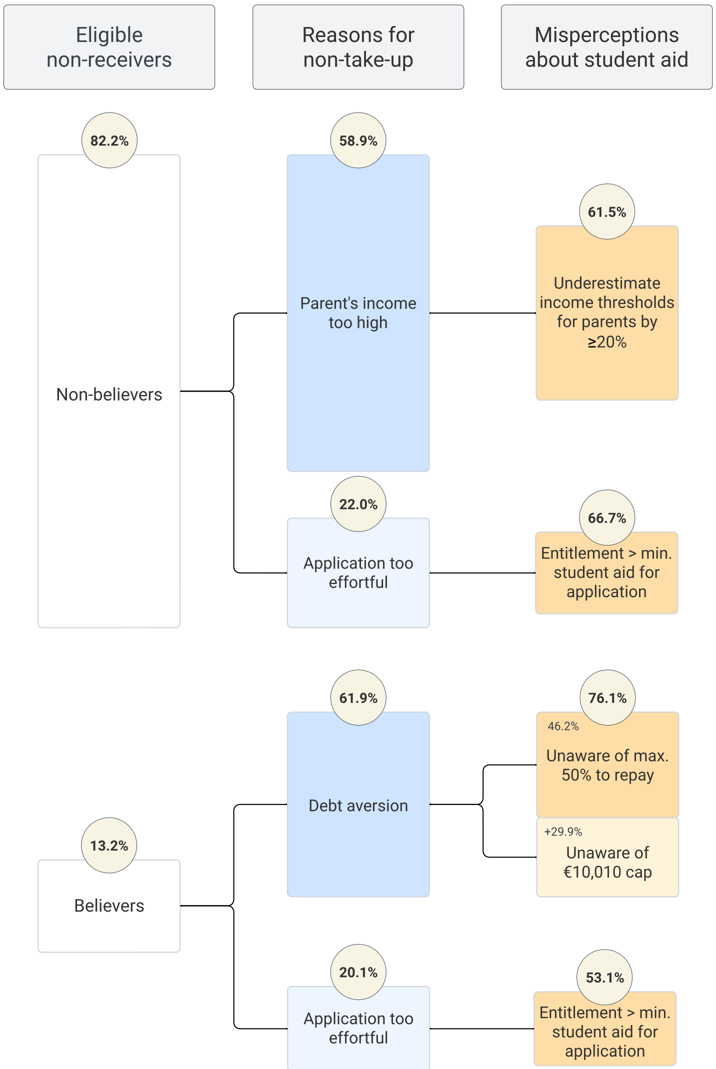
In addition to unawareness about eligibility and actively deciding against student aid, we find that significantly more non-believers state that they already receive enough financial support from their parents. This corroborates earlier findings that students with more need for financial support are more likely to realize their eligibility. Additionally, more believers stated that they could not provide the performance certificate or did not want to disclose financial information to the student aid administration. This suggests that believers are more aware of the necessary documents for the claiming process of student aid. Thus, they are also more likely to realize the necessity for a performance certificate or experience privacy concerns than non-believers, which could cause them to not apply for these administrative reasons. We find the same patterns for the full sample, presented in Table B.8.

Taken together, students on the eligibility and application barrier to take-up have different reasons for non-take-up, closely aligned with their misperceptions. At the eligibility barrier, non-believers mostly show unawareness about their eligibility, which is in line with their misperceptions about the eligibility conditions of student aid. At the application barrier, believers are more likely to state other reasons related to the application or the design of student aid, such as complexity and debt. Interestingly, a prominent reason among both groups is that the expected student aid amount is too low to be worth the application effort. Yet, around two-thirds of students in both groups stated that they would apply for a minimum aid amount below their simulated entitlement. This underlines that misperceptions about the student aid characteristics and the entitlement inhibit take-up. Reducing these misperceptions could therefore address both the eligibility and application barriers to take-up.

3.4.5 How Misperceptions Drive the Barriers to Take-Up

Until now, we have seen that many students do not take up student aid despite their eligibility. The majority of them face a problem at the eligibility barrier as they are unaware of their eligibility. Yet, a non-negligible fraction of non-take-up happens at the application barrier as students do not claim their entitlement despite knowing about their eligibility. At the eligibility barrier, students indicate reasons for non-take-up related to perceived ineligibility. Simultaneously, especially misperceptions about the eligibility conditions of student aid are a driver of non-take-up. At the application barrier, students state that debt accumulation is a main reason for non-take-up. Simultaneously, especially misperceptions about the repayment drive non-take-up. Using the data on an individual level, we can quantify how many

FIGURE 3.1: Reasons for Non-Take-Up and Underlying Misperceptions



Notes: The figure shows the eligibility and application barriers, what reasons student state for non-take-up, and how much of this is driven by specific misperceptions from left to right. The relative frequency is displayed above each group's box.

students have a specific reasoning coinciding with misperceptions. The connection of the groups are presented in Figure 3.1.

On the upper left side, we can see the eligibility barrier to take-up with 82.2% of eligible non-receivers who do not think they are eligible. With 58.9%, the majority of them state that their parents' income is too high for eligibility. Yet, among them, 61.5% underestimate the income thresholds for parents by at least 20%. This shows that many students underestimate the eligibility thresholds and therefore falsely conclude they are not eligible and do not take up student aid. Among the students who do not state that their parents earn too much, the most prominent reason for non-take-up is the necessary effort of the application. Yet, 66.7% of them state a minimum student aid amount necessary for them to apply that is lower than their simulated entitlement. That is, if these students did not misperceive their entitlement, they would potentially make the effort and take up student aid. Overall, misperceptions explain a large proportion of the eligibility barrier to take-up.

On the lower left side of Figure 3.1, we can see the application barrier to take-up with 13.2% of eligible non-receivers who know they are eligible but still do not claim aid. With 61.9%, the majority of them state that debt accumulation is their main reason for non-take-up. Yet, 46.2% among them do not know that only half of the student aid amount needs to be repaid. Another 29.9% know about this repayment rule but do not know that the maximum debt is capped at €10,010. That is, 76.1% of the students who indicate debt accumulation as a reason for non-take-up overestimate the potential debt from receiving student aid. Among those who do not list debt accumulation as a reason for non-take-up, the application's effort is a major concern. Yet, 53.1% among them state a minimum student aid amount necessary for them to apply that is lower than their simulated entitlement. Similar to the non-believers, these students would potentially take up student aid if they knew about their entitlement. Overall, misperceptions explain a large proportion of the application barrier to take-up, as well.

Students with a required minimum amount below their entitlement state that they would apply at €242 per month, on average. Their average entitlement is €590 per month. That is, their prospective student aid amount is nearly 2.5 times larger than what they claim as their minimum amount for application. Additionally, many of these students state that the application effort is a reason for non-take-up. Given a minimum required amount of student aid of €242 and taking into account that student aid is paid out for 12 months, their yearly requirement is €2,904. The students expect to take 8:56 working hours for the application, which is remarkably close to the actual application time of receivers of 9:07 working hours, on average. That is, students seemingly have a good estimate of their expected effort for the application. Yet, using the expected working time and the yearly requirement, a back-of-the-envelope calculation shows that the reservation wage for the students to apply at their minimum required level is €325 per hour. This reservation wage can be interpreted as an upper bound as we did not incorporate any additional disutility from applying and waiting for the decision, discount factors on the monthly payments, or increased application time in case the non-receivers have more complex applications. Yet, using the students' simulated entitlement, this reservation wage increases to €792 per hour. Given that a regular student job paid approximately 15€ per hour in 2023 (Seegers et al., 2024), these reservation wages underline that misperceptions play a crucial role for non-take-up, and that resolving misperceptions could increase take-up substantially.

3.5 Conclusion

While student aid aims to tackle inequality in higher education, take-up falls short of eligibility rates (e.g. Bettinger et al., 2012; Dynarski et al., 2021; Herber & Kalinowski, 2019). It is unclear, however, if students have a problem at the eligibility barrier as they do not know about their eligibility, or at the application barrier if they know about their eligibility but still do not apply. To effectively tackle non-take-up, it is necessary to identify the extent of both barriers and understand the reasons for non-take-up. We quantify these gaps and find that student aid non-take-up is mostly an eligibility barrier. Yet, for both the eligibility and application barrier, the reasons students state for their non-take-up largely coincide with misperceptions about the student aid conditions. Resolving these misperceptions could tackle both barriers simultaneously and increase take-up.

In an online survey with 22,222 enrolled university students in Germany, we use a microsimulation model to identify the group of students who are eligible for student aid based on their socioeconomic situation but do not take up their entitlement. Reweighting our sample to represent the general student body, we find that 70% of all eligible students do not take up student aid. At the eligibility barrier, 82.2% do not believe they are eligible. While 59% state that they did not take up student aid because their parents' income is too high, the majority simultaneously underestimates the income thresholds for parents by at least 20%. For the 13.2% at the application barrier who know they are eligible, the main reason for non-take-up is debt aversion. Yet, 76% simultaneously overestimate the repayment amount of student aid. In addition, 66% of eligible non-receivers report a minimum amount of student aid needed to take up that is less than their entitlement. Based on their entitlement and their estimated time for applying, they would demand a reservation wage of €792 per hour for applying. That is, they would potentially take up student aid if they knew their entitlement.

Our results show that misperceptions are not only an important determinant of student aid non-take-up, but that they are important for both barriers of non-take-up and the reasoning behind these barriers. Eliciting perceived eligibility and perceptions about the terms of student aid, we can see that most students have misperceptions about their eligibility and their respective entitlement, largely explained by misperceptions about the eligibility conditions. In case students correctly perceive their eligibility, most of them still have misperceptions about the repayment conditions of student aid. Addressing these misperceptions could tackle both barriers simultaneously. As take-up is also determined by knowing a receiver, a potential intervention to increase take-up could be to increase the transparency of the program overall by making the application accessible and promoting who is eligible for what entitlement. More transparency of student aid programs could help to bring more people to their entitlement, which tackles social inequality by reducing the students' and their families' financial burdens, and improving their academic performance.

Chapter 4

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

4.1 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 is the cost of higher education, which is 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; Bird et al., 2021; Castleman & Page, 2016; Kofoed, 2017). Previous work shows that information provision about possible aid amounts or the application is often insufficient to increase take-up (Bettinger et al., 2012; Bird et al., 2021; Booij, Leuven & Oosterbeek, 2012; Marx & Turner, 2020), while assisting students with their application has been found to be more effective (Bettinger et al., 2012; Castleman & Page, 2016; Dynarski et al., 2021; Hoxby & Turner, 2015). 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

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

similar to the Free Application for Federal Student Aid (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 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 their perceptions are systematically wrong. 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 the 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 (Athey, Tibshirani & Wager, 2019; Athey & Wager, 2019; Wager & Athey, 2018). 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; Bird et al., 2021; Booij, Leuven & Oosterbeek, 2012; Marx & Turner, 2020; Peter, Spiess & Zambre, 2021; Peter & Zambre, 2017). 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; Castleman & Page, 2016; Dynarski et al., 2021; Dynarski, Page & Scott-Clayton, 2023; Hoxby & Turner, 2015). 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; Goldin et al., 2022; Ihlanfeldt, 2021), unemployment aid (Castell et al., 2025; Chareyron, Gray & L'Horty, 2018), 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 (Kacker et al., 2022; Nguyen, Le & Connelly, 2020), 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 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 4.2 explains the context of student aid in Germany. In section 4.3, I explain the experimental design and data collection. The intervention effects on misperceptions and take-up are described in section 4.4. Section 4.5 concludes the paper.

4.2 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 on 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

[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.

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 (Avdic & Gartell, 2015; Triventi, 2014) 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 amount of paid work while studying, and lower earnings after graduation (persistence: Bettinger et al., 2019; Castleman & Long, 2016; Denning, 2019; Fack & Grenet, 2015; Glocker, 2011; Murphy & Wyness, 2023; Nguyen, Kramer & Evans, 2019; workload: Denning, 2019; Herber & Kalinowski, 2019; Kofoed, 2022; Park & Scott-Clayton, 2018; 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 collection (Destatis, 2022a). Given the magnitude of these thresholds, students are likely to 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

[3] The maximum amount is reduced to €812 if the student is covered by their parents' health insurance. 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.

[5] Additional allowances apply if one is married and/or has children to take care of.

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.

4.3 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) before the data collection started. The preregistration was updated 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).

4.3.1 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 C.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 C.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 4.3.2.

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 C.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 4.3.3.

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 close 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.

4.3.2 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. Attanasio & Kaufmann, 2014; Boneva, Golin & Rauh, 2022; Boneva & Rauh, 2018; Manski, 2004). 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 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.[6] Additionally, I recorded if participants left the online survey website on each survey page of 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

[6]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.

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 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".

4.3.3 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 C.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 C.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, 2021). Students from the same university, study program, and cohort were always assigned to the same group to minimize spillovers. Appendix C.1.1 provides a detailed description of the stratification process.

4.3.4 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.[7] 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.[8] 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 4.1. 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 the repayment amounts by €2,827, on average. As the p-values in the last column of Table 4.1 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

[7] Participants who indicate a student aid amount over €10,000 per month in all three questions in the first scenario, income thresholds for parents over €500,000 for both questions in the second scenario, or repayment amounts over €100,000 for all three questions in the third scenario were excluded.

[8] 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 4.1: 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 €)	1048.478	484.295	1045.080	504.735	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
Studies in 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	224.420	0.585
Income thresholds for parents	-14951.85	24695.91	-15923.55	23028.30	0.108
Repayment amounts	2887.425	4317.000	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.

this information on misperceptions and take-up rates that were measured as part of the second and third wave of data collection.

4.4 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.

