The Effects of Student Aid - Evidence from Germany

Master's Thesis
in Economics (Science Track)

by

Fynn Lohre
(Student number h12232560)

Supervisor: Univ. Prof. Dr. Martin Halla
Submission date: 12.08.2024

For the newest version check
https://github.com/VARFynn/University_Contributions/tree/main/01_Master/01_Masterthesis/01_Masterthesis.pdf

Abstract

This thesis uses German panel data (SOEP; 2002–2019) to investigate the effect of BAfoeG, an example for a means-tested student aid within a corporatist welfare state, on *Enrollment*, *Labor Supply*, and *Non-Take-Up* decisions.

Using a fixed-effects model, the calculated mechanical BAfoeG variation, BAfoeG max. rate and the variation in time of the real value, a positive effect of 1.1–1.3 pp on enrollment is indicated for a 10 % increase in the real max. rate. The enrollment reaction seems to be more elastic for younger individuals. However, a null-effect of individual changes rather than the max. rate cannot be falsified. Further, the effect of max. rate seems to rather affect estimated non-recipients than recipients, which could be driven by a misperception of BAfoeG as well as a negative externality of increasing market tightness. These results, which are rather correlational than causal, are not very robust, e.g. for changes in the time period.

In an IV setup with the mechanical BAfoeG variation as instrument for the simulated BAfoeG it is shown that the inverse relationship of potential BAfoeG and non-take-up is not as pronounced as anticipated (-1.29 pp for 100 € increase in simulated BAfoeG). This suggests that either misperception about the BAfoeG reception exists over the full income distribution or, more general, that the low BAfoeG recipients are not the driving force, as it was anticipated before.

Using a similar setup for labor supply, it is also visible that the negative incentive effects of more student aid both on the extensive margin and intensive margin are much smaller than in pooled regression, again highlighting the endogeneity issue of observed data. Hence, if one assumes this difference is purely driven by simultaneity, one could argue for a relaxation of BAfoeG's own income constraint. Still, the estimated negative effect for students is higher than previous estimated general effects within a RCT.

DISCLAIMER: *AI (ChatGPT & DeepL) was only used in the following ways: (1) to identify grammatical and typographical errors (ex-post; chapter wise, AI identifies weaknesses & I manually correct), and (2) to suggest synonyms for in my view not-fitting words or phrases (while writing; only sentence fragments chosen by myself).*

Acknowledgment

[Will be added for printed bib-version after grading]

Contents

List of Tables

List of Figures

1 Introduction

Increasing skill premia, demographic challenges and positive productivity effects – expenditures on education remain as critical drivers from the states' points of view to address not only today's but also tomorrow's challenges. This also holds for tertiary education and state-funded student aid, with a high number of students relying on financial support from the state. Yet, the design of student aid varies a lot between different welfare states, and empirical evaluations remain rare, leaving the distinct effects as well as mechanisms of student aid policies to some extend unquantified.

How does means-tested student aid affect (i) the decision to enroll, (ii) the decision to take up student aid, and (iii) labor supply during studies? Generally, means-testing relies on screening to target financial aid towards those who meet specific eligibility criteria, ensuring that resources are allocated to those in need. However, it also depends on opt-in principles and deductions, raising questions about potential negative incentive effects. With respect to means-tested student aid, particularly Germany's BAfoeG, similar considerations arise. One would expect that – given a nuanced screening – the enrollment decision displays a high elasticity related to the student aid. On the other hand, more financial means due to higher student aid could lead to less incentive to supply labor during the studies. Further, are there reasons to not take up student aid, e.g. socio-economic induced barriers?

Focusing on a few precise mechanisms of student aid and one policy instruments, namely the financial amount of student aid, is hereby in line with the literature. As stated by Joensen and Mattana (2024), complex eligibility, the variety of programs and the unobserved counterfactual otherwise make it hard to causally identify any effects (see Deming and Dynarski, 2010 for further challenges). The variety of programs is implicitly stemming from the variety in welfare state types explained by Esping-Andersen (1990), with means-tested student aid resembling the corporatist, central European welfare state. Consequently, it must be distinguished from the Nordic welfare state, which adopts a more universal approach by viewing educational spending as an active form of redistribution (Andersen, 2008, 2018). Similarly, it differs from the Anglo-Saxon welfare state model, in which student funding relies more heavily on individual debt but, as a consequence, has lower income taxes (as seen implicitly in Black et al., 2023; Joensen and Mattana, 2024; Lochner and Monge-Naranjo, 2012). Thus, it has to be differentiated between the broad and the precise level of literature.

On the broad level of literature, short term effects can be shown on four areas, "college enrollment, persistence, performance, and graduation" (Joensen and Mattana, 2024, p.5). A positive effect on college enrollment is visible in both the Nordic (Nielsen et al., 2010) and the Anglo-Saxon welfare states (Deming and Dynarski, 2009), with the latter effect being larger (1.35 pp vs. 2-5 pp for a 1000 $ increase; see Mattana, 2018 for a review and discussion). Further, it has been shown that dropouts seem to decrease with increasing student aid (Anglo-Saxon: see Bettinger et al., 2004 or Dynarski, 2003; Nordics: see Arendt, 2013) and students seem to live in better neighborhoods ex-post (Bettinger et al., 2019; Scott-Clayton and Zafar, 2016). Yet, one has to be aware that most of these studies rely on specific reform changes suitable for a regression discontinuity design, such as the 1988 reform in Denmark. While this approach provides a strong basis for causal identification, it also raises concerns about the validity of these effects in today's environment.

Looking precisely at Germany, however, the picture is not as clear. On the one hand, no such (quasi-)experimental setup is possible, leading to mixed results. Baumgartner and Steiner (2005) is the only paper attempting a quasi-experimental approach for the 1990 reform, which could be interpreted as showing a non-effect (as e.g. Mattana, 2018 interprets it). However, it rather reflects the effect of a shift in the grant-loan ratio without direct (short-term) consequences on the students' budget constraint. In contrast, Lauer (2002) finds a positive effect (0.8 pp for 1000 DM, which is about 500 €). Nonetheless, as correctly stated by Baumgartner and Steiner (2005), the discrete choice model might be biased due to endogeneity caused by the inclusion of BAfoeG indicator variables. Yet, the only existing literature that attempts to validly overcome the endogeneity problem is the microsimulation literature, which simulates counterfactual BAfoeG values. Notable studies include Herber and Kalinowski (2019) and Steiner and Wrohlich (2012), with the latter finding positive effects on enrollment of similar magnitude as Nielsen et al. (2010) in Denmark. Herber and Kalinowski (2019) analyze the issue of non-take-up due to the opt-in design and identify potential reasons.

Another issue in Germany is the high complexity of BAfoeG itself. The calculation of BAfoeG is governed by the respective law, the Bundesausbildungsförderungsgesetz (BAföG[1]), and involves the following calculation algorithm, which is presented in a very simplified form[2]. Firstly, the student's own net income and assets are assessed, followed by the income of their spouse or parents (see §§ 21-24 BAföG). The total amount of support is determined by subtracting these contributions from the BAfoeG max. rate (see § 13 BAföG), which varies depending on the type of educational institution and the student's living situation. Additionally, §§ 12 and 14 BAföG outline allowances and deductions for necessary expenses, e.g. health-insurance. In theory, 50 % of the student aid is a grant and 50 % a loan. However, specific allowances are a 100 % grant and the repayment is capped to 10,000 €. Hence, the loan- & grant share can differ.