4.4.1 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 4.1, 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.6 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 4.2. 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).

$$MDiff_i = \beta_0 + \beta_1 Int_i + \beta_2 (Int_i \times Overest_i) + \beta_3 Overest_i + \delta_j X_{ij} + \alpha_s + \gamma_u + \epsilon_i \quad (4.1)$$

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 4.3. 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 4.2 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 4.2, 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 the sociodemographic and economic background of the students allow me to determine the individual eligibility of students

TABLE 4.2: Intervention Effect on Difference in Misperceptions from 1st to 2nd Wave

	Correction of Misperceptions (in %)				
	Amounts of Student Aid (1)	Income Thresh. for Parents (2)	Repayment Amounts (3)	Pooled Domains (4)	Total Number (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 to 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 4.3.2. 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 4.3, 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

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 4.3 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 in Table 4.3. 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

TABLE 4.3: 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)	(0.255)
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 4.3, 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

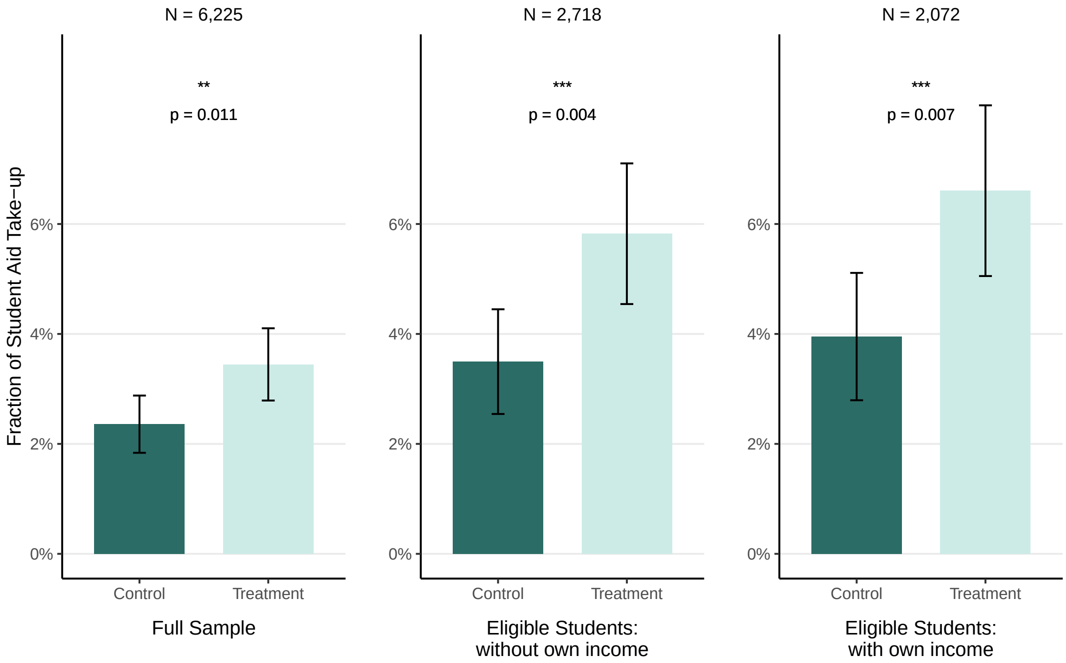
pp. Given that already 10% of the control group correct their eligibility misperceptions after participating in the first wave, as shown by the constant in columns 1 and 3, the intervention amplifies this correction by 30-57%. Using all changes in the Likert scale question on perceived eligibility as 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.

4.4.2 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

FIGURE 4.1: 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.

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 4.1, 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 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 C.4 as robustness checks.[9]

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.[10] 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.[11]

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 that 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.

4.4.3 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) (Angrist, Imbens & Rubin, 1996; Imbens & Angrist, 1994). All assumptions to estimate the LATE are fulfilled. A detailed discussion is provided in Appendix C.1.3.

The LATE yields the causal effect of correcting misperceptions on take-up for the compliers, i.e., the students whose misperceptions are correctable

[9] 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.

[10] 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.

[11] 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.

TABLE 4.4: 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 (1)	*Likert* (2)	*Binary* (3)	*Likert* (4)	*Pooled* (5)	*Total* (6)
Correction of Misperceptions (in %)	0.551** (0.242)	0.535** (0.210)	0.398*** (0.148)	0.444*** (0.166)	0.384** (0.166)	0.424** (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 estimations 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 to 4 use misperceptions about the participant's own eligibility and columns 5 and 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 point increase in take-up through a correction of misperceptions by 100% 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 4.3, 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

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 4.2 and 4.3 show the two-stage least squares model (2SLS).

$$MDiff_i = \beta_0 + \beta_1 Int_i + \delta_j X_{ij} + \alpha_s + \gamma_u + \epsilon_i \tag{4.2}$$

$$Takeup_i = \pi_0 + \pi_1 \widehat{MDiff}_i + \mu_j X_{ij} + \alpha_s + \gamma_u + \eta_i \tag{4.3}$$

In the first stage, $Takeup_i$ is the correction of misperceptions from the first to the second wave, and $Takeup_i$ is the intervention indicator. In the second stage, $Takeup_i$ is the indicator for take-up as the dependent variable, and $\widehat{MDiff}_i$ 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 4.3, 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 4.4 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 of Table 4.4 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.[12]

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 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.[13]

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

4.4.4 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 (Athey & Wager, 2019; Wager & Athey, 2018), 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

[12] I find similar significances when I use a probit model as second stage, as shown in Table C.24.

[13] 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 Kroher et al., 2023; Middendorff et al., 2017).

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 C.1.2.

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.[14] Both causal forests used clustering on the study field per university level, one level above the strata from the treatment assignment, as done throughout my analysis. 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 for heterogeneity based on the causal forest are presented in Figure 4.2.

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.

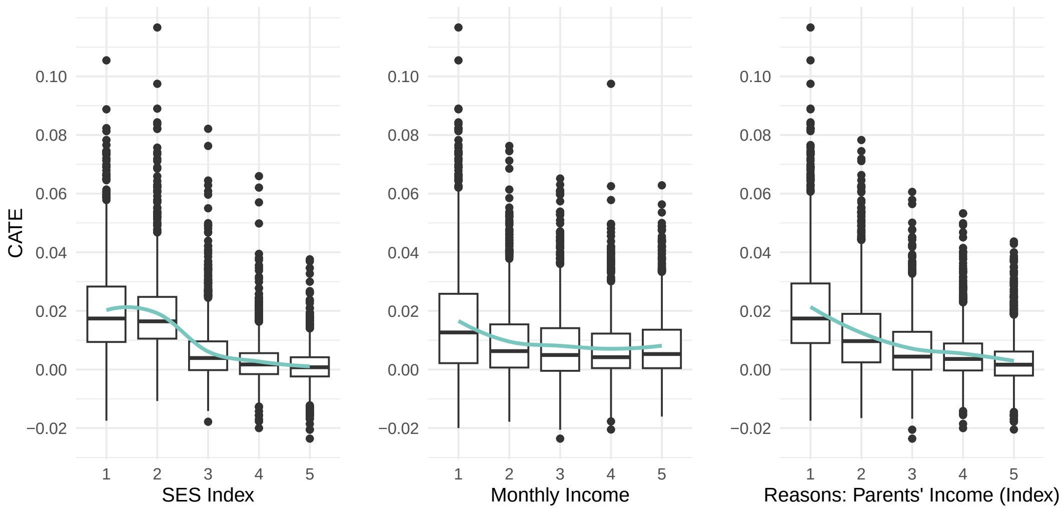
Analyzing these heterogeneities not only for the predicted CATEs but also the true intervention effects, I estimate the following model:

$$Takeup_i = \beta_0 + \beta_1 Int_i + \beta_2 X_i + \beta_3(Int_i \times D_i) + \delta_1 Aid_i + \alpha_s + \gamma_u + \epsilon_i \tag{4.4}$$

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

[14] 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 4.2: 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 the variables' quintiles are displayed. The mean-CATEs are connected with a fitted line.

forest, as shown in Figure 4.2, and D_i is an indicator equal to 1 for students below the 40%-quantile, so in the lowest two quintiles of X_i. Since all variables that drive heterogeneities are related to the students' 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 4.5.

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.[15] 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

[15]The respective probit estimations are reported in Tables C.26 to C.31.

TABLE 4.5: 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

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 (Avdic & Gartell, 2015; Bettinger et al., 2019; Black et al., 2023; Callender, 2008; Triventi, 2014). Additionally, the reduction in parental support indicates that the families also benefit from take-up. 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.

4.5 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.

Appendix A

Appendix to Chapter 2

A.1 Figures

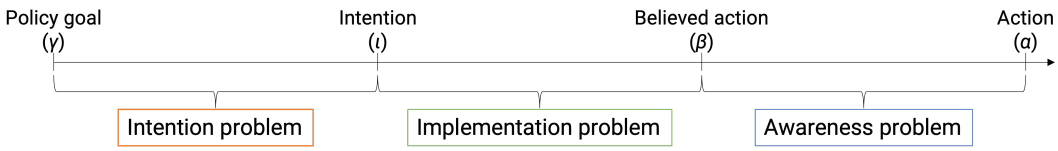


FIGURE A.1: The Framework's Fundamental Problems Along the Decision Process

When we adjust the predicted effectiveness of interventions within our framework, we have to consider fundamental problems that exist between the problem we address and the actual performance in the decision process, as shown in Figure A.1. If concurrent problems towards actual performance exist, we need to adjust for them as they distort the intervention's effectiveness to change actual performance.

As the first fundamental problem along this process is the intention problem, we need to adjust the prediction for an intervention that tackles the intention problem by other problems that might interfere with actual performance. Since the implementation and the awareness problem are between the intention problem and actual performance, we need to adjust the predicted effectiveness of an intention intervention for these two problems in case they exist concurrently to the intention problem.

Similarly, for predicting the effectiveness of an implementation intervention, we need to consider a concurrent awareness problem. The awareness problem can still interfere with the implementation of an intention as an individual can deviate from their believed performance.

For an intervention that addresses the awareness problem, no further adjustments are necessary. The awareness gap directly tackles actual performance through correcting believed performance. An intention and implementation problem do not interfere with this correction.

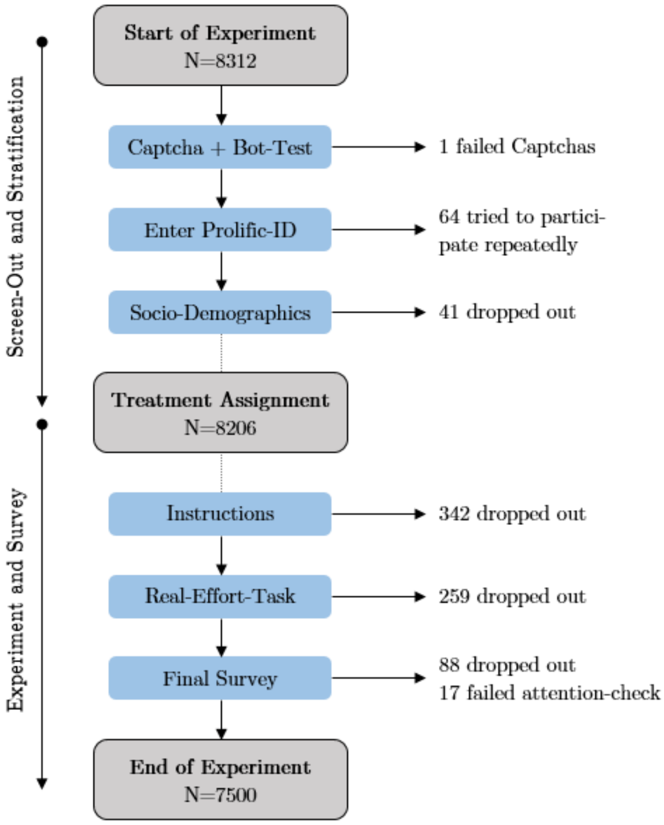


FIGURE A.2: Flowchart of the Sampling Process

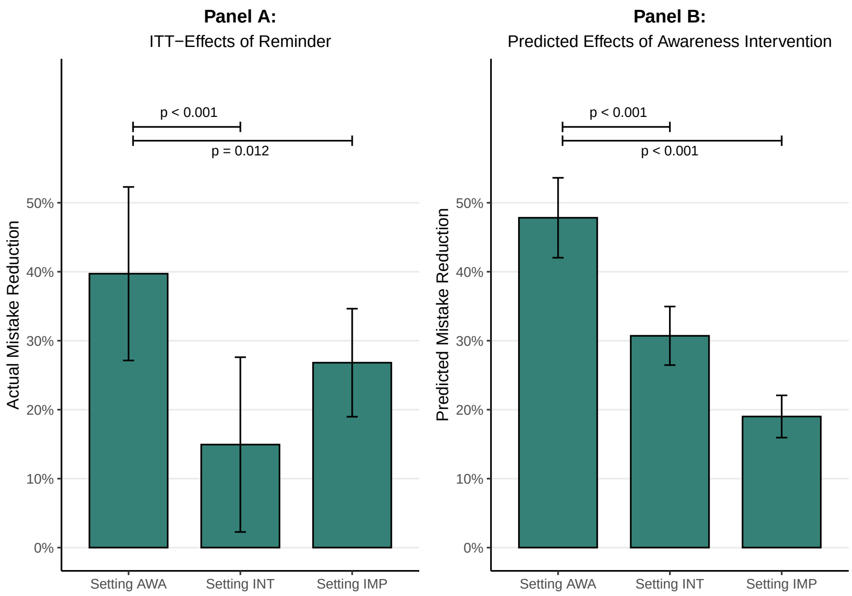


FIGURE A.3: ITT-Effects and Predictions of an Awareness-Addressing Intervention

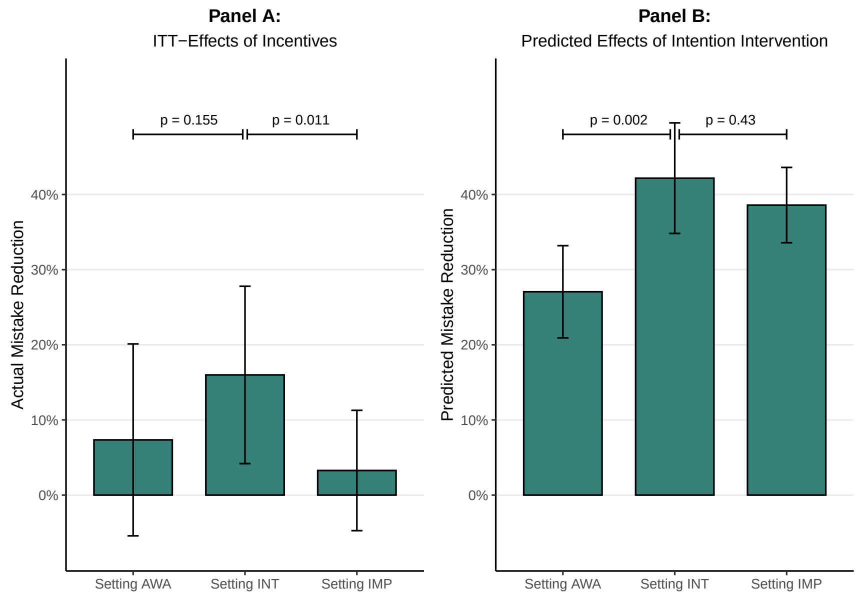


FIGURE A.4: ITT-Effects and Predictions of an Intention-Addressing Intervention

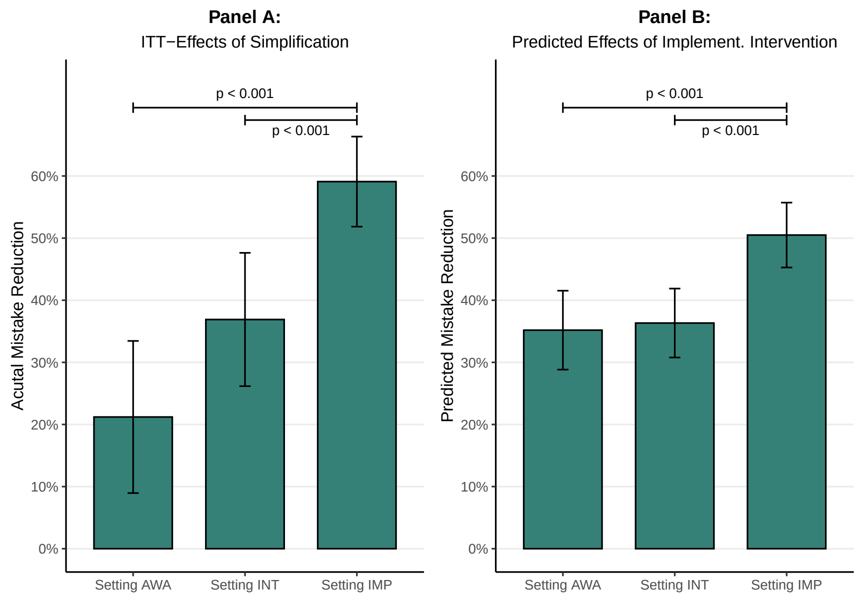


FIGURE A.5: ITT-Effects and Predictions of an Implementation-Addressing Intervention

A.2 Tables

TABLE A.1: Balance Table for the Experimental Sample

Experimental Group	N	Age	Female	College	Republ.	Income	HH-Size	Studying	Unempl.	Working	Self-empl.	Retired
Setting AWA:												
Baseline	636	34.728	0.492	0.627	0.132	8.429	2.789	0.116	0.145	0.615	0.086	0.038
Reminder	629	34.809	0.498	0.622	0.138	8.226	2.801	0.116	0.146	0.595	0.113	0.030
Incentive	634	35.251	0.497	0.618	0.155	8.349	2.793	0.110	0.161	0.585	0.098	0.046
Simplification	640	34.392	0.498	0.630	0.148	8.097	2.694	0.123	0.164	0.597	0.086	0.030
Setting INT:												
Baseline	645	35.101	0.509	0.623	0.146	7.969	2.780	0.096	0.164	0.588	0.112	0.040
Reminder	631	34.778	0.507	0.626	0.166	8.379	2.800	0.105	0.171	0.578	0.116	0.030
Incentive	623	34.920	0.512	0.624	0.141	7.918	2.729	0.109	0.175	0.575	0.109	0.032
Simplification	644	34.991	0.505	0.630	0.148	7.924	2.769	0.115	0.175	0.568	0.102	0.039
Setting IMP:												
Baseline	593	34.642	0.492	0.631	0.123	8.083	2.776	0.115	0.170	0.599	0.081	0.035
Reminder	608	34.243	0.487	0.643	0.148	8.113	2.768	0.130	0.141	0.607	0.084	0.038
Incentive	588	34.350	0.493	0.641	0.107	8.163	2.743	0.121	0.146	0.612	0.095	0.026
Simplification	629	35.134	0.512	0.623	0.138	8.224	2.738	0.118	0.145	0.599	0.113	0.025
F-statistic		0.403	0.178	0.146	1.166	1.740	0.375	0.481	0.805	0.559	1.154	0.739
p-value		0.955	0.999	0.999	0.305	0.059	0.966	0.916	0.635	0.863	0.314	0.702
Significant Diff.		0.000	0.000	0.000	0.106	0.136	0.000	0.000	0.000	0.000	0.015	0.000

Notes: The table shows sociodemographic variables per experimental group. Each row corresponds to one group. The last three rows show the joint F-statistic and the corresponding p-value on whether the means of the groups are significantly different from each other, and the fraction of significant t-tests from all pairwise comparisons. The columns show the averages of the sociodemographic variables per group. *Female*, *College*, *Republican*, *Studying*, *Unemployed*, *Working*, *Self-employed* and *Retired* are indicator-variables that equal 1 if the person meets the corresponding characteristic. *Income* is a categorical variable. The categories 7 and 8 for income correspond to a household net income of $2,500 to $3,000 and $3,000 to $3,500 per month. The treatment assignment was stratified by age, gender, and college education.

TABLE A.2: Balance Table for the Full Sample

Experimental Group	N	Finished	Age	Female	College	Republ.	Income	HH-Size	Studying	Unempl.	Working	Self-empl.	Retired
Setting AWA:													
Baseline	682	636	34.930	0.507	0.626	0.130	8.309	2.782	0.116	0.152	0.610	0.085	0.037
Reminder	683	629	35.135	0.504	0.627	0.142	8.221	2.785	0.108	0.149	0.592	0.113	0.038
Incentive	683	634	35.363	0.504	0.625	0.152	8.329	2.792	0.105	0.163	0.592	0.097	0.044
Simplification	683	640	34.524	0.504	0.628	0.146	8.133	2.700	0.123	0.163	0.596	0.085	0.034
Setting INT:													
Baseline	686	645	35.350	0.504	0.625	0.144	7.968	2.770	0.093	0.169	0.589	0.108	0.041
Reminder	679	631	35.047	0.505	0.626	0.163	8.377	2.797	0.103	0.166	0.586	0.112	0.032
Incentive	683	623	35.510	0.505	0.627	0.139	7.971	2.739	0.104	0.176	0.570	0.111	0.040
Simplification	679	644	35.203	0.504	0.627	0.150	7.959	2.766	0.110	0.174	0.571	0.106	0.038
Setting IMP:													
Baseline	685	593	34.978	0.504	0.626	0.121	8.028	2.750	0.111	0.180	0.587	0.085	0.038
Reminder	681	608	34.675	0.505	0.627	0.153	8.060	2.794	0.131	0.151	0.590	0.087	0.041
Incentive	682	588	35.362	0.504	0.629	0.123	7.988	2.748	0.120	0.158	0.591	0.089	0.041
Simplification	683	629	35.120	0.507	0.625	0.142	8.183	2.725	0.113	0.152	0.594	0.114	0.026
F-statistic		7.075	0.345	0.004	0.004	0.865	1.488	0.348	0.692	0.534	0.316	1.189	0.428
p-value		0.000	0.976	1.000	1.000	0.575	0.128	0.975	0.748	0.882	0.983	0.289	0.945
Significant Diff.		0.424	0.000	0.000	0.000	0.030	0.121	0.000	0.015	0.000	0.000	0.000	0.000

Notes: The table shows sociodemographic variables per experimental group. Each row corresponds to one group. The last three rows show the joint F-statistic and the corresponding p-value on whether the means of the groups are significantly different from each other, and the fraction of significant t-tests from all pairwise comparisons. The columns show the averages of the sociodemographic variables per group. *Female, College, Republican, Studying, Unemployed, Working, Self-employed* and *Retired* are indicator-variables that equal 1 if the person meets the corresponding characteristic. *Income* is a categorical variable. The categories 7 and 8 for income correspond to a household net income of $2,500 to $3,000 and $3,000 to $3,500 per month. The treatment assignment was stratified by age, gender, and college education.

TABLE A.3: Intervention Effects

	Standardized Mistakes					
	Reminders		Incentives		Simplifications	
	(1)	(2)	(3)	(4)	(5)	(6)
Intervention (=1)	-0.538^{***}	-0.539^{***}	-0.293^{***}	-0.292^{***}	-0.670^{***}	-0.666^{***}
	(0.046)	(0.046)	(0.045)	(0.045)	(0.032)	(0.032)
Intervention X Setting INT	0.299^{***}	0.303^{***}			0.261^{***}	0.255^{***}
	(0.061)	(0.061)			(0.035)	(0.035)
Intervention X Setting IMP	0.225^{***}	0.223^{***}	0.247^{***}	0.246^{***}		
	(0.047)	(0.047)	(0.048)	(0.048)		
Intervention X Setting AWA			0.202^{***}	0.204^{***}	0.416^{***}	0.414^{***}
			(0.059)	(0.058)	(0.042)	(0.042)
Age		0.002		-0.001		0.001
		(0.002)		(0.002)		(0.002)
Female (=1)		-0.096^{***}		-0.046		-0.059^{*}
		(0.034)		(0.034)		(0.031)
College degree (=1)		0.004		-0.059		-0.020
		(0.038)		(0.038)		(0.034)
Republican (=1)		0.055		0.047		0.048
		(0.052)		(0.052)		(0.046)
Net income		-0.014^{**}		-0.018^{***}		-0.016^{***}
		(0.006)		(0.006)		(0.005)
Household size		0.005		0.026^{*}		0.031^{**}
		(0.013)		(0.013)		(0.012)
City size		0.008		-0.003		0.007
		(0.009)		(0.009)		(0.008)
Working (=1)		0.137^{***}		0.171^{***}		0.077^{*}
		(0.049)		(0.048)		(0.045)
Self-employed (=1)		0.097		0.085		0.004
		(0.071)		(0.070)		(0.065)
Student (=1)		0.049		0.059		-0.005
		(0.067)		(0.068)		(0.061)
Retired (=1)		-0.056		0.089		0.052
		(0.111)		(0.118)		(0.110)
Constant	1.000^{***}	0.927^{***}	1.000^{***}	1.040^{***}	1.000^{***}	0.950^{***}
	(0.026)	(0.102)	(0.026)	(0.097)	(0.026)	(0.093)
Observations	3,742	3,742	3,719	3,719	3,787	3,787
R^2	0.036	0.043	0.010	0.017	0.066	0.072
F Statistic	46.775^{***}	11.823^{***}	12.535^{***}	4.672^{***}	89.459^{***}	20.765^{***}

Notes: This table shows OLS results of the intervention effects on the number of mistakes in the real-effort task across setting with and without control variables based on the participants who finished the experiment. The dependent variable is the number of incorrect answers given in the real-effort task divided by the average number of incorrect answers of the baseline groups per setting for comparison. Which intervention effects are considered is displayed above the column numbers. *Intervention* is a dummy variable whose coefficient shows the treatment effect of the respective intervention in the setting that serves as reference group. The interaction terms show the additional effects in the other two settings. Omitted settings for the interaction are the reference groups, respectively. Robust standard errors are in parentheses.

$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

TABLE A.4: Intention-To-Treat Effects

	Standardized Mistakes					
	Reminders		Incentives		Simplifications	
	(1)	(2)	(3)	(4)	(5)	(6)
Intervention (=1)	−0.397***	−0.395***	−0.160***	−0.162***	−0.591***	−0.587***
	(0.049)	(0.049)	(0.047)	(0.047)	(0.033)	(0.033)
Intervention X Setting INT	0.248***	0.250***			0.222***	0.216***
	(0.064)	(0.064)			(0.040)	(0.040)
Intervention X Setting IMP	0.129**	0.129**	0.127**	0.127**		
	(0.051)	(0.051)	(0.050)	(0.050)		
Intervention X Setting AWA			0.086	0.092	0.379***	0.380***
			(0.061)	(0.061)	(0.047)	(0.047)
Age		0.006***		0.005***		0.004**
		(0.002)		(0.002)		(0.002)
Female (=1)		−0.047		−0.020		−0.036
		(0.033)		(0.033)		(0.031)
College degree (=1)		−0.011		−0.033		−0.038
		(0.037)		(0.036)		(0.034)
Republican (=1)		0.038		0.011		0.027
		(0.050)		(0.049)		(0.045)
Net income		−0.019***		−0.025***		−0.016***
		(0.006)		(0.005)		(0.005)
Household size		0.003		0.028**		0.021*
		(0.013)		(0.013)		(0.012)
City size		−0.0004		0.001		0.002
		(0.009)		(0.008)		(0.008)
Working (=1)		0.062		0.066		0.002
		(0.049)		(0.048)		(0.045)
Self-employed (=1)		−0.009		−0.035		−0.066
		(0.069)		(0.069)		(0.064)
Student (=1)		−0.056		−0.037		−0.094
		(0.063)		(0.064)		(0.059)
Retired (=1)		−0.036		0.031		−0.010
		(0.120)		(0.116)		(0.114)
Constant	1.000***	0.946***	1.000***	0.948***	1.000***	0.978***
	(0.024)	(0.098)	(0.024)	(0.093)	(0.024)	(0.092)
Observations	4,096	4,096	4,101	4,101	4,098	4,098
R^2	0.021	0.030	0.003	0.012	0.051	0.057
F-Statistic	28.654***	8.875***	4.251***	3.539***	73.022***	17.680***

Notes: This table shows OLS results of the intervention effects on the number of mistakes in the real-effort task across setting with and without control variables based on the participants who were assigned to an experimental group. The dependent variable is the number of incorrect answers given in the real-effort task divided by the average number of incorrect answers of the baseline groups per setting for comparison. Which intervention effects to be considered is displayed above the column numbers. *Intervention* is a dummy variable whose coefficient shows the treatment effect of the respective intervention in the setting that serves as reference group. The interaction terms show the additional effects in the other two settings. Omitted settings for the interaction are the reference groups, respectively. Robust standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

TABLE A.5: Raw Diagnosis Prediction of Intervention Effects

	Actual Mistake Reduction (standardized)			
	Reminders	Incentives	Simplification	Pooled
	(1)	(2)	(3)	(4)
Panel A: Individual Level				
Predicted Mistake Reduction	0.001***	−0.0004*	0.004***	0.001***
(raw, standardized)	(0.0004)	(0.0002)	(0.0004)	(0.0003)
Panel B: Aggregate Level - Intercept				
Predicted Mistake Reduction	0.851***	0.360	2.135***	0.587***
(raw, standardized)	(0.211)	(0.505)	(0.588)	(0.169)
Panel C: Aggregate Level - No Intercept				
Predicted Mistake Reduction	1.085***	0.323***	0.996***	0.759***
(raw, standardized)	(0.082)	(0.060)	(0.051)	(0.038)

Notes: This table shows OLS results of the predicted mistake reduction based on the raw diagnoses of our framework on the actual mistake reduction due to the intervention without adjusting for concurrent problems. We use the diagnosis of the baseline settings in each of the three settings for the prediction of how many mistakes are reduced due to one respective intervention. The actual reduction of mistakes per intervention and setting is used as the outcome variable. Both the predicted and the actual mistake reduction are standardized by the average mistakes of the baseline groups in each setting. Regression coefficients are in percentage terms. For Panel A, we use the predictions on the individual level as independent variable with robust standard errors in parentheses. To obtain standard errors for the aggregate levels in Panels B and C, we used bootstrapping to resample the original sample 1000 times, calculate the mean intervention and predicted effects for each setting, and perform the OLS analysis on the aggregate data with and without allowing for an intercept. The standard deviations of these bootstrapped coefficients are used as standard errors and reported in parentheses.
*p<0.1; **p<0.05; ***p<0.01

TABLE A.6: Framework Prediction of ITT Effects

	Actual Mistake Reduction ITT (standardized)			
	Reminders (1)	Incentives (2)	Simplification (3)	Pooled (4)
Panel A: Individual Level				
Predicted Mistake Reduction	0.021***	0.002	0.018***	0.016***
(standardized)	(0.004)	(0.002)	(0.006)	(0.003)
Panel B: Aggregate Level - Intercept				
Predicted Mistake Reduction	0.521**	0.345	2.091***	0.940***
(standardized)	(0.235)	(0.648)	(0.632)	(0.190)
Panel C: Aggregate Level - No Intercept				
Predicted Mistake Reduction	0.798***	0.250	0.993***	0.704***
(standardized)	(0.234)	(0.291)	(0.188)	(0.095)