The value added of this thesis is the contribution to the aforementioned microsimulation literature by simulating counterfactual BAfoeG based on the full calculation algorithm rather than an approximation, as e.g. done by Baumgartner and Steiner (2005). Utilizing the latest version of the German SOEP, it is possible to include more years (2002–2019) while also employing a very inclusive data handling approach similar to Glocker (2011). Given the complexity of BAfoeG, which acts as a gate-keeper for this kind of literature, newer papers like Fidan and Manger (2022) still relied on the data prepared by Herber and Kalinowski (2019). Furthermore, this thesis will use the simulated BAfoeG, i.e., based on observables, as well as the calculated mechanical BAfoeG variation, inspired by the labor-supply literature (Kleven and Schultz, 2014), which excludes changes due to the endogenous choice of labor supply.

The thesis is structured as follows: First, Data and Descriptive Statistics are presented, focusing on the SOEP specific data handling. Second, the Empirical Framework is introduced. Following this, the Results are presented, leading into the Discussion and Limitations. The latter addresses potential treatment heterogeneity and includes robustness checks. Finally, the thesis concludes with a Summary and Concluding Remarks.

[1]In this thesis: "BAföG" refers to the law as source, while "BAfoeG" refers to the student aid.

[2]There are very specific rules, such as how distinct income types are considered or standard deductions for social contributions. These rules are clearly outlined in the respective sections in the BAföG and, although not covered in this description for the sake of simplicity, they are included in the calculations. The most important aspect to understand is that any own income above the threshold is fully deducted (negative incentive effect), while for parental (in period t-2) or spouse income above the threshold, only a share is deducted from the individual's max. rate (no incentive for behavioral changes).

2 Data and Descriptive Statistics

In order to identify the effect of Germany's state funded student aid, BAfoeG, this thesis relies primarily on the German Socio Economic Panel (SOEP, 2023) enhanced by inflation- (Destatis, 2024b) as well as average wage-data (Destatis, 2024a) by the Federal Statistical Office of Germany. The SOEP is a longitudinal survey beginning with 1984, which involves nearly 15,000 households and around 30,000 individuals (Goebel et al., 2019). As explained in Goebel et al. (2019), random probability samples are used, with general population samples being drawn through a nationwide two-stage stratified sampling procedure and panel attrition being addressed by the incorporation of refreshment samples. Thus, it can be considered representative with respect to Germany.

The main contribution of this thesis is the micro-simulation of the student aid (BAfoeG) received to address the underlying endogeneity issue of observational data allowing a shot for a causal identification (similar to Herber and Kalinowski, 2019). Based on the underlying SOEP variables, it is possible to estimate a *Simulated BAfoeG*, i.e., BAfoeG determined by observable characteristics if an individual were to apply for it. Furthermore, it is possible to estimate a *Mechanical BAfoeG*, i.e., BAfoeG if an individual were to apply for it without deducting their own income. While this distinction may seem trivial, Figure 1 shows the differences over time. It is visible that, on average, more than 20 % of students actually receive BAfoeG (black line). However, the sim. BAfoeG (orange line) shows that, on average, more than 30 % could receive BAfoeG if they were to apply. Hence, the difference between the two lines is purely *Non-Take-Up*. The mech. BAfoeG (blue line) shows that, on average, more than 40 % could receive BAfoeG if they did not work. Hence, the difference between these lines highlights the unconsidered endogeneity in basic correlational studies that use only the observed amount of BAfoeG one is receiving (see Figure 13 in Appendix for visualization of decision nodes).

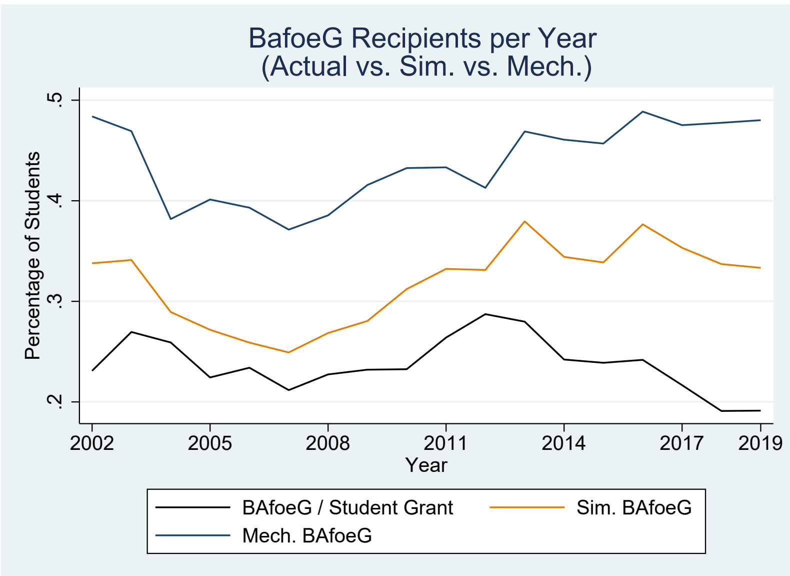

Figure 1: BAfoeG Recipients per Year; Actual vs. Simulated vs. Mechanical BAfoeG

Yet, this calculation is based on the specific data handling of the SOEP and differs from that of Herber and Kalinowski (2019). While Herber and Kalinowski (2019) adopted a very restrictive approach regarding the student sample, this thesis includes as many student observations as possible through imputations and an inclusive student definition (similar to the definition in Glocker, 2011).[3] The aim of this inclusive approach is to maintain the representativeness of the SOEP dataset, as restrictive criteria could selectively exclude non-traditional educational paths. Even though, the overview provided by Table 10 (Appendix) covers the creation of the core variables needed to replicate this thesis, it is necessary to discuss data decisions with potential identification impact.

Arguably, the most influential data decision in this thesis is to drop individuals who lack any information about their parents as this is needed for the BAfoeG calculation. Specifically, individuals are retained as long as there is information on at least one parent. A prominent example is only the mother is included and the father's information is missing (Father's SOEP ID missing and not answered the question with respect to status of his/her father). In those cases, the father is treated as dead with respect to BAfoeG calculations but the individual is kept. Additionally, individuals are kept if both parents are dead but the IDs are missing in the SOEP. While this decision could introduce a bias, the descriptive statistics (see Figure 15, Appendix) shows no indication of such. There exists a minor difference in income and BAfoeG, which is within the range of one standard deviation. This difference is mostly driven by the age difference, since the likelihood of the parents not being in the SOEP increases by design in age. Thus, the considered students should tilt more towards, what in public debates is considered "true students".

The definition of a student follows Glocker (2011). A person is considered a student if they are enrolled for at least one month of the semester at a German University. Technically, this means that the binary-string education variable (pab0013) is decoded to form the basis of the student identifier. Subsequently, this identifier is corrected based on eligibility to study, age, and university type. It is reasonable to assume that students' weekly working hours are capped at 80, making it impossible to work more than 40 hours alongside full-time studies. Therefore, students reporting working hours beyond this limit are disregarded. Additionally, the top 5 % of income earners are excluded to account for full-time workers who are merely enrolled in studies. As Table 1 shows, still some high earning students are included, which, however, seems reasonable.