Notes: This table shows OLS results of the predicted mistake reduction based on our framework on the actual mistake reduction due to the intervention using the ITT sample and effects. We use the diagnosis of the baseline settings in each of the three settings for the prediction of how many mistakes are reduced due to one respective intervention. The actual reduction of mistakes in the ITT sample per intervention and setting is used as the outcome variable. Both the predicted and the actual mistake reduction are standardized by the average mistakes of the baseline groups in each setting. Regression coefficients are in percentage terms. For Panel A, we use the predictions on the individual level as independent variable with robust standard errors in parentheses. To obtain standard errors for the aggregate levels in Panels B and C, we used bootstrapping to resample the original sample 1000 times, calculate the mean intervention and predicted effects for each setting, and perform the OLS analysis on the aggregate data with and without allowing for an intercept. The standard deviations of these bootstrapped coefficients are used as standard errors and reported in parentheses.
*p<0.1; **p<0.05; ***p<0.01

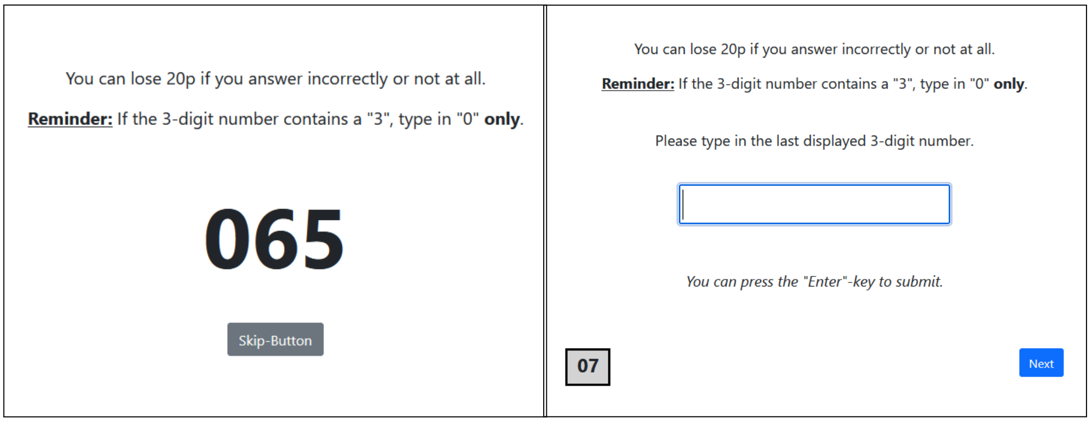
TABLE A.7: Prediction of Intervention Effects by the Framework

	Predicted Mistake Reduction (standardized)		
	Individual Level	Aggregate Level	Aggregate Level - No Intercept
Dependent Variable:	(1)	(2)	(3)
Actual Mistake Reduction	0.020***	1.196***	0.892***
(standardized)	(0.004)	(0.198)	(0.045)

Notes: This table shows OLS results of the predicted mistake reduction based on our framework on the actual mistake reduction pooled over all interventions. We use the diagnosis of the baseline groups in each of the three settings for the prediction of how many mistakes are reduced by the respective intervention. The actual reduction of mistakes is used as the outcome variable. Both the predicted and the actual mistake reduction are standardized by the average mistakes of the baseline groups in each setting. Regression coefficients are in percentage terms. For column 1, we use the predictions on the individual level as independent variable with robust standard errors in parentheses. To obtain standard errors for the aggregate levels in columns 2 and 3, we used bootstrapping to resample the original sample 1000 times, calculate the mean intervention and predicted effects for each setting, and perform the OLS analysis on the aggregate data with and without allowing for an intercept. The standard deviations of these bootstrapped coefficients are used as standard errors and reported in parentheses.
*p<0.1; **p<0.05; ***p<0.01

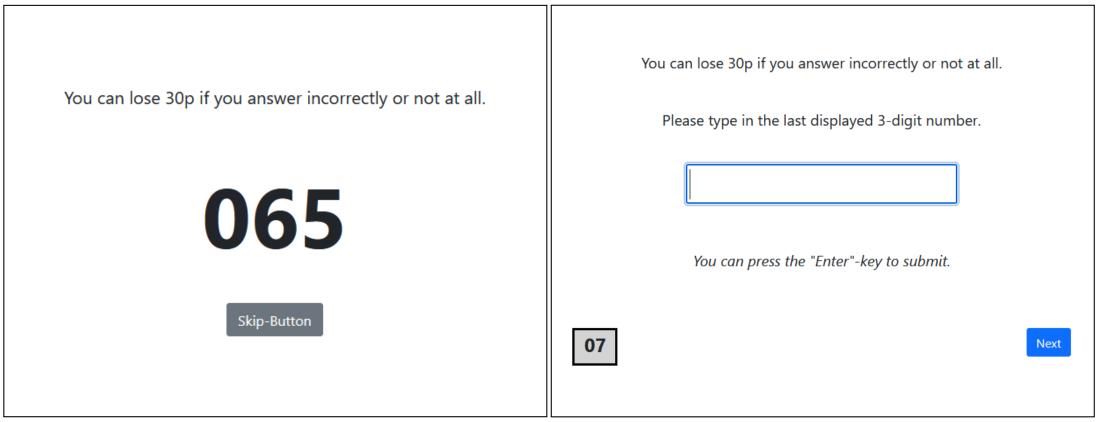
A.3 Experimental Screens

FIGURE A.6: Screens Setting AWA - Reminder



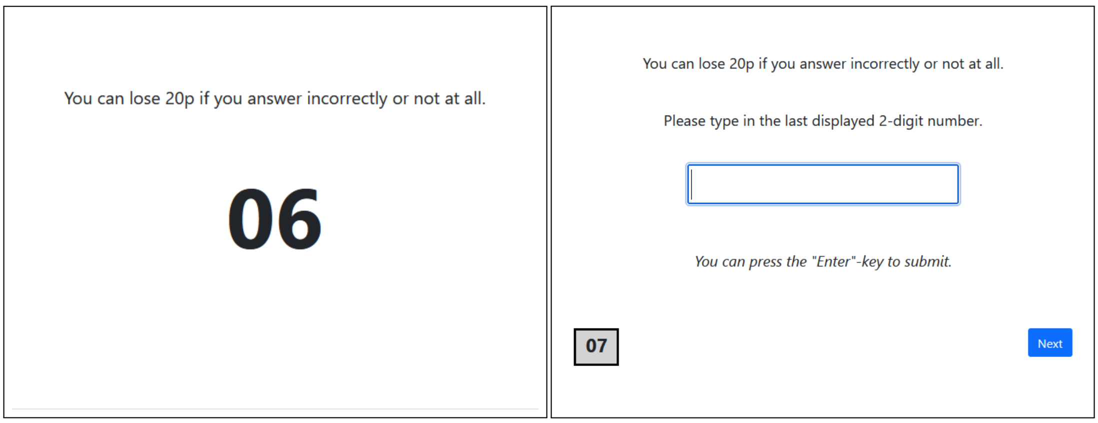
A: Numbers displayed during task B: Query to enter last displayed number

FIGURE A.7: Screens Setting AWA - Incentives



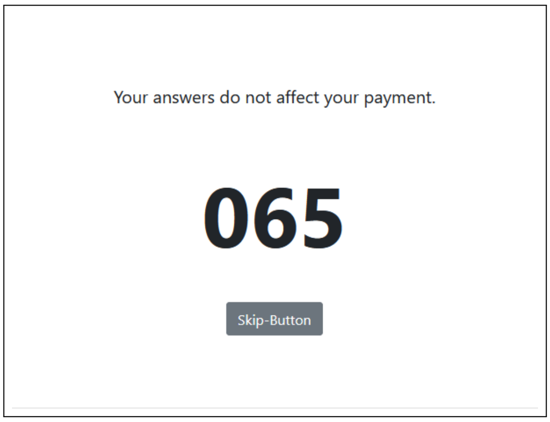
A: Numbers displayed during task B: Query to enter last displayed number

FIGURE A.8: Screens Setting AWA - Simplifications

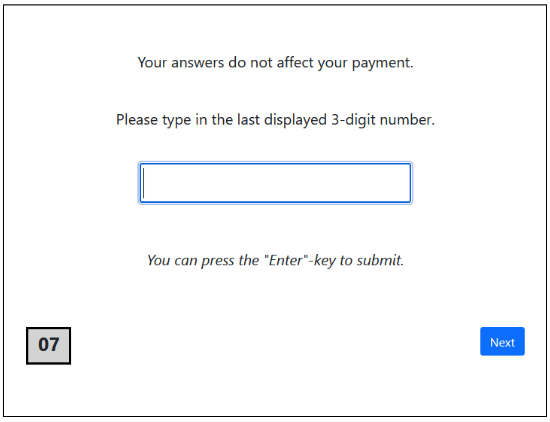


A: Numbers displayed during task B: Query to enter last displayed number

FIGURE A.9: Screens Setting INT - Baseline

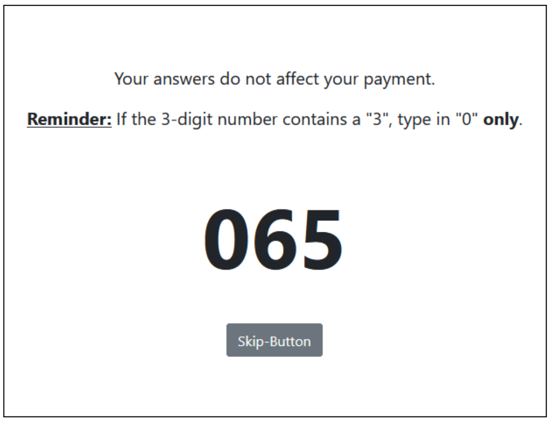


A: Numbers displayed during task

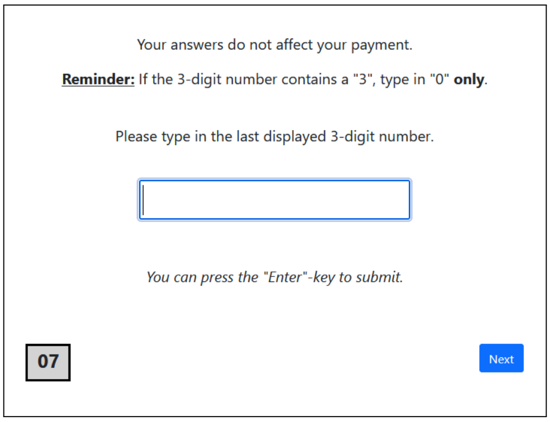


B: Query to enter last displayed number

FIGURE A.10: Screens Setting INT - Reminder

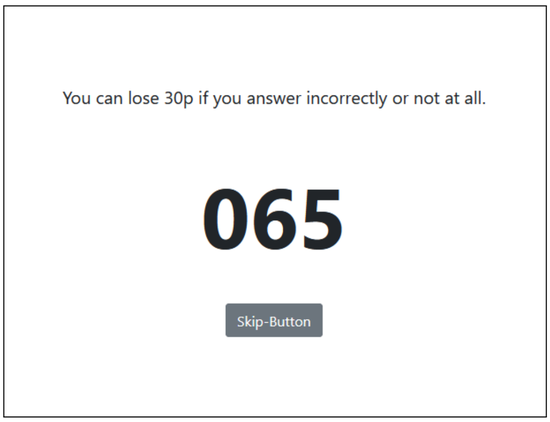


A: Numbers displayed during task



B: Query to enter last displayed number

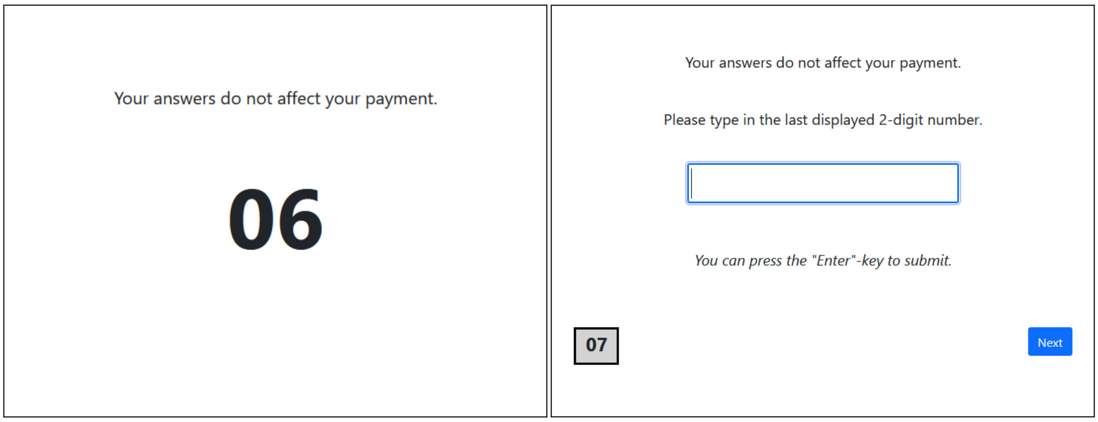
FIGURE A.11: Screens Setting INT - Incentives



A: Numbers displayed during task

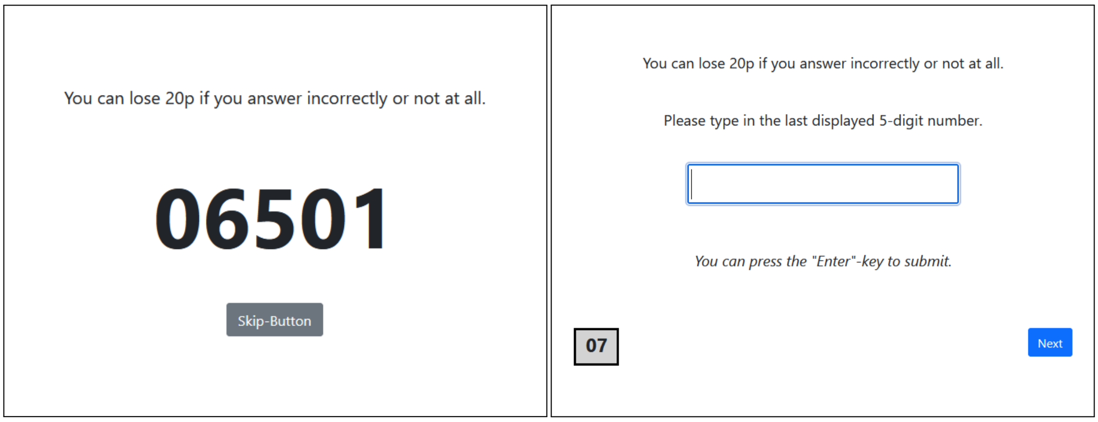
B: Query to enter last displayed number

FIGURE A.12: Screens Setting INT - Simplifications



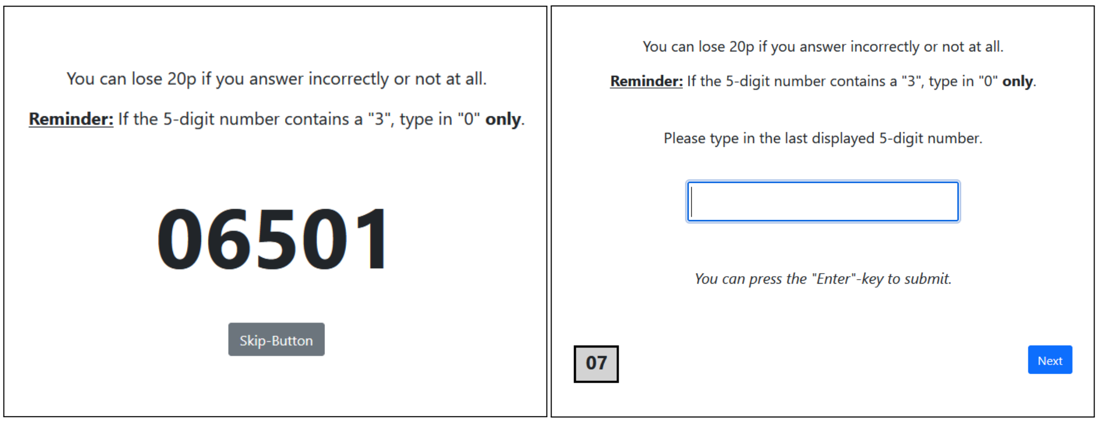
A: Numbers displayed during task B: Query to enter last displayed number