For BAfoeG, the parental and spouse income from two years before studying is relevant, whereas for the student's own income, the current year's income is considered. Technically, this means the sum of all positive income (§ 2 EStG). Therefore, all income streams are considered, as listed in Table 10 (Appendix). If the parental main income is missing in the specific wave, it is either imputed if possible.[4] Otherwise, the income from one year prior or the current income is taken in this order. Income is therefore income in the sense of BAföG (see §21 IV BAföG) and excludes: BAfoeG itself, alimony, unemployment benefit II, housing benefits, parental allowances, maternity benefits, social assistance and loans.

[3]Restricting the data sample based on the approach by Herber and Kalinowski (2019) yields similar student and BAfoeG recipient numbers, which validates the BAfoeG calculation and general data cleaning. This verification was conducted as part of the Fraunhofer FIT project work to verify my model and not by myself.

[4]The labor supply and wage are imputed using a Heckman correction, which was already integrated into a Fraunhofer FIT labor supply model. It estimates four different equations (male/female, west/east) and includes the following variables: age, age^2, educational levels, time since apprenticeship, time since apprenticeship^2 divided by 100, years of unemployment or out of the labor force, labor market experience full time, labor market experience full time^2 divided by 100, labor market experience part time, labor market experience part time^2 divided by 100, degree of disability, degree of disability^2 divided by 100, children under 3 years old, children under 6 years old, children under 16 years old, state in Germany, and the tax scheme in the respective year. Strong outliers in hourly wages are removed by excluding the upper and lower 2 % from the estimation.

Based on this BAföG relevant gross income, a standard social deduction is calculated (percentage based on work type according to § 21 II BAföG 1–4). Further, based on the so-called "Programmablaufplan" (BMF, 2024), it is possible to manually estimate income tax, church tax, and the solidarity surcharge ("Soli"). Thus, the gross income minus the standard social deduction and taxes results in the BAföG relevant net income.

However, it is important to note that this calculation does not account for income outside of Germany (see § 21 Abs. 2a BAföG) and is conditional on the optimal behavior assumption. It is required to assume that individuals behave optimal e.g. with respect to their taxation. An example is the choice of tax class in marriage. In case of missing tax classes, it must be assumed that households minimize the taxes during the year. However, as Buettner et al. (2019) show, in practice this might not always hold true. Additionally, specific tax breaks that can be interchanged between years (i.e. "Verlustvortrag") cannot be considered. The same applies to income that is tax-free under the so-called Trainer's or Volunteer Allowance. Furthermore, the SOEP does not adequately capture wealth, and, hence, it cannot be considered in calculations.[5] Additionally, it is not accounted for the specific handling of the "Riester-Rente".

After the data cleaning, the dataset contains 12,589 student observations from 3,824 distinct students (see Table 1). While Herber and Kalinowski (2019), using a restrictive data approach, report 986 student observations classified as non-take-up or take-up (2002-2013), this thesis identifies 133 % more students in the same subsample, totaling 2,327. However, the main characteristics are roughly the same. While some differences between Table 1 and Herber and Kalinowski (2019) can be attributed to time trends (e.g., the share of males or migration background), the differences in *household with parents* and *labor supply* (hrs.) arise from the distinct data decisions. As the SOEP variable for household does by design not purely reflect the actual living place for a student, it is corrected as accurately as possible (e.g. by the question where the parents live). Consequently, this thesis reports a share of roughly 43 % of students living at home, which aligns more closely with the roughly 20-30 % reported in questionnaires such as Seegers et al. (2023). While most of the differences in labor hours can be attributed to variations in reporting (with averages taken over all student observations), a small difference remains. This is driven by their strict exclusion criteria, which exclude more non-traditional career paths.

The BAfoeG numbers of approx. 24 % are slightly higher than official reportings (Federal Government of Germany, 2023). The main reason is that the SOEP question in some waves included merit-based students grants. However, the share of merit-based study grants remains constant with student numbers (see indirectly BMBF, 2016) such that the bias should also be constant. The recipient decline after 2014 is equally visible in both official reporting and this thesis (see Table 9, Appendix). Furthermore, students above the age of 35 and some students who are fully in the labor force are excluded. Thus, the slightly higher share is in line with official reports. Additionally, the approximate 32 % share in sim. BAfoeG aligns with the only similar estimation by Herber and Kalinowski (2019). However, one must be aware that it is not possible to exclude students based on wealth and the regular student time per degree[6] based on the SOEP. Hence, simulations are potentially overestimated and rely on the assumption that this bias remains constant.

[5]Although, in theory, one could try to estimate wealth through rental and capital income, this approach is problematic. In practice, it results in an unreasonably high number of BAfoeG recipients being simulated with no BAfoeG, as these forms of income correlate not only with the amount of wealth but also with its forms, risk preferences, and the potential for inter-temporal shifting.

[6]It is only possible to correct for clear cases that exceed the regular time for all possible degrees.

Table 1: Summary Statistics Students (2002-2019)

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Individual Characteristics:					
Age	12589	23.5436	3.2963	17	35
Male	12589	.4799	.4996	0	1
HH with Parents	12589	.4273	.4947	0	1
Migration Background	12589	.2273	.4191	0	1
First Gen	12589	.3395	.4736	0	1
Academic:					
University	12589	.7283	.4448	0	1
Polytechnic	12589	.2332	.4229	0	1
Work Academy	12589	.0274	.1633	0	1
Year in University	12589	3.3196	2.3323	1	16
Income:					
Inc. Student (€, Gross, Mth.)	12589	506.285	690.0756	0	3000
Labor Supply	12589	.4902	.4999	0	1
Labor Supply (Weekly Hours)	6171	20.3635	12.6687	1	40
Inc. Father (€, Gross, Mth.)	12589	4068.4187	5151.7772	0	92318
Inc. Mother (€, Gross, Mth.)	12589	1785.1596	2277.5304	0	72000
BAfoeG:					
Student Grants (SOEP)	12589	.2361	.4247	0	1
Student Grants (€, Mth.)	2151	430.0014	236.917	10	3000
BAfoeG (Simulated)	12589	.3220	.4673	0	1
BAfoeG (Simulated, €, Mth.)	4054	358.6306	206.7908	10	2599
BAfoeG (Mechanical)	12589	.4418	.4966	0	1
BAfoeG (Mechanical, €, Mth.)	5562	388.0062	210.0179	10	2599
Eligible for BAfoeG	12589	.8989	.3015	0	1

Note: This includes every individual who studied for at least one semester in the respective year. Consequently, individuals appearing in j study years influence the mean with a weighting of j/n. Student Grants include state funding (BAfoeG) as well as other forms of stipends. BAfoeG amounts are calculated under the assumption of optimal behavior and always assume an application is made if the amount is greater than zero. Therefore, these figures may be higher than the actual amounts received.

To access the quality of simulation and the non-take-up-rates, this thesis follows Herber and Kalinowski (2019), who themselves follow Bargain et al. (2012). The non-take-up rate (NTU) can be expressed as share of simulated eligible (SE) and reported recipients (BAF),

$$NTU = \frac{SE - (BAF|SE)}{SE} = \frac{\overline{BAF}|SE}{SE}. \tag{1}$$

The quality of the simulation can – to some extend – be evaluated over the beta error rate,

$$\beta = \frac{BAF|\overline{SE}}{BAF}, \tag{2}$$

which captures the share of falsely simulated students. Yet, as briefly mentioned by Herber and Kalinowski (2019), a low but non-zero beta error rate does not imply that the simulation is incorrect. Rather, it reflects the complexity of means-testing combined with the absence of specific variables (e.g. subject data) and potential measurement errors (e.g. false reportings). Consequently, as also done by the respective literature, the lower bound non-take-up rate(NTU_{LB}) can be expressed as

$$NTU_{LB} = \frac{(\overline{BAF}|SE)}{SE + (BAF|\overline{SE})}, \tag{3}$$

while the NTU itself can be considered the upper bound.

Given the data decision and the extended sample, the beta error rate is with approx. 20 % significantly higher than the approx. 8 % in Herber and Kalinowski (2019), which is the main disadvantage of the inclusive student definition and imputations. However, Herber and Kalinowski (2019) additionally decided to be less restrictive in their BAfoeG calculations, for close deviations as they started with β of approx. 16 %, which is in line with this thesis' results (see their Table 11, p. 57).

Yet, as visible in Figure 2, the non-take-up rate itself (black lines) ranges from roughly 40–55 % in 2004 to 55–65 % in 2018. While such high numbers may initially seem incorrect, they are only slightly higher than general estimates for social assistance non-take-up in Germany, which range from 34–43 % (Bruckmeier et al., 2013), with the main differences occurring from 2015 onward. Furthermore, from the estimated mech. BAfoeG (yellow) the potential dark number of non-take-up cases is visible, which had not been covered in literature before, peaking at roughly 65–75 % in 2018. This group includes students who potentially work and, thus, do not take up BAfoeG (by choice) because it would potentially not be sufficient. It is further visible in Figure 3 that these differences are consistent for the first and second parental income quartiles, with the non-take-up rate being slightly higher for the second income quartile. This could indicate that they either do not know about BAfoeG or do not need it. The significant differences in the third quartile primarily stem from the small sample size in this group, combined with the fact that it is very rare to receive BAfoeG in this income range (only in very specific cases, such as families with many children).

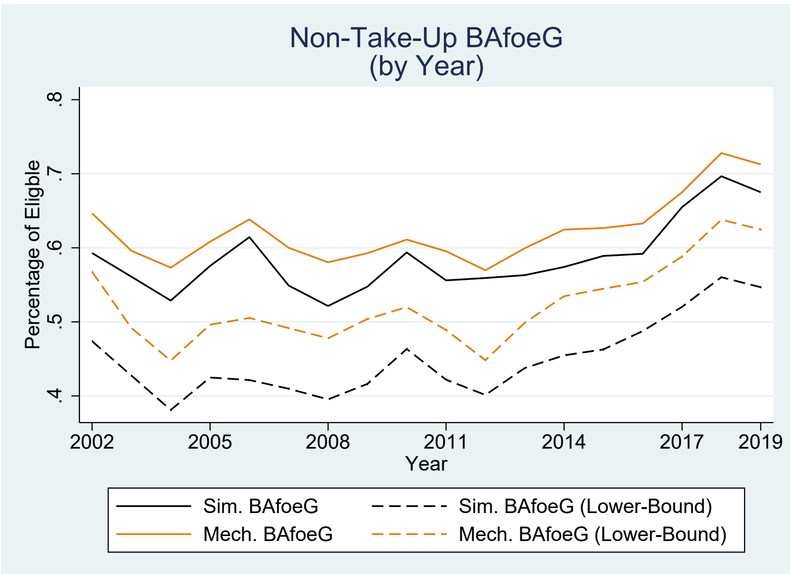

Figure 2: Non-Take-Up by Year (Upper vs. Lower Bound & Sim. vs. Mech. BAfoeG)

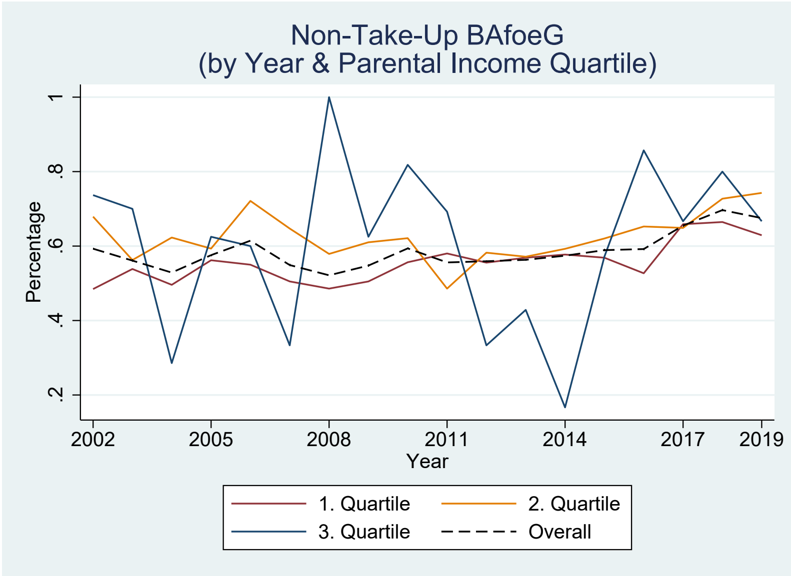

Figure 3: Non-Take-Up by Year & Parental Income Quartile (Upper Bound)

The sub-sample considered for the enrollment decision includes all individuals under 35 years old who have the respective qualifying high school degree, are not students, and do not have any University degree (n = 25,458; see summary Table 8, Appendix), together with the first-year student observations (n = 3,353). Consequently, the decision to enroll only captures the first tertiary degree (usually Bachelor). This sub-sample now considers 9,081 different individuals and e.g. indicates parental income differences between students and non-students. However, as visible in Figure 4, the summary statistics Table 8 (Appendix) are skewed to the right since students who start studying tend to start relatively earlier (approximately 20+ % for age < 22) and, hence, leave the panel. Nonetheless, small differences between subgroups like first-generation students exist (also visible in Figure 14, showing the variation over time).

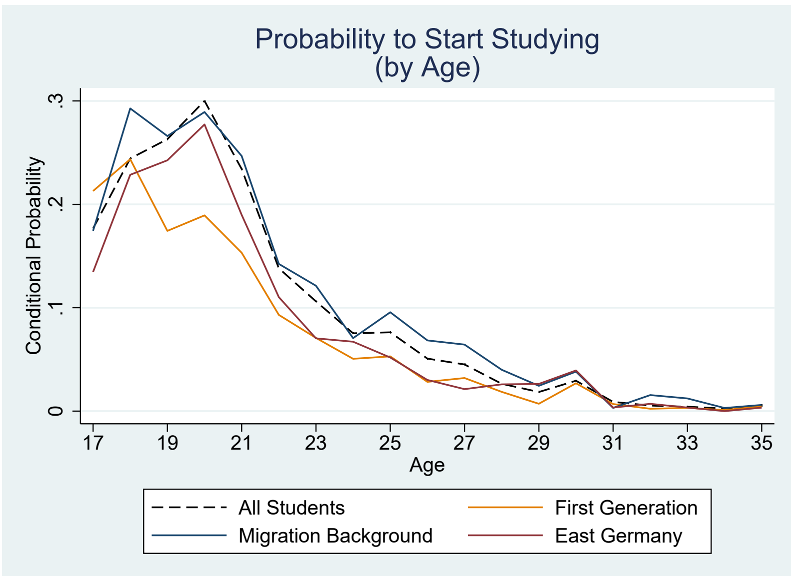

Figure 4: Probability to Enroll by Age & Subgroup

3 Empirical Framework

The main empirical setting used in this thesis is the variation in real BAfoeG over time. As shown in Figure 5, Germany experienced several changes in the nominal value of BAfoeG from 2002 to 2019 (see the change of further parameters in Figure 10 ff., Appendix). As a consequence, this leads due to changing price levels to constant variation in real BAfoeG. This setting, by design, assumes that inflation is orthogonal to students' behavior. Given that only pre-COVID data with € as currency is included and inflation (mostly) follows the same gradient in this period, this assumption appears reasonable.