FIGURE A.13: Screens Setting IMP - Baseline



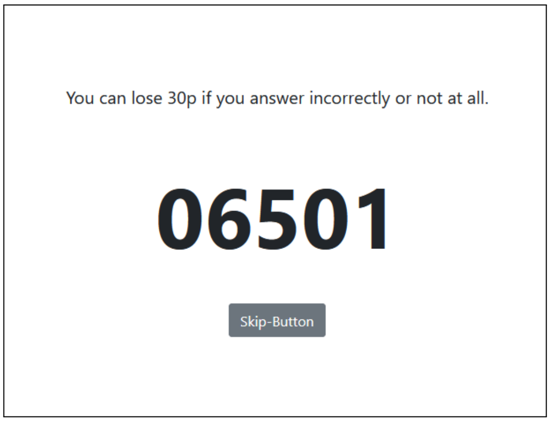
A: Numbers displayed during task B: Query to enter last displayed number

FIGURE A.14: Screens Setting IMP - Reminder



A: Numbers displayed during task B: Query to enter last displayed number

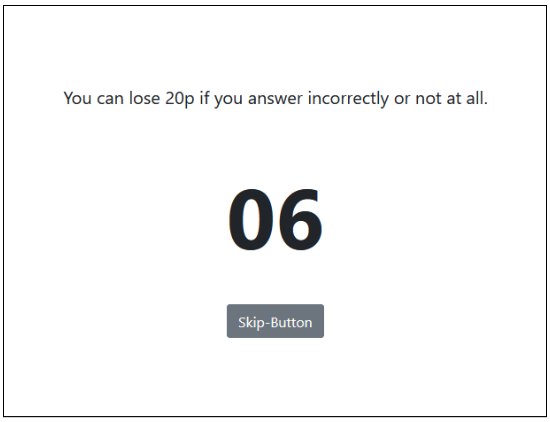
FIGURE A.15: Screens Setting IMP - Incentives



A: Numbers displayed during task

B: Query to enter last displayed number

FIGURE A.16: Screens Setting IMP - Simplifications



A: Numbers displayed during task

B: Query to enter last displayed number

Appendix B

Appendix to Chapter 3

B.1 Additional Technical Explanations

B.1.1 Misperception Elicitation

We ask eight questions with three scenarios to elicit misperceptions about (i) the student aid amount, (ii) the income thresholds for parents, and (iii) the repayment. For scenarios (i) and (iii), the second and third questions (Q2 and Q3) are randomized in order. All questions are incentivized. If the student enters a value in the interval around the correct answer displayed in parentheses behind each question, they receive an extra lottery ticket to win a prize of €25 or €50, depending on the survey wave.

Student aid amount depending on parents' income

Now consider the following basis scenario, which is the same for all three questions:

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.

For the BAföG calculation, the income from two years ago is considered.

Q1: How much BAföG do you think Anna receives per month (in EUR per month ±100€)?

Q2: Now, instead, imagine that Anna's mother is an employee with a gross annual income of €20,000 two years ago.
How much BAföG do you think Anna receives per month (in EUR per month ±100€)?

Q3: Now, instead, imagine that Anna has assets on her own in form of a savings account with €18,000.
How much BAföG do you think Anna receives per month (in EUR per month ±100€)?

Income thresholds for parents for a given student aid amount

Now consider the following basis scenario, which is the same in both questions:

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.

For the BAföG calculation, the parents' income from two years ago is considered.

"Unavailable" means that neither Max nor the BAföG office can find his father. Therefore, he is not included in the BAföG calculation.

Q1: What do you think Max's mother's gross annual income was 2 years ago (in EUR per year ±€7500)?

Q2: Now, instead, imagine that Max has a sister (Lisa, 24) who is also studying and lives in a student dormitory.
What do you think Max's mother's gross annual income was 2 years ago (in EUR per year ±€7500)?

Student aid repayment

Now consider the following basis scenario, which is the same for all three questions:

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.

Q1: How much do you think Sara has to pay back in total (in EUR ±500€)?

Q2: Now, instead, imagine that Sara repays her loan in one sum.
How much do you think Sara has to pay back in total (in EUR ±500€)?

Q3: Now, instead, imagine that Sara studied for 5 years and received €500 per month in BAföG so that she received a total of €30,000.
How much do you think Sara has to pay back in total (in EUR ±500€)?

B.1.2 Preference Elicitation

Debt Aversion

How do you see yourself: are you generally a person who is willing to take on debt or do you try to avoid debt?

Please answer using the following scale, where 0 means not at all willing to take on debt and 10 means very willing to take on debt. You can use the values in between to grade your assessment.

Impulsiveness

How do you see yourself: are you generally a person who thinks and reflects for a long time before acting, so you are not impulsive at all? Or are you a person who acts without thinking, so you are very impulsive?

Please answer using the following scale, where 0 means not at all impulsive and 10 means very impulsive. You can use the values in between to grade your assessment.

Patience

How do you see yourself: are you generally a person who is impatient, or who always has a lot of patience?

Please answer using the following scale, where 0 means: very impatient and 10 means: very patient. You can use the values in between to grade your assessment.

B.1.3 Weight Construction: Non-Take-Up Rate

To ensure that our sample accurately reflects the German student body, we apply the algorithm described by Merz (1985) to reweight our observations with minimal information loss. We use different data sources. Whenever possible, we use official statistics from the Federal Statistical Office of Germany, as reported and referenced in Destatis (2024). In case no official statistics exist, we use data from a representative survey of students in Germany, as reported in Kroher et al. (2023) with the data reference Becker et al. (2024). For the take-up rate of institutionally eligible students and their respective average student aid amount, we use numbers from the official federal student aid report by the government (Deutscher Bundestag, 2023). We create the weights based on student characteristics in the following order.

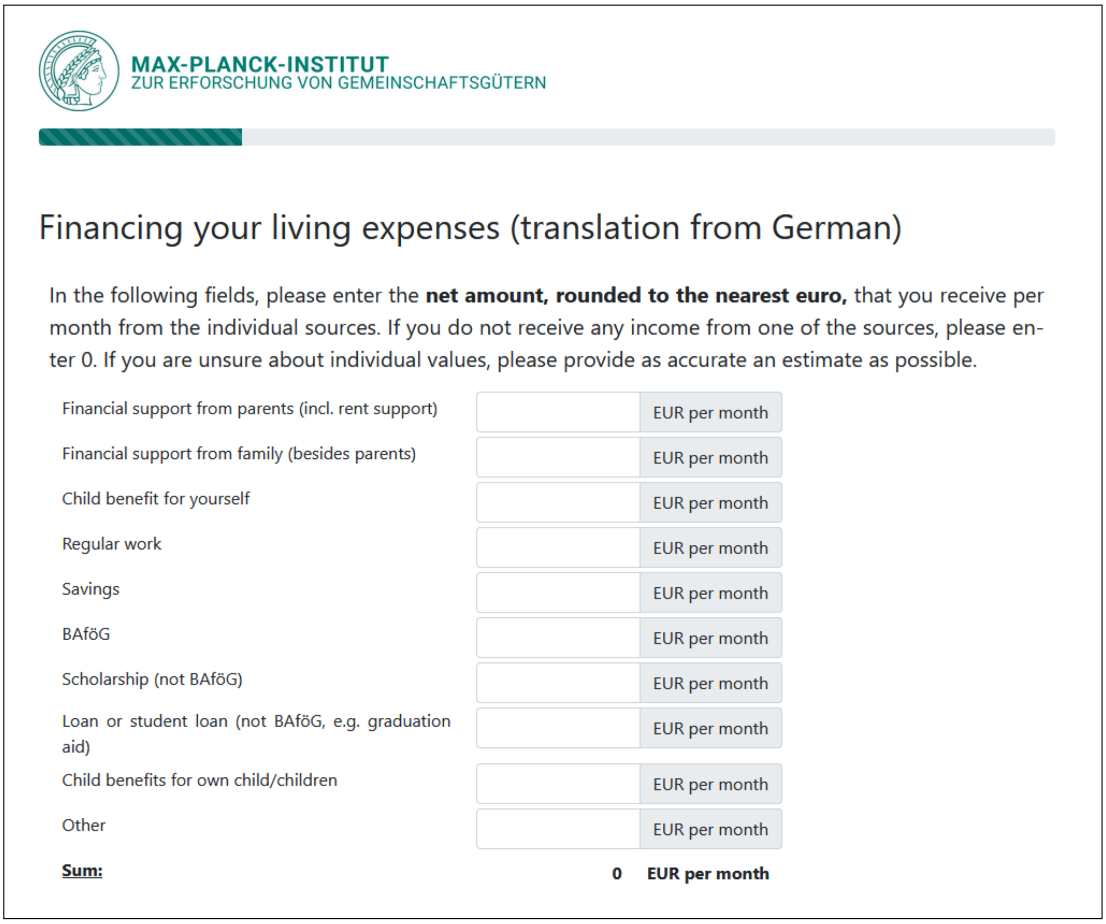
1. **Gender:** We use weights based on the gender distribution from the representative survey as reported in Kroher et al. (2023).
2. **Age:** We adjust the age distribution based on official statistics (Destatis, 2024). The age groups are: $\leq 17, 18, 19, \ldots, 35, \geq 36$.
3. **Field of Study:** We use the fraction of students enrolled in different fields of study corresponding to our list based on official statistics to adjust our sample (Destatis, 2024).
4. **Degree Type:** We create weights to represent the distribution of degree types among the institutionally eligible students based on calculations for the government by the Fraunhofer Institute for Applied Information Technology FIT. The calculation uses official enrollment numbers (Destatis, 2024) to estimate the number of institutionally eligible students.
5. **Migration Background and Parental Education:** These variables are weighted using data from the representative survey (Becker et al., 2024), restricted to institutionally eligible students to align with our sample.
6. **University location (East/West Germany):** Our weighting represents the fraction of students enrolled at universities in East Germany and West Germany from official statistics (Destatis, 2022a).

7. **Living with parents:** The weighting for the fraction of students living with their parents is based on two sources:

   - *Non-Receivers:* Weighted using data from the representative survey (Becker et al., 2024).
   - *Receivers:* Weighted using the numbers from the official report (Deutscher Bundestag, 2023).

8. **Eligibility and Take-Up Rates:** We use the official numbers of institutionally eligible students and receivers among them from the official student aid report (Deutscher Bundestag, 2023). Note that this report only calculates a take-up rate for *institutionally* eligible students. It remains unclear from this report how many of them could actually receive student aid based on their sociodemographic and economic situation. We calculate the actual (non-)take-up only considering students who have a positive entitlement and are, therefore, *actually* eligible.

B.2 Survey Screenshots



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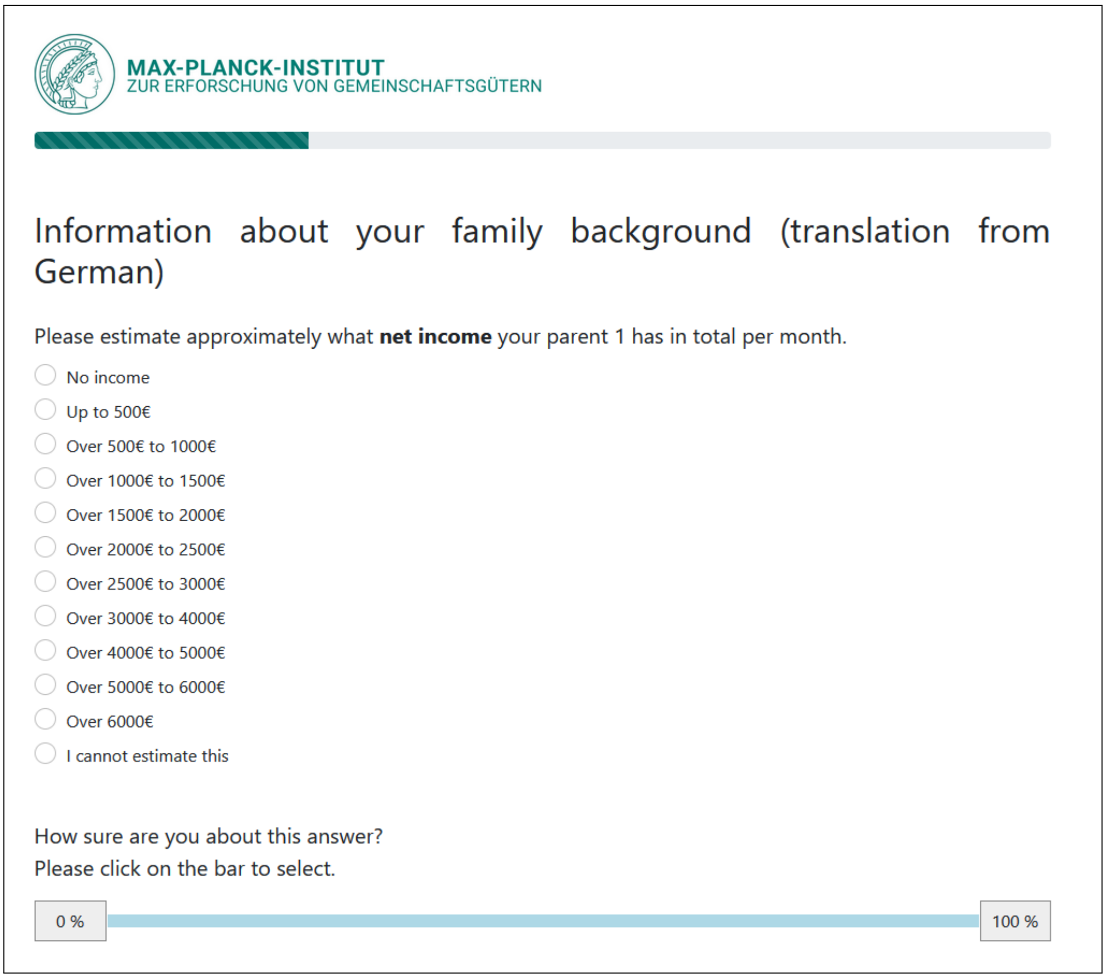
Financing your living expenses (translation from German)

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



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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.