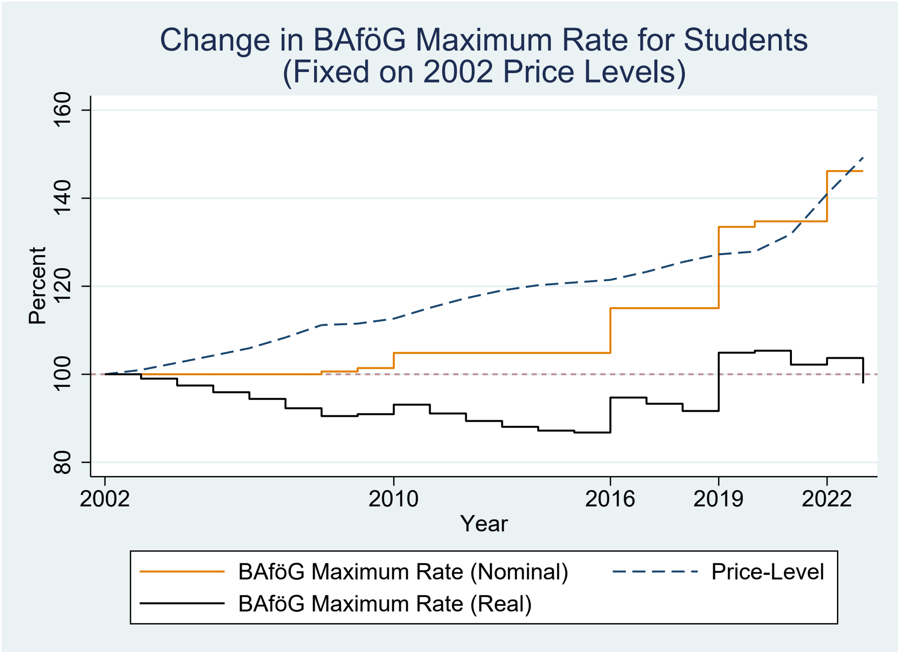

Figure 5: Change of BAfoeG Maximum Rate in Time

As introduced before, there exist two streams of exogenous variation for real BAfoeG values. While Figure 5 captures the real variation in the max. rate, it should be noted that the absolute value differs depending on the living situation, with students living at home having a smaller max. rate.[7] Changes in the max. rate impact mech., sim., and actual BAfoeG values by altering the base amount before deductions. Two effects are possible: first, a full pass-through effect when BAfoeG remains significantly above zero, and second, a negligible effect when BAfoeG drops to zero post-deductions for the specific student. Thus, mech. BAfoeG captures more variation, as it includes the changes due to deductions and allowances, which otherwise is not captured. Simulated/actual BAfoeG would reflect the full variation, however, including the observed variation from the endogenous labor supply decision. Hence, it cannot be used as exogenous variation.

[7] The term "maximal BAfoeG" differs slightly from its use in political discourse, where it assumes a student who does not live at home and must cover insurance costs, thus receiving additional allowances (typically applicable to students 25 years and older). Moreover, BAfoeG is not strictly capped, as flat allowances for children can increase the support available, making it technically unbounded. Therefore, "maximum rate" refers to BAfoeG before any income deductions and allowances are applied.

3.1 Enrollment (FE Estimation)

To determine the effect on enrollment, both the real variation in the maximum BAfoeG rate and the mech. BAfoeG are considered. The maximum rate BAfoeG is directly visible, while the mech. BAfoeG is now being made observable through the simulation excluding the labor supply choice. This is especially important as non-students are also within the considered subset, reporting a higher wage as a consequence of not studying.

In a standard approach, one would begin with a (naive) pooled pre-regression, estimating the conditional probability of student i enrolling at a given point t in the following form:

$$PR_{it}(\text{Enroll} \mid \text{Eligble}) = \beta_0 + \beta_1 \times \text{BAfoeG Max Rate}_t + W'\zeta + \varepsilon_{it}. \quad (4)$$

This typically includes socio-economic controls ($W'\zeta$) and β_1 is the (causal) effect of interest capturing the variation in maximum BAfoeG rate. Since it is included as a rate, it does not differ between i and only varies with t. This variation could demonstrate the perceived change in BAfoeG by students, for example, through media announcements.

Yet, this setup could still suffer from endogeneity if $W'\zeta$ does not capture all con-founders. One example of a con-founder, which is not directly observable and, thus, not implementable, is the students' general talent or preferences. In order to capture this unobserved heterogeneity a linear probability model (LPM) is estimated including individual fixed effects (μ_i),

$$PR_{it}(\text{Enroll} \mid \text{Eligble}) = \beta_0 + \beta_1 \times \text{BAfoeG Max Rate}_t + W'\zeta + \mu_i + \varepsilon_{it}. \quad (5)$$

As visible in Table 11 (Appendix), the estimates for the marginal effect of interest do not differ between a probit and a LPM, allowing, under a slight abuse of Kolmogorov's axioms, the use of the LPM and, thus, the inclusion of fixed effects. However, the inclusion of time fixed effects to capture general time trends is not possible as it would collide with the yearly variation.

This thesis includes a similar approach. However, through the estimated mech. BAfoeG it is possible to extent the standard approach. By incorporating a "treatment identifier" s,

$$PR_{it}(\text{Enroll} \mid \text{Eligble}) = \beta_0 + \beta_1 \times \text{BAfoeG Max Rate}_t \times s_{it} + W'\zeta + \mu_i + \varepsilon_{it}, \quad (6)$$

it is possible to identify the group who might react to announced changes in BAfoeG. This identifier turns one, if the estimated mech. BAfoeG is larger than zero, i.e. they would receive (some) BAfoeG if they do not work.

Further, in a fixed-effects model it is also possible to directly include the mech. BAfoeG as it captures the actual differences one would encounter assuming they do not work. This can be written as

$$PR_{it}(\text{Enroll} \mid \text{Eligble}) = \beta_0 + \beta_1 \times \text{Mech. BAfoeG}_{it} + W'\zeta + \mu_i + \varepsilon_{ist}. \quad (7)$$

In all variants of the fixed effects models, it is essential to control for time-variant factors. This includes the self-reported gross income in period $t-1$ as a proxy for the outside option, the time since an apprenticeship, and living situation (living at home with parents or not). In some specification parental log. income and health status are added.

3.2 Non-Take-Up & Labor Supply (Instrumental Setup)

In case of non-take-up and labor supply, the primary source of endogeneity is the behavioral change due to the inter-linkage between non-take-up and labor supply itself (see Figure 13, Appendix for visualization). This effect is mediated through the simultaneous causal relationship between labor supply and BAfoeG. Specifically, if a student increases their labor supply, their BAfoeG decreases. Conversely, if a student receives less aid by design, they must substitute this with more labor.

To address this issue, this thesis relies on the instrumental approach by Herber and Kalinowski (2019) used explicitly for non-take-up and uses the mech. BAfoeG variation as the primary instrument (see Figure 6). However, there is no second instrument for "independent funding" employed, as independent funding is primarily linked to age and prior apprenticeships or jobs. Therefore, it does not provide purely exogenous variation due to its high correlation with social capital. As Herber and Kalinowski (2019) themselves state, they are the only study that uses the specific maximum rate[8] instead of the maximum amount of benefits normally used for other social assistance programs (e.g. Bruckmeier and Wiemers, 2012). Yet, this thesis additionally includes the variation in max. BAfoeG rates instead of the mech. BAfoeG to check for the robustness.

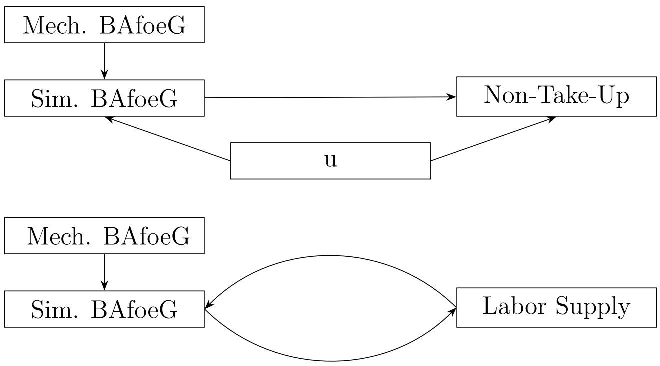

Figure 6: Instrumental Approach used for Labor Supply & Non-Take-Up

In order for mech. BAfoeG to be a valid instrument it must be relevant and exogenous. By design, mech. BAfoeG explains most of sim. BAfoeG's variation, i.e. $Cov(\text{Mech. BAfoeG, Sim. BAfoeG}) \neq 0$ & $Cov(\text{Mech. BAfoeG, Sim. BAfoeG}) >> 0$. Additionally, strong first stage effects are visible. Hence, mech. BAfoeG is relevant. To be exogenous, it must be orthogonal to the error term, i.e. $Cov(\text{Mech. BAfoeG, u}) = 0$. As most of the potential socio-economics factors are within the set of controls and mech. BAfoeG is constructed to contain the exogenous parts of variation, only wealth – both financially and culturally – remains as potential danger to exogeneity. While it seems unlikely that much within variation of Mech. BAfoeG can be attributed to sudden changes in (family) wealth, it is not possible to control for that due to lack of variables. Hence, it remains a risk, which should lead to different estimates when using the max. rate as a check-up. Still, mech. BAfoeG appears as exogenous, thus being a valid instrument.

[8] Herber and Kalinowski (2019) calculate individual maximum benefit amounts "by assuming that the student is not living with her [sic] parents and receives the maximum rent subsidy" (p.17) and then deduct the exogenous parts. This means what they refer to as "individual maximum benefits" is very similar to mech. BAfoeG, except for that living assumption. In this thesis, the actual living situation is considered, with an additional control in the regressions, as otherwise potential differences between the groups might be a biasing factor.

4 Results

This section includes the presentation of the key results, divided into the subtopics Enrollment, Non-Take-Up & Labor Supply.

4.1 Enrollment

The main argument for an effect on enrollment is that higher financial means, in the form of BAfoeG, enable certain groups to pursue studies that would otherwise be inaccessible. For others, it increases the economic incentive to study compared to the outside option of regular employment. As shown in Table 2, there is an indication of such a positive relationship between a higher BAfoeG maximum rate and the probability of enrollment.

Table 2: Enrollment: OLS/FE with Max. Rate BAfoeG

PR(Enrollment)	(1) OLS	(2) OLS	(3) FE	(4) FE	(5) FE	(6) FE	(7) FE
Real BAfoeG (Max. Rate)	-.0017***	.0001	.0010**	.0010**	.0011**	.0011**	.0013***
	(.0004)	(.0004)	(.0004)	(.0004)	(.0004)	(.0004)	(.0004)
Specification							
Set of Individual Controls		√					
Gross Income (Main Job, T-1)				√	√	√	√
HH with Parents				√	√	√	√
Apprenticeship (This Year)					√	√	√
Apprenticeship (Last Year)					√	√	√
Parental Log. Income						√	√
Health Status							√
Individual FE			√	√	√	√	√
N	28811	28056	28811	28811	28811	28811	26547
Cluster	9081	8726	9081	9081	9081	9081	8909
R^2	.0006	.2056	.0003	.0007	.0052	.0053	.0098
Prob > F	.0001	.0000	.0184	.0166	.0000	.0000	.0000

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table shows the estimated effect of Real BAfoeG (Max. Rate) on the probability to enroll. (1) is a baseline OLS estimation without controls, while (2) contains the set of individual controls: gross income (T-1), HH with parents, age, age^2, parental income, apprenticeship (last year; current year & general), first generation, migration background, siblings, partner, children, living in the city, sex, state and east Germany. (3)–(7) are the estimations in a fixed effects model including individual fixed effects.

While an initial OLS estimation, without any controls, even suggests a negative effect ($\hat{\beta} = -.0017$; $p < .01$), it is observable that this vanishes when adding potential controls on the individual level ($\hat{\beta} = .0001$; $p =$ ns). However, as this model doesn't adequately control for unobserved heterogeneity, the difference in the effect of interest to the FE estimations ($\hat{\beta} = .0010$; $p < .05$) is visible. Such an effect would suggest that an increase of the individual max. rate before any deductions of 1 % leads on average to an increase in the likelihood of enrollment by 0.1 pp.

This effect is statistically significant at the 5 % level in the initial specification without potential further controls. However, adding further controls, which capture within changes such as gross income and the living situation with parents ($\hat{\beta} = .0010$; $p < .05$), and whether the person did an apprenticeship this or last year to account for higher entry probability during this stage of life ($\hat{\beta} = .0011$; $p < .05$), does not significantly impact the precision of the marginal effect but rather improves the overall model and potential predictive performance. In (6), the parental log income is added, which does not lead to a change in β nor R^2, despite the additional optimization dimension. This provides a first indication that within changes in income of the parents might not influence the students' behavior.[9] Adding the health status significantly improves the overall model performance and also slightly raises the effect of max. rate ($\hat{\beta} = .0013$; $p < .01$). Yet, it remains unclear if this is driven by the dropped sample of 2,264 observations.

Economically this effect, which is rather correlational than causal, can also be considered significant. Assuming a student living not at home and a change of 1,000 €, which is approx. a 10 %. In this case, the model with FE would suggest an increase in enrollment probability of 1–1.3 pp. This lies in the range of the few previous estimates of 0.8 pp in Germany of Lauer (2002) and 1.35 pp in Denmark (Nielsen et al., 2010).