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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.

B.3 Additional Results

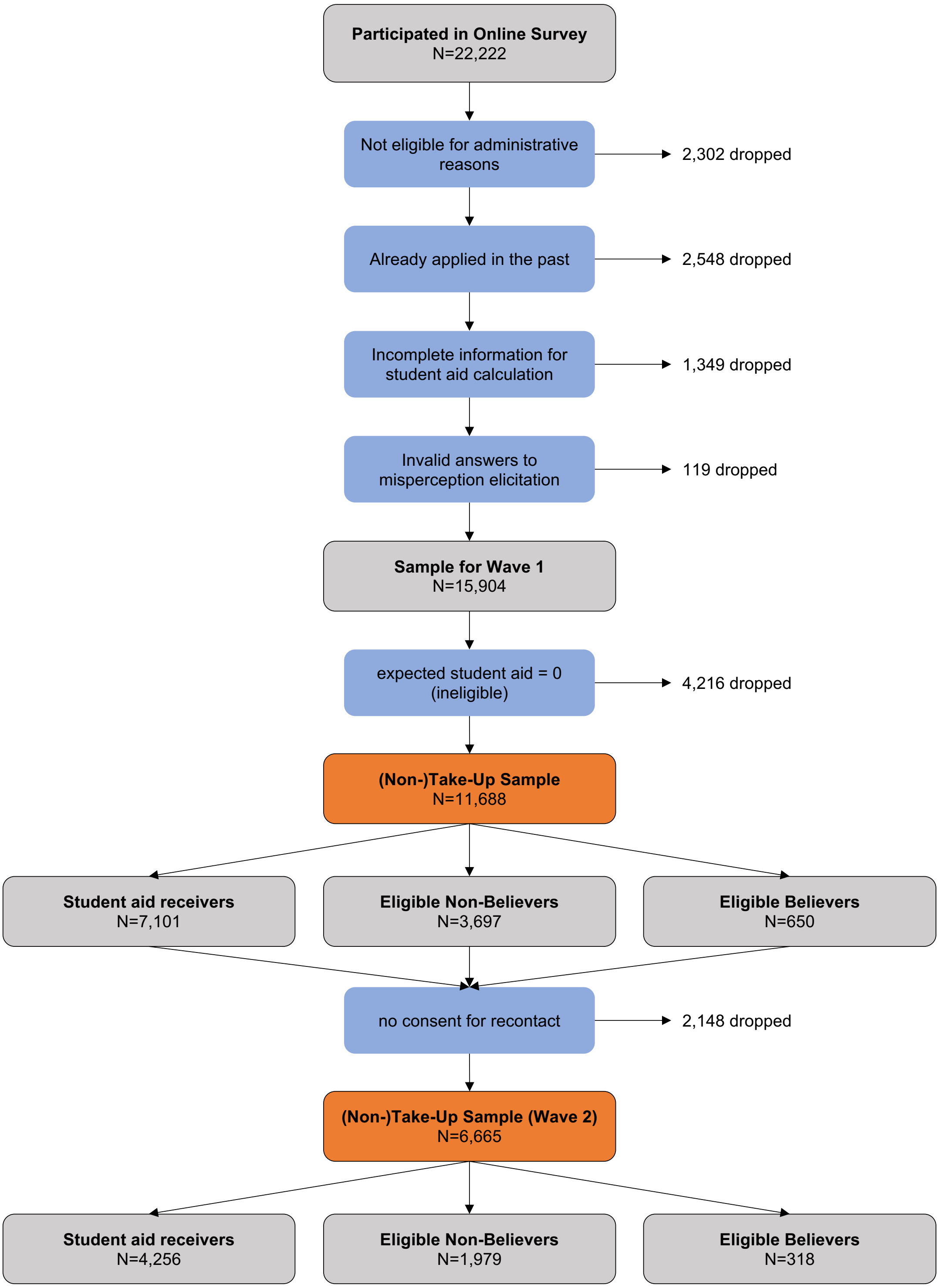


FIGURE B.4: Sample Construction

TABLE B.1: Non-Take-Up Rates

	Semesters beyond standard study period					
Non-Take-Up Selection	0	1	2	3	4	N
Excl. Students' Income and Wealth						
Full Sample						
Unweighted					**39.25%**	11,688
Weighted	61.87%	65.74%	68.25%	**70.55%**	71.99%	11,688
- entitlement >€450	53.97%	58.05%	60.75%	63.27%	64.88%	8,638
Full Sample - Only First Two Years						
Unweighted	40.43%					6,634
Weighted	60.67%					6,634
- entitlement >€450	55.10%					4,834
Restricted Sample						
Unweighted					**36.14%**	6,665
Weighted	61.58%	65.53%	68.08%	**70.42%**	71.88%	6,665
- entitlement >€450	54.69%	58.82%	61.55%	64.10%	65.71%	4,975
Restricted Sample - Only First Two Years						
Unweighted	36.68%					3,732
Weighted	59.17%					3,732
- entitlement >€450	54.95%					2,745
Incl. Students' Income and Wealth						
Full Sample						
Unweighted					35.39%	10,990
Weighted	58.82%	62.81%	65.42%	67.83%	69.34%	10,990
- entitlement >€450	47.39%	51.53%	54.32%	56.97%	58.68%	7,828
Full Sample - Only First Two Years						
Unweighted	36.75%					6,248
Weighted	57.27%					6,248
- entitlement >€450	48.61%					4,381
Restricted Sample						
Unweighted					32.41%	6,297
Weighted	58.93%	62.98%	65.62%	68.06%	69.59%	6,297
- entitlement >€450	48.13%	52.34%	55.17%	57.84%	59.55%	4,554
Restricted Sample - Only First Two Years						
Unweighted	33.36%					3,546
Weighted	**56.00%**					3,546
- entitlement >€450	48.72%					2,513

Notes: The table shows the non-take-up rates for the sample of eligible students. We use different numbers for semesters beyond the standard period of study since students enrolled in 2020 and 2021 received up to 4 additional semesters for eligibility time due to COVID. We calculate eligibility excluding and including the students' own income and wealth. Non-take-up rates are shown using the full sample and the restricted sample, using all observations or only students in the first two years, as the additional semesters due to COVID do not apply to them. We show unweighted, weighted, and weighted non-take-up-rates only using students with an entitlement above €450 as they are very likely eligible irrespective of any simulation errors. Weights are calculated as described in Appendix B.1.3. All students beyond their standard period of study who participated in our survey in 2023 received 4 additional semesters as they started in 2020 or 2021, respectively. Therefore, the unweighted non-take-up rate is only calculated for 4 additional semesters. Bold non-take-up rates are the ones reported in the main text. We use the weighted rate with 3 additional semesters in line with the eligibility rates reported by the German government (Deutscher Bundestag, 2023).

TABLE B.2: Summary Statistics by Take-Up and Eligibility Beliefs (Full Sample)

	Non-Eligible	Eligible Non-Receivers			Receivers
	All (N=4,216)	All (N=4,587)	Non-Believers (N=3,697)	Believers (N=650)	All (N=7,101)
Sociodem. Background (=1)					
Age (in years)	22.94	24.63	24.61	24.22	24.39
Female	0.62	0.63	0.65	0.59	0.67
Studies in East Germany	0.18	0.18	0.18	0.21	0.27
Knows student aid receiver	0.47	0.42	0.43	0.42	0.67
Economic Background (in €)					
Student aid entitlement	0.00	498.69	465.15	614.28	649.91
Income from work	335.70	437.01	453.07	377.18	241.14
Support from parents	577.90	397.92	424.69	284.00	126.17
Total income	1,050.24	1,021.85	1,061.77	880.15	1,136.26
Average Misperception (in €)					
Student aid amount	-258.59	-260.45	-275.17	-194.71	-248.20
Income thresh. for parents	-15,508.07	-18,643.19	-17,798.78	-20,084.01	-12,497.93
Repayment amount	3,074.40	2,930.09	2,898.63	2,753.76	1,330.56

Notes: The table shows the summary statistics for the full sample without migration background and educational background of parents, as explained in Section 3.3.3. Column 1 shows students who are not eligible for student based on our calculated entitlement. Column 2 shows students who do not take up student aid but are eligible for it. Columns 3 and 4 further separate this group into students who do not believe they are eligible (Non-Believers) and who believe they are eligible (Believers). Summary statistics for student aid receivers are presented in column 5. The entitlements are calculated by our microsimulation for columns 1-4. The actual student aid amount is used for column 5. Based on the groups sizes, we find that 39.2% of eligible students do not take up student aid. Additionally, 80.6% of the non-take-up group does not believe to be eligible for student aid.

TABLE B.3: Comparison of Summary Statistics: Restricted and Full Sample

	Non-Eligible		Non-Take-Up & Eligible						Receivers	
	All		All		Non-Believers		Believers		All	
	Full Sample	Diff. to Restricted	Full Sample	Diff. to Restricted	Full Sample	Diff. to Restricted	Full Sample	Diff. to Restricted	Full Sample	Diff. to Restricted
Economic Background (in €)										
Student aid entitlement	0.00	0.00	498.69	-6.86	465.15	-3.06	614.28	-14.53	649.91	0.96
Income from work	335.7	-3.83	437.01	-5.89	453.07	-9.97	377.18	-8.90	241.14	-0.03
Support from parents	577.90	2.21	397.92	7.98	424.69	6.49	284.00	26.40	126.17	-0.54
Total income	1050.24	-2.34	1021.85	-1.82	1061.77	-10.06	880.15	20.88	1136.26	6.86
Sociodemographic Background (=1)										
Lives with parents	0.21	-0.01	0.14	0.00	0.12	0.01	0.25	-0.02	0.11	-0.01**
Studies in East Germany	0.18	-0.01	0.18	-0.01	0.18	-0.01	0.21	-0.02	0.27	0.01
Additional Variables										
Age	22.94	0.13**	24.63	0.02	24.61	0.00	24.22	-0.13	24.39	0.13*
Female (=1)	0.62	-0.02*	0.63	0.01	0.65	0.01	0.59	0.00	0.67	0.00
Siblings	0.66	-0.01	1.11	0.00	1.13	0.01	1.02	-0.01	1.10	-0.05**
Semester	4.69	0.07	4.66	0.08	4.78	0.07	4.16	0.11	4.86	0.02
Knows student aid receiver (=1)	0.47	0.01	0.42	0.00	0.43	-0.01	0.42	0.05	0.67	-0.01
Prior vocational training (=1)	0.06	0.00	0.14	0.01	0.13	0.01	0.15	0.00	0.16	0.01
Receives other scholarship (=1)	0.06	0.01	0.07	0.00	0.07	0.00	0.10	0.00	0.02	0.00
Debt aversion (0-10)	8.30	0.12**	8.31	0.13***	8.33	0.14***	8.23	0.07	8.05	-0.07**
Average Misperception (in €)										
Student aid amount	-258.59	4.69	-260.45	-5.10	-275.17	-6.03	-194.71	6.77	-248.20	0.84
Income thresholds for parents	-15508.07	1351.34**	-18643.19	1709.49***	-17798.78	1270.84**	-20084.01	4077.98**	-12497.93	1198.37**
Repayment amount	3074.40	-92.43	2930.09	-7.63	2898.63	-0.80	2753.76	41.71	1330.56	-36.57
Observations	4,216	2,263	4,587	2,409	3,697	1,979	650	318	7,101	4256

Notes: The table shows summary statistics of the full sample and differences to the restricted sample used for the main analysis, as explained in Section 3.3.3. We look at the same groups as in Tables 3.1 and B.2: non-eligible students, eligible students who do not take up student aid (all, only those who believe to be eligible, only those who do not believe to be eligible), and student aid receivers. Significance stars are based on two-sided t-tests of the means with unequal variances.
*p<0.1; **p<0.05; ***p<0.01

TABLE B.4: Representativeness of Non-Receivers Sample

	Non-Receivers Sample (N=4,672)		Representative Data (N=113,119)		Diff. t-test
Variable	Mean	SD	Mean	SD	p-value
Age	23.883	3.439	23.270	3.467	0.000
Female (=1)	0.619	0.486	0.514	0.500	0.000
Total income	1033.528	500.022	885.658	975.581	0.000
Migration Background (=1)	0.202	0.402	0.191	0.393	0.055
Single (=1)	0.973	0.162	0.948	0.220	0.000
Study year	2.455	1.374	2.882	1.739	0.000
Lives with parents (=1)	0.171	0.377	0.333	0.471	0.000
Studies in East Germany (=1)	0.171	0.377	0.185	0.388	0.017

Notes: The table shows the summary statistics of the non-receivers from the restricted sample and from representative data for students in Germany in 2021 (Becker et al., 2024). The non-receivers from the representative data were selected the same way as the restricted sample by only including students that are institutionally eligible, as explained in Section 3.3.2. The last column shows the p-value corresponding to two-sided t-tests of the means of each group, respectively.

TABLE B.5: Representativeness of Receivers Sample

	Receivers Sample (N=4,256)		Representative Data (N=11,601)		Diff. t-test
Variable	Mean	SD	Mean	SD	p-value
Age	24.523	3.800	24.382	3.652	0.034
Female (=1)	0.664	0.472	0.539	0.498	0.000
Total income	1143.116	401.843	960.611	561.682	0.000
Migration Background (=1)	0.297	0.457	0.261	0.439	0.000
Single (=1)	0.974	0.159	0.955	0.206	0.000
Study year	2.510	1.350	3.226	1.803	0.000
Lives with parents (=1)	0.094	0.292	0.161	0.367	0.000
Studies in East Germany (=1)	0.279	0.448	0.267	0.442	0.142

Notes: The table shows the summary statistics of the receivers from the restricted sample and from representative data for students in Germany in 2021 (Becker et al., 2024). For both samples, only students who receive a positive amount of student aid are selected. The last column shows the p-value corresponding to two-sided t-tests of the means of each group, respectively.

TABLE B.6: Determinants of Take-Up and Belief to be Eligible (Full Sample)

	Believer (=1)	Take-Up (=1)		
Comparison Group:	Non-Believers (1)	Non-Receivers (2)	Non-Believers (3)	Believers (4)
Sociodemographic Background				
Age (standardized)	-0.052***	-0.089***	-0.093***	-0.001
	(0.006)	(0.007)	(0.007)	(0.003)
Female (=1)	-0.008	0.026**	0.020	0.010*
	(0.011)	(0.012)	(0.012)	(0.006)
Studies in East Germany (=1)	0.029**	0.088***	0.093***	0.007
	(0.014)	(0.013)	(0.012)	(0.005)
Knows student aid receiver (=1)	0.016	0.197***	0.185***	0.045***
	(0.010)	(0.011)	(0.011)	(0.006)
Economic Background (in €100)				
Student aid entitlement	0.023***	0.025***	0.028***	-0.002**
	(0.002)	(0.002)	(0.002)	(0.001)
Support from parents	-0.007***	-0.083***	-0.079***	-0.014***
	(0.002)	(0.003)	(0.003)	(0.001)
Misperceptions (in %)				
Absolute underestimation	-0.112***	-0.074***	-0.118***	0.035***
student aid amount	(0.021)	(0.023)	(0.023)	(0.010)
Absolute underestimation	0.007	-0.142***	-0.127***	-0.028**
income thresholds for parents	(0.025)	(0.028)	(0.027)	(0.012)
Absolute overestimation	0.013*	-0.151***	-0.120***	-0.043***
repayment amount	(0.008)	(0.014)	(0.014)	(0.006)
Observations	4,584	11,688	10,798	7,751
Pseudo R^2	0.130	0.357	0.379	0.260

Notes: The table shows the marginal effects from probit regressions on the dependent variables students' own belief about their aid eligibility (column 1) and take-up (columns 2-4) for the full sample. *Believer* equals 1 for students who believe they are eligible. *Take-Up* equals 1 for all students who receive student aid. The comparison group for each regression is shown below the dependent variable. In column 1, we compare all eligible believers with all eligible non-believers. In columns 2-4, we compare all receivers to eligible non-receivers. We differentiate between all eligible non-receivers (column 2), eligible students who believe to be ineligible (column 3), and eligible students who believe to be eligible (column 4). Marginal effects >0 relate to belief/take-up, and marginal effects <0 relate to non-belief/non-take-up, respectively. We include study field and university fixed effects, and dummies for leaving the online survey during the misperception elicitation, indicating parents' income thresholds ≤€10,000 in both respective misperception elicitation questions, and no confidence level for parents' income. Robust standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

TABLE B.7: Misperceptions at the Barriers of Non-Take-Up (Full Sample)

	Misperceptions of Student Aid Terms (in %)			
	Eligibility Conditions	Student Aid Amount	Income Thresh. for Parents	Repayment Amount
	(1)	(2)	(3)	(4)
Eligible non-believers (=1)	0.043***	0.083***	-0.006	-0.086**
	(0.009)	(0.011)	(0.009)	(0.042)
Receivers (=1)	0.007	0.030***	-0.037***	-0.208***
	(0.009)	(0.011)	(0.009)	(0.041)
Mean Misperception Elig. Believers	0.558	0.594	0.477	0.692
Observations	11,688	11,688	11,688	11,688
R^2	0.218	0.086	0.371	0.100
F Statistic	37.09***	12.77***	360.96***	24.50***

Notes: The table presents OLS regression results on misperceptions, measured as the percentage deviation from the correct answer in the hypothetical scenarios, using the full sample. Column 1 pools misperceptions about eligibility conditions from the first two scenarios, while columns 2 and 3 separate them into misperceptions about student aid amounts (first scenario) and income thresholds for parents (second scenario). Column 4 covers misperceptions about repayment amounts (third scenario). Coefficients represent eligible non-receivers (=1) and student aid receivers (=1) who underestimate eligibility conditions or overestimate repayment. The reference group is the eligible believers, whose average underestimation (overestimation in column 4) is shown in the first row below the coefficients. Positive coefficients indicate a higher underestimation of student aid in percentage points. We include study field and university fixed effects, and dummies for leaving the online survey during the misperception elicitation, indicating parents' income thresholds ≤€10,000 in both respective misperception elicitation questions, and no confidence level for parents' income. Robust standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

TABLE B.8: Reasons for Non-Take-Up by Eligibility Belief (Full Sample)

	Non-Believers (N=3,697)		Believers (N=650)		Diff. t-test
Reason (=1)	Mean	SD	Mean	SD	p-val.
Unawareness of Eligibility					
I realized parents' income is too high	0.587	0.492	0.123	0.329	0.000
Parents said their income is too high	0.578	0.494	0.177	0.382	0.000
Spouse's income too high	0.039	0.194	0.040	0.196	0.904
Own income too high	0.251	0.433	0.195	0.397	0.002
Own assets too high	0.488	0.500	0.208	0.406	0.000
Active Decision against Take-Up					
Do not want to be seen as a student aid receiver	0.038	0.190	0.106	0.308	0.000
Do not want to get money from the state	0.088	0.283	0.174	0.379	0.000
Do not want to incur debt	0.440	0.496	0.617	0.487	0.000
Application effort too high	0.442	0.497	0.618	0.486	0.000
Family situation too complex	0.150	0.357	0.243	0.429	0.000
Expected funding amount too low	0.268	0.443	0.340	0.474	0.000
Other					
Sufficient support from parents	0.635	0.482	0.432	0.496	0.000
Do not want to disclose income information	0.120	0.324	0.171	0.377	0.000
Cannot provide certificate of performance	0.114	0.318	0.174	0.379	0.000

Notes: The table shows the fraction of how many students indicated which reasons for not applying for student aid in the full sample. The reasons are measured on a 5-point Likert scale. The fractions show how many students stated that a specific reason "applies" or "rather applies" to them. The fractions are separately reported for eligible students who do not believe they are eligible and those who do. The last column shows p-values from two-sided t-tests between the means per group, respectively.

B.4 Extended Tables

TABLE B.9: Determinants of Take-Up and Belief to be Eligible (extended)

	Believer (=1)	Take-Up (=1)		
Comparison Group:	Non-Believers (1)	Non-Receivers (2)	Non-Believers (3)	Believers (4)
Age (standardized)	-0.054***	-0.070***	-0.072***	0.005
	(0.008)	(0.009)	(0.009)	(0.003)
Female (=1)	-0.026*	0.001	-0.010	0.010*
	(0.015)	(0.016)	(0.015)	(0.006)
Studies in East Germany (=1)	0.018	0.106***	0.099***	0.012**
	(0.018)	(0.016)	(0.015)	(0.005)
Knows student aid receiver (=1)	0.049***	0.174***	0.171***	0.019***
	(0.014)	(0.014)	(0.014)	(0.006)
Born outside Germany (=1)	0.078**	-0.274***	-0.184***	-0.122***
	(0.033)	(0.032)	(0.037)	(0.025)
Parents born outside Germany (=1)	0.051**	0.101***	0.105***	0.005
	(0.021)	(0.019)	(0.017)	(0.006)
First generation academic (=1)	0.027*	0.102***	0.092***	0.019***
	(0.015)	(0.017)	(0.016)	(0.006)
Both parents college degree (=1)	-0.015	-0.029	-0.045**	0.007
	(0.016)	(0.021)	(0.020)	(0.006)
Student aid entitlement	0.017***	0.019***	0.020***	-0.002**
	(0.003)	(0.003)	(0.003)	(0.001)
Support from parents	-0.004**	-0.078***	-0.071***	-0.012***
	(0.002)	(0.004)	(0.003)	(0.001)
Absolute underestimation	-0.083***	-0.093***	-0.125***	0.020*
student aid amount	(0.027)	(0.030)	(0.029)	(0.010)
Absolute underestimation	-0.043	-0.132***	-0.130***	-0.008
income thresholds for parents	(0.031)	(0.037)	(0.035)	(0.012)
Absolute overestimation	-0.008	-0.111***	-0.096***	-0.018***
repayment amount	(0.011)	(0.019)	(0.019)	(0.006)
Lives with parents (=1)	0.107***	-0.186***	-0.137***	-0.096***
	(0.027)	(0.025)	(0.026)	(0.019)
Siblings	-0.003	-0.005	-0.008	0.002
	(0.006)	(0.007)	(0.006)	(0.002)
Semester (standardized)	0.002	0.006	-0.001	0.003
	(0.007)	(0.007)	(0.007)	(0.003)
Prior vocational training (=1)	0.023	0.016	0.018	-0.011
	(0.020)	(0.022)	(0.021)	(0.010)
Receives other scholarship (=1)	0.035	-0.340***	-0.245***	-0.189***
	(0.026)	(0.039)	(0.045)	(0.042)
Debt aversion (Scale 0-10)	-0.000	-0.029***	-0.025***	-0.005***
	(0.003)	(0.004)	(0.004)	(0.001)
Patience (Scale 0-10)	-0.001	-0.006*	-0.006*	-0.001
	(0.003)	(0.003)	(0.003)	(0.001)
Impulsiveness (Scale 0-10)	0.002	0.002	0.002	-0.000
	(0.003)	(0.003)	(0.003)	(0.001)
Observations	2,409	6,665	6,235	4,574
Pseudo R^2	0.162	0.391	0.414	0.297

Notes: Continued on next page.

TABLE B.9: Determinants of Take-Up and Belief to be Eligible (extended)

	Believer (=1)	Take-Up (=1)		
Comparison Group:	Non-Believers (1)	Non-Receivers (2)	Non-Believers (3)	Believers (4)
Conf. misp. student aid amount	-0.033	0.263***	0.218***	0.064***
	(0.041)	(0.049)	(0.047)	(0.017)
Conf. misp. parents' income	0.096**	-0.237***	-0.188***	-0.072***
	(0.045)	(0.051)	(0.049)	(0.017)
Conf. misp. repayment	0.014	0.621***	0.561***	0.100***
	(0.029)	(0.037)	(0.035)	(0.012)
Conf. parents' income	0.032	-0.020	0.000	-0.006
	(0.031)	(0.035)	(0.034)	(0.012)
Observations	2,409	6,665	6,235	4,574
Pseudo R^2	0.162	0.391	0.414	0.297

Notes: The table shows the all marginal effects from the probit regression as reported in 3.2. The dependent variables are students' own belief about their aid eligibility (column 1) and take-up (columns 2-4). *Believer* equals 1 for students who believe they are eligible. *Take-Up* equals 1 for all students who receive student aid. The comparison group for each regression is shown below the dependent variable. Marginal effects >0 relate to belief/take-up, and marginal effects <0 relate to non-belief/non-take-up, respectively. We include study field and university fixed effects, and dummies for leaving the online survey during the misperception elicitation, indicating parents' income thresholds ≤€10,000 in both respective misperception elicitation questions, and no confidence level for parents' income. Robust standard errors are in parentheses.

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

TABLE B.10: Misperceptions at the Barriers of Non-Take-Up (extended)

	Misperceptions of Student Aid Terms (in %)			
	Eligibility Condi- tions	Student Aid Amount	Income Thresh. for Parents	Repay- ment Amount
	(1)	(2)	(3)	(4)
Receivers (=1)	0.010	0.027*	-0.009	-0.132***
	(0.013)	(0.016)	(0.013)	(0.040)
Eligible non-believers (=1)	0.053***	0.090***	0.023*	-0.004
	(0.013)	(0.016)	(0.013)	(0.044)
Eligible do not know (=1)	0.001	0.006	0.023	0.278*
	(0.024)	(0.030)	(0.025)	(0.159)
Receivers (=1) X	-0.114**	-0.183**		
Overest. student aid amount (=1)	(0.049)	(0.073)		
Eligible non-believers (=1) X	-0.096*	-0.164**		
Overest. student aid amount (=1)	(0.051)	(0.075)		
Eligible do not know (=1) X	-0.101*	-0.075		
Overest. student aid amount (=1)	(0.061)	(0.092)		
Receivers (=1) X	0.016		-0.026	
Overest. income thresh. parents (=1)	(0.030)		(0.033)	
Eligible non-believers (=1) X	0.008		-0.020	
Overest. income thresh. parents (=1)	(0.031)		(0.036)	
Eligible do not know (=1) X	0.071		-0.124**	
Overest. income thresh. parents (=1)	(0.051)		(0.062)	
Receivers (=1) X				-0.018
Underestimated repayment (=1)				(0.054)
Eligible non-believers (=1) X				-0.076
Underestimated repayment (=1)				(0.058)
Eligible do not know (=1) X				-0.238
Underestimated repayment (=1)				(0.178)
Overest. student aid amount (=1)	0.048	-0.000		
	(0.048)	(0.071)		
Overest. income thresh. parents (=1)	-0.107***		0.009	
	(0.028)		(0.032)	
Underestimated repayment (=1)				-0.222***
				(0.053)
Conf. misp. student aid amount	0.236***	0.252***		
	(0.015)	(0.016)		
Conf. misp. income thresh. parents	-0.110***		0.001	
	(0.016)		(0.012)	
Conf. misp. repayment				-0.309***
				(0.034)
Mean Misperception Elig. Believers	0.536	0.582	0.434	0.629
Observations	6,665	6,665	6,665	6,665
R^2	0.230	0.111	0.336	0.120
F Statistic	21.11***	10.54***	170.50***	14.28***

Notes: Continued on next page.

TABLE B.10: Misperceptions at the Barriers of Non-Take-Up (extended)

	Misperceptions of Student Aid Terms (in %)			
	Eligibility Condi- tions (1)	Student Aid Amount (2)	Income Thresh. for Parents (3)	Repay- ment Amount (4)
Age (standardized)	0.002	0.007	-0.004	0.027*
	(0.003)	(0.005)	(0.003)	(0.015)
Female (=1)	0.005	0.010	0.002	-0.008
	(0.006)	(0.008)	(0.005)	(0.017)
Lives with parents (=1)	0.009	0.001	0.022**	0.010
	(0.008)	(0.012)	(0.009)	(0.021)
Siblings	-0.006***	-0.008***	-0.003	0.005
	(0.002)	(0.003)	(0.002)	(0.011)
Studies in East Germany (=1)	-0.086	-0.043	-0.109**	-0.004
	(0.055)	(0.067)	(0.052)	(0.129)
Knows student aid receiver (=1)	-0.007	-0.006	-0.012**	-0.048***
	(0.005)	(0.007)	(0.005)	(0.014)
Semester (standardized)	0.001	0.005	-0.002	-0.009
	(0.002)	(0.004)	(0.003)	(0.008)
Student aid entitlement	0.003***	0.003**	0.004***	0.006**
	(0.001)	(0.001)	(0.001)	(0.003)
Support from parents	-0.000	-0.000	-0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.003)
Prior vocational training (=1)	-0.001	0.006	-0.003	0.018
	(0.007)	(0.010)	(0.008)	(0.027)
Receives other scholarship (=1)	0.013	0.033	-0.029**	-0.054
	(0.015)	(0.023)	(0.011)	(0.035)
Born outside Germany (=1)	-0.007	-0.039**	0.043***	0.208***
	(0.011)	(0.016)	(0.012)	(0.041)
Parents born outside Germany (=1)	0.017**	0.019*	0.018***	0.007
	(0.007)	(0.010)	(0.007)	(0.017)
First generation academic (=1)	0.004	0.002	0.009	-0.010
	(0.006)	(0.009)	(0.006)	(0.017)
Both parents college degree (=1)	-0.002	-0.004	0.007	-0.016
	(0.007)	(0.011)	(0.008)	(0.024)
Conf. parents' income	-0.043***	-0.050*	-0.038***	0.050
	(0.017)	(0.027)	(0.013)	(0.033)
Mean Misperception Elig. Believers	0.536	0.582	0.434	0.629
Observations	6,665	6,665	6,665	6,665
R^2	0.230	0.111	0.336	0.120
F Statistic	21.11***	10.54***	170.50***	14.28***

Notes: Continued on next page.