The micro-simulation approach used in this thesis offers an advantage: it allows to estimate the otherwise unobservable BAfoeG unconditional on the working decision (i.e. mech. BAfoeG). Hence, we can differentiate between potential receivers (mech. BAfoeG > 0) and non-receivers in both groups, students and non-students. Table 3 differentiates in this manner, while, additionally, distinguishing between age groups.

It is observable that in the model with FE estimation and all potential controls, the initial effect of 1.1 pp increases to 2.4 pp for individuals under 25 ($\hat{\beta} = .0024$; $p < .05$), 4.0 pp for those under 23 ($\hat{\beta} = .0040$; $p < .05$), and even 10.1 pp for those under 21 ($\hat{\beta} = .0101$; $p < .01$), given a 10 % change. On a broader level, this suggests that individuals are more elastic in their response to financial incentives right after finishing their education. This finding supports the path dependency argument, which indicates that once an individual's trajectory is set, it becomes more fixed and less likely to change (also indirectly visible in Figure 4). However, an increase of 10.1 pp seems unexpectedly high compared to the US estimates of 5 pp, which could be seen as upper bound estimates. One potential source of bias could be that some individuals considered by the FE estimation are only observed at a single point in time. This is possible because in Germany high school graduates are typically 17–20 years old.

In contrast, columns (4)–(7) display the differentiation for two subgroups: non-BAfoeG recipients (row 1) and BAfoeG recipients (row 1 + row 2). The positive effect on students' demand by the max. rate is observed primarily in the non-recipient group ($\hat{\beta} = .0035$; $p < .01$). However, this positive effect is negated for BAfoeG recipients, where the "added" effect is negative ($\hat{\beta} = -.0037$; $p < .01$). This (at first) counterintuitive result may be attributed to one of three possible explanations: a) model misspecification, b) negative externalities, or c) misperception of BAfoeG. The first issue, model misspecification, is straightforward. Second, negative externalities might arise if the maximum rate – as publicly discussed value – affects the application market. Increased market tightness could lead to more applications from both groups, potentially disadvantaging those in lower economic brackets if there is a correlation between economic status and grades. Finally, misperception about whether one receives BAfoeG and the amount could bias the distinction between the two sub-groups.

[9] This argument is important w.r.t. the use of mech. BAfoeG as an instrument.

Table 3: Enrollment: FE with Max. Rate and Specifications for Age & Treated

PR(Enrollment)	(1) FE	(2) FE	(3) FE	(4) FE	(5) FE	(6) FE	(7) FE
Real BAfoeG (Max. Rate)	.0024**	.0040***	.0101***	.0035***	.0053***	.0076***	.0127***
	(.0009)	(.0013)	(.0023)	(.0008)	(.0016)	(.0021)	(.0034)
Real BAfoeG (Max. Rate) × Simulated Treatment				-.0037***	-.0048***	-.006**	-.005
				(.0009)	(.0018)	(.0024)	(.0043)
Specification							
Age	< 25	< 23	< 21		< 25	< 23	< 21
Gross Income (Main Job, T-1)	√	√	√	√	√	√	√
HH with Parents	√	√	√	√	√	√	√
Apprenticeship (This Year)	√	√	√	√	√	√	√
Apprenticeship (Last Year)	√	√	√	√	√	√	√
Parental Log. Income	√	√	√	√	√	√	√
Individual FE	√	√	√	√	√	√	√
N	15100	11210	6060	28811	15100	11210	6060
Cluster	6656	5860	3943	9081	6656	5860	3943
R^2	.0063	.0089	.0290	.0064	.0072	.0102	.0297
Prob > F	.0000	.0000	.0000	.0000	.0000	.0000	.0000

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table shows the estimated effect of Real BAfoeG (Max. Rate) on the probability of enrollment in different specifications. (1)–(3) use the prior model specification with different age groups. (4)–(7) introduce a treatment identifier for mechanical BAfoeG > 0.

The misperception argument with respect to BAfoeG is further evident in Table 4. When using the mechanical variation instead of the communicated maximum rate of BAfoeG, the null effect cannot be falsified, with even a negative sign in most of the specification. Although the effect is so economically small that it is negligible. This suggests that actual individual monetary differences from changes in BAfoeG do not seem to drive the decision to enroll. This null effect persists across further specifications leading to the conclusion that it is not an anomaly. The most plausible explanation is that students are unaware of these individual differences between years.

To summarize, in the model with a FE estimation using BAfoeG max. rate a positive, both economically and statistical economic effect is visible on enrollment in range of 1–1.3 pp. Further specifications of this model indicate that this relationship is more pronounced for younger individuals and that it most prominently affects non-recipients. In the model with FE estimation using mech. BAfoeG, which can simulate for students and non-students at any time point, no effect of this actual within differences are visible. Yet, one has to be aware that in this setup the relationship is rather correlational than causal.

Table 4: Enrollment: OLS/FE with Mechanical BAfoeG

PR(Enrollment)	(1) OLS	(2) OLS	(3) FE	(4) FE	(5) FE	(6) FE	(7) FE
Real BAfoeG (Mechanical, in 100 €)	-.0177***	-.0002	-.0001	-.0006	-.0004	-0.0005	.0000
	(.0008)	(.0010)	(.0010)	(.0010)	(.0010)	(.0010)	(.0010)
Specification							
Set of Individual Controls		√					
Gross Income (Main Job, T-1)				√	√	√	√
HH with Parents				√	√	√	√
Apprenticeship (This Year)					√	√	√
Apprenticeship (Last Year)					√	√	√
Parental Log. Income						√	√
Health Status							√
Individual FE			√	√	√	√	√
N	28811	28056	28811	28811	28811	28811	26547
Cluster	9081	8726	9081	9081	9081	9081	8909
R^2	.0181	.2056	.0000	.0004	.0049	.0049	.0092
Prob > F	.0001	.0000	.9308	.1422	.0000	.0000	.0000

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table shows the estimated effect of Real BAfoeG (Mechanical, in 100 €) on the probability to enroll. (1) is a baseline OLS estimation without controls, while (2) contains the set of individual controls: gross income (T-1), HH with parents, age, age^2, parental income, apprenticeship (last year; current year & general), first generation, migration background, siblings, partner, children, living in the city, sex, state and east Germany. (3)–(7) are the estimations in a fixed effects model including individual fixed effects. Euros are expressed in 2002 euros, adjusted using the relevant price index.

4.2 Non-Take-Up

The main argument for non-take-up is that the BAfoeG amount and non-take-up are inversely related, i.e. students with lower BAfoeG amounts are more likely to not take up BAfoeG, either due to incomplete information or bureaucratic reasons (Herber and Kalinowski, 2019). As shown in Table 5, there is an indication of this inverse relationship. The estimated average marginal effect of a 100 € increase in simulated BAfoeG is a 3.18 pp decrease in the non-take-up probability in the baseline specification of a pooled probit estimation ($\hat{\beta} = .0318$; $p < .01$). After adding further controls, the average marginal effect in this pooled probit estimations suggests a decrease in the range of 2.54–2.74 pp, depending on the specification.