TABLE B.10: Misperceptions at the Barriers of Non-Take-Up (extended)

	Misperceptions of Student Aid Terms (in %)			
	Eligibility Conditions (1)	Student Aid Amount (2)	Income Thresh. for Parents (3)	Repayment Amount (4)
Debt aversion (Scale 0-10)	-0.000	-0.001	0.001	-0.006
	(0.001)	(0.002)	(0.001)	(0.004)
Patience (Scale 0-10)	-0.002**	-0.003**	-0.002	0.001
	(0.001)	(0.002)	(0.001)	(0.004)
Impulsiveness (Scale 0-10)	0.001	0.000	0.002**	0.005
	(0.001)	(0.002)	(0.001)	(0.004)
Mean Misperception Elig. Believers	0.536	0.582	0.434	0.629
Observations	6,665	6,665	6,665	6,665
R^2	0.230	0.111	0.336	0.120
F Statistic	21.11***	10.54***	170.50***	14.28***

Notes: The table shows the all coefficients from the OLS regression as reported in 3.3. The dependent variable is misperceptions, measured as the percentage deviation from the correct answer in the hypothetical scenarios. Coefficients represent eligible non-receivers (=1) and student aid receivers (=1) who underestimate eligibility conditions or overestimate repayment. The reference group is the eligible believers, whose average underestimation (overestimation in column 4) is shown in the first row below the coefficients. Positive coefficients indicate a higher underestimation of student aid in percentage points. We include study field and university fixed effects, and dummies for leaving the online survey during the misperception elicitation, indicating parents' income thresholds ≤€10,000 in both respective misperception elicitation questions, and no confidence level for parents' income. Robust standard errors are in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Appendix C

Appendix to Chapter 4

C.1 Additional Technical Explanations

C.1.1 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, 2021) 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[1] 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[2] of the first study field group is assigned to treatment while the second cohort[3] 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.

C.1.2 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

[1]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.

[2]Students in the first and second semester.

[3]Students in the third and fourth semester.

indicator for being a half-orphan, an indicator for believing that parents are relatively poor, migration background[4], potential civil servant status of parents and parents' educational background[5]. 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.

C.1.3 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**[6]: An individual's outcome is not affected by the treatment assigned to others.
   As explained in Section 4.4.2 and shown in Table C.13, I do not find any evidence for spillovers of the treatment, so the SUTVA holds.

[4]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 outside of Germany.

[5]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.

[6]Stable unit treatment value assumption.

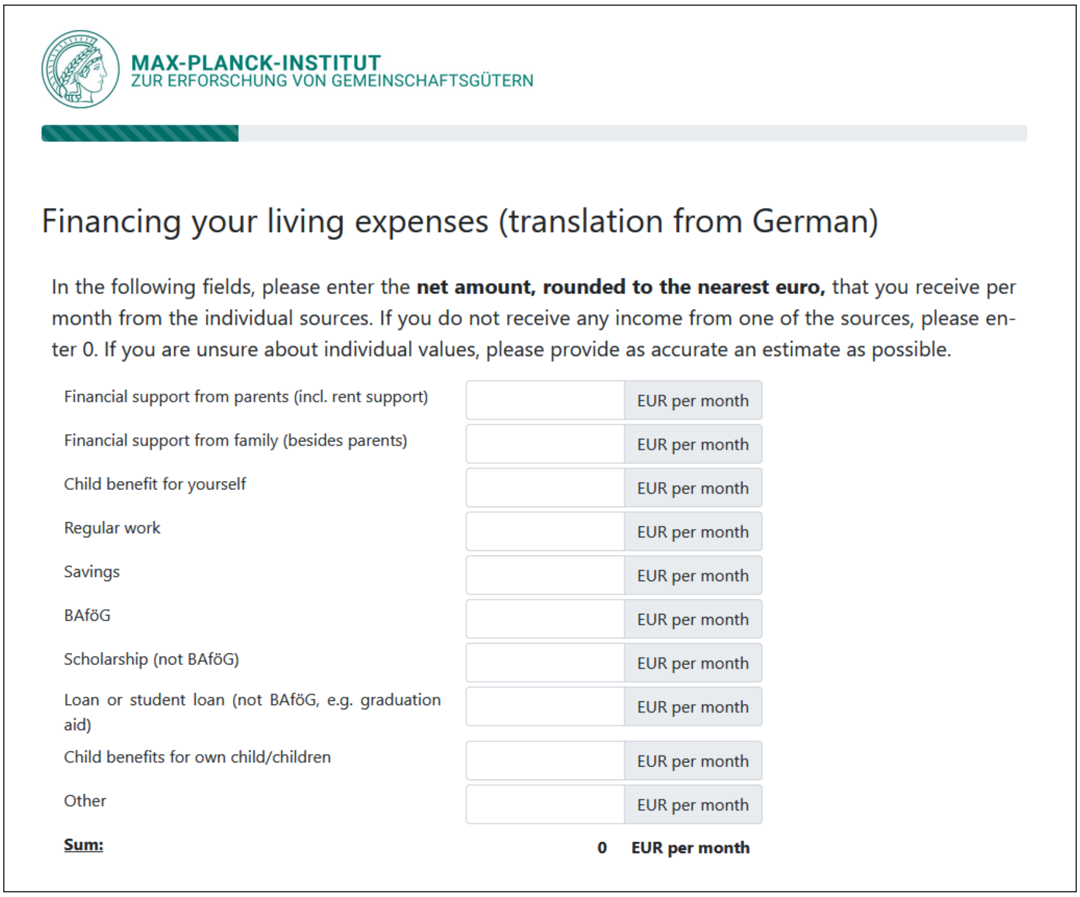
2. **Independence**: Random assignment of the treatment.
   The information intervention was stratified, and the control and treatment group are balanced, shown in Table 4.1.

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 4.2 and 4.3, 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.

C.2 Survey Screenshots



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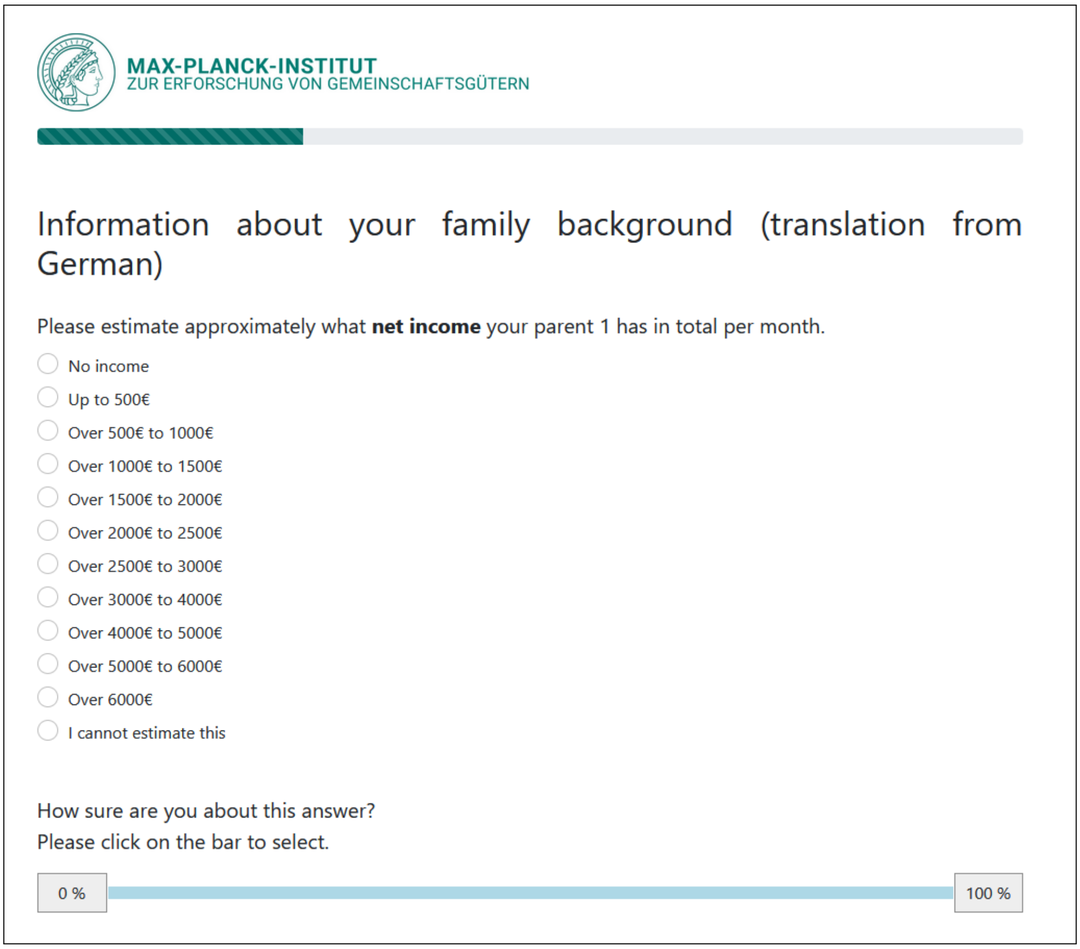
Financing your living expenses (translation from German)

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 C.1: Question on Student's Income per Month.



MAX-PLANCK-INSTITUT
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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 C.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 C.3: Question on Reasons against Applying for Student Aid.

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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 C.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 C.5: Screenshot of the Information Intervention - Page 2.

C.3 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.

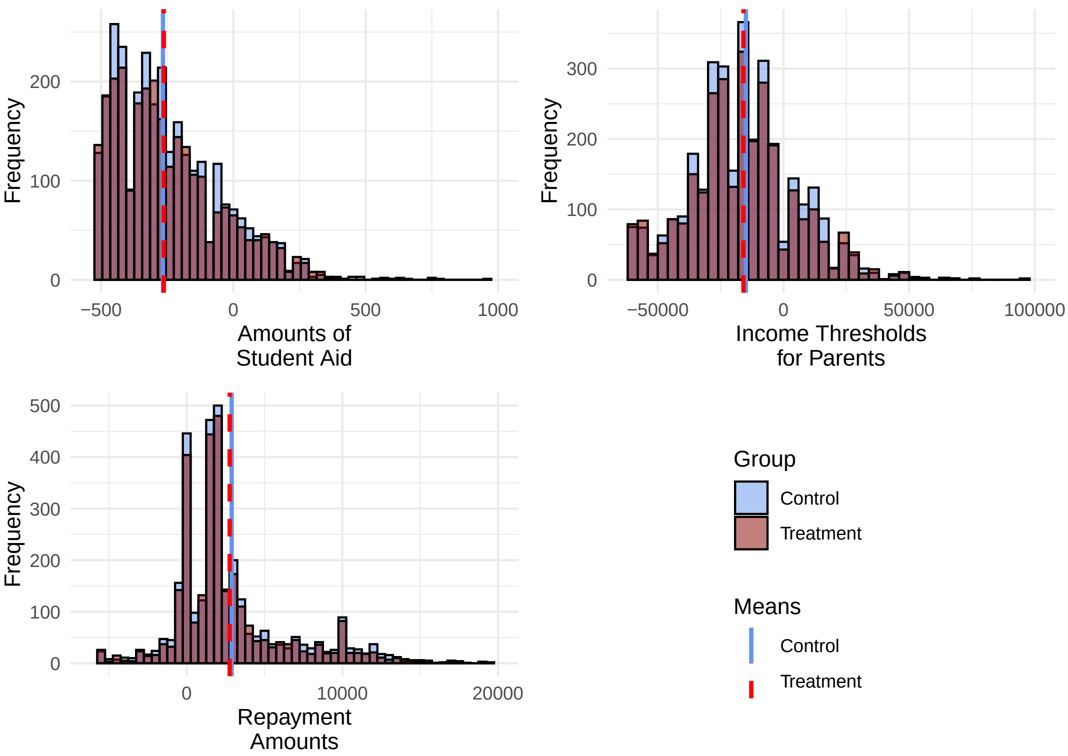

FIGURE C.6: Distribution of Average Misperceptions per Area.

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	Repay-ment 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)	(0.047)	(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 4.3.2. 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 4.3, 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	Repay-ment Amounts	Pooled Domains	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 (Control Group Underest.)	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 4.3.2. 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 4.3, 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 (Intensive)

	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 4.3, 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 4.3, 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 in W1 (in %)	−0.007	−0.007	−0.003	−0.003
	(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 in W1 (in %)	0.008	0.008	0.006	0.006
	(0.006)	(0.007)	(0.006)	(0.006)
Confidence Misp. Income Thresholds for Parents	0.001	0.004	−0.007	−0.003
	(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	−0.0003	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)
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 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)
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)
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)
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: 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)
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 4.3, 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 4.3, 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 Take-Up

	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 applied. 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 4.3, 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	0.080
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	0.040	0.053	0.080
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 Low Quintiles SES (=1)	0.035			0.020
	(0.022)			(0.024)
Monthly Income (in %)		−0.041***		−0.040***
		(0.011)		(0.011)
Intervention X Low Quintiles Income (=1)		0.040**		0.033*
		(0.018)		(0.018)
Reasons: Parents' Income (Index)			−0.017***	−0.014***
			(0.004)	(0.004)
Intervention X Low Quintiles Reasons: P. Income (=1)			0.031	0.021
			(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	0.060	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	0.030	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)	(0.052)	(0.110)	(0.121)	(0.061)	(0.063)
Wave 2 (=1)	0.043***	0.061***	0.148***	0.269***	−0.039***	−0.049***
	(0.008)	(0.010)	(0.020)	(0.029)	(0.011)	(0.012)
Wave 3 (=1)	0.127***	0.161***	0.350***	0.551***	−0.022	−0.033**
	(0.011)	(0.012)	(0.027)	(0.038)	(0.014)	(0.014)
Take-Up (=1) X	0.080*	0.101**	−0.139*	−0.206**	−0.081*	−0.093*
Wave 2 (=1)	(0.046)	(0.051)	(0.083)	(0.094)	(0.049)	(0.052)
Take-Up (=1) X	0.121**	0.195***	−0.446***	−0.449***	−0.158***	−0.171***
Wave 3 (=1)	(0.053)	(0.054)	(0.124)	(0.122)	(0.052)	(0.055)
Reference Income in €	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 1 for the respective period, and *Take-Up* is 1 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

C.4 Probit Regressions on Take-Up

TABLE C.21: 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 4.3, 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.22: 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 4.3, 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.23: 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 4.3, 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.24: 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 %)	12.903**	12.511**	8.559***	9.554***	7.769***	8.577***
	(5.039)	(4.886)	(3.275)	(3.655)	(2.772)	(3.060)
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 4.4 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 4.3, 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 C.25: 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 instead of OLS. I control for all sociodemographic and other control variables mentioned in Section 4.3, 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.26: 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 4.5 using Probit estimation 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 C.27: 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 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 C.28: 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 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 C.29: 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 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 C.30: 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 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 C.31: 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 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

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