One should be aware that including parental income in the same regression as (sim.) BAfoeG carries a risk of multicollinearity, as sim. BAfoeG and parental income are closely related. BAfoeG is essentially a function of parental income and, thus, captures similar variations, though in an inverse form. This issue is visible in specifications (2)–(4). Although income was not included directly as a continuous variable, using income categories (parental income quartiles) instead of sim. BAfoeG indicates that the second ($\hat{\beta} = .1308$; $p < .05$) and third income quartiles ($\hat{\beta} = .1456$; $p = ns$) have a higher probability of non-take-up. The non-significance of the latter effect might be due to the small sample size. However, when sim. BAfoeG is included, both effects reverse, showing the shared variation problem. Since the average marginal effect (+0.0020) and the Pseudo-R^2 (+0.0001) increase only slightly, income is excluded from further specifications. This exclusion does not affect other average marginal effects, such as those for first-generation students or individuals with a migration background as visible through the specifications.

Table 5: Non-Take-Up: Pooled Probit & IV-Probit

	(1)		(2)		(3)		(4)		(5)	
PR(Non-Take-Up)	Probit	dydx	Probit	dydx	Probit	dydx	Probit	dydx	IV-Probit	dydx
Real BAfoeG (Simulated, in 100 €)	-.0839***	-.0318***	-.0698***	-.0254***			-.0752***	-.0274***	-.0338*	-.0129*
	(.0158)	(.0058)	(.0171)	(.0062)			(.0201)	(.0072)	(.0202)	(.0075)
2. Quartile Parental Income					.1308**	.0479**	-.0118	-.0043		
					(.0571)	(.0208)	(.0651)	(.0237)		
3. Quartile Parental Income					.1456	.0532	-.098	-.0359		
					(.1252)	(.0451)	(.1369)	(.0504)		
First Generation	-.1408**	-.0535**	-.2048***	-.0746***	-.2153***	-.0787***	-.2075***	-.0756***	-.2190***	-.0799***
	(.0602)	(.0227)	(.0611)	(.0221)	(.0614)	(.0223)	(.0616)	(.0223)	(.0612)	(.0226)
Migration Background	.0459	.0174	-.0953	-.0347	-.1120*	-.0410*	-.0955	-.0348	-.1112*	-.0404*
	(.0613)	(.0233)	(.0656)	(.0238)	(.0653)	(.0238)	(.0657)	(.0239)	(.0657)	(.0240)
Age (Centered at 23)			√		√		√		√	
Apprenticeship			√		√		√		√	
Sex			√		√		√		√	
Living in City			√		√		√		√	
Living with Parents			√		√		√		√	
East Germany			√		√		√		√	
Siblings			√		√		√		√	
Year (Dummies)	√		√		√		√		√	
N	4054		3930		3929		3929		3930	
Pseudo-R^2	.0201		.0573		.0535		.0574			
Wald Chi^2	66.6764		158.6455		150.1893		158.3297		144.68	
Baseline predicted probability	.5388		.5974		.5971		.5973			
Wald Test									15.31	
Prob. > Chi ^{2}									.0001	
Instrument (First Stage)									.0091***	
									(.0001)	

Clustered Std. Errors in Parentheses (on ID); * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table shows the effect of sim. BAfoeG (in 100 €) on non-take-up. The estimated mech. BAfoeG variation is used as an instrument. Regression (1) & (2) are baseline regressions with and without controls. (2)–(4) indicate the issue of including both parental income and BAfoeG. In (5), the instrument is used (IV probit). As visible, the marginal effect (dydx = AME) of interest is significantly smaller in absolute values. Euros are expressed in 2002 euros, adjusted using the relevant price index.

Through the first pooled probit specifications, the presented results for BAfoeG's effect, the baseline predicted probability, and the baseline effects of first generation and migration background are in line with Herber and Kalinowski (2019) and fairly similar. Their analysis is more nuanced with respect to potential covariates like risk preference or experience with BAfoeG's bureaucracy. However, using mech. BAfoeG as an instrument, and, thus, relying solely on exogenous variation, leads to a notable difference. While the average marginal effect of BAfoeG remains in the same range for Herber and Kalinowski (2019), it decreases in absolute value to -0.0129 ($p < 0.10$) here, which can barely be considered economically significant. This suggests a much weaker inverse relationship than anticipated, potentially driven by two factors. Either the strong negative relationship visible in the pooled probit estimations is driven by the labor supply decisions of recipients who receive smaller amounts, which are insufficient, or the misperception about BAfoeG reception affect not only the group around the cutoff but affects all groups.

4.3 Labor Supply

The main argument regarding labor supply is that higher BAfoeG incentivizes a decrease in labor supply, as all income streams of the students can be considered substitutes. However, as shown in Table 6, a negative incentive effect is observed across all specifications. In the pooled regression, the average marginal effect is similar in both OLS estimation within a LPM ($\hat{\beta} = -.0401$; $p < .01$) and probit estimation ($\hat{\beta} = -.0398$; $p < .01$), and slightly higher in absolute terms compared to the probit estimation without controls ($\hat{\beta} = -.0364$; $p < .01$). These correlational results suggest that for every 100 € increase in BAfoeG, students' labor supply decreases by approximately 4 pp. This can be considered economically significant since a 4 pp change for a general labor supply participation of 49 % on average reflects a change of roughly 8 %.

Yet, using mech. BAfoeG as an instrument in the LPM shows that the effect size is only 37.5 % ($\hat{\beta} = -.0150$; $p < .01$) or 44.8 % ($\hat{\beta} = -.0179$; $p < .01$) of the initial effect observed in the endogenous model. However, it still remains negative and statistically significant suggesting that a negative labor supply effect still exists despite accounting for the exogenous variation in BAfoeG. If the difference is attributed solely to simultaneity, it further suggests that relaxing the constraint of the student's own income could potentially weaken the negative incentive effects of increased means-tested student aid.

Table 6: Labor Supply – Extensive Margin: Pooled Probit, OLS & IV

	(1)		(2)		(3)	(4)	(5)
PR(Labor Supply)	Probit	dydx	Probit	dydx	OLS	IV	IV
Real BAfoeG (Simulated, in 100 €)	-.0926***	-.0364***	-.106***	-.0398***	-.0401***	-.0150***	-.0179***
	(.009)	(.0035)	(.0096)	(.0035)	(.0035)	(.0045)	(.0045)
First Generation			.0968***	.0363***	.0361***	.0107	.0101
			(.042)	(.0158)	(.0158)	(.0161)	(.0160)
Migration Background			-.0928**	-.0348**	-.0345**	-.0562***	-.0596***
			(.042)	(.0158)	(.0158)	(.0161)	(.016)
Apprenticeship			√		√	√	√
Age			√		√	√	√
Sex			√		√	√	√
Living in City			√		√	√	√
Living with Parents			√		√	√	√
East Germany			√		√	√	√
Siblings			√		√	√	√
Year (Dummies)							√
N	12589		12589		12589	12589	12589
Cluster	3824		3824		3824	3824	3824
Pseudo-R^2	.0126		.0551				
R^2					.0739	.0669	.0742
Wald Chi^2	105.5566		492.9869			431.93	539.87
RMSE					.4800	.4800	.4800
Baseline predicted probability	.4896		.4899		.5442	.524	.5178
Instrument (First Stage)						.0073***	.0074***
						(.0001)	(.0001)

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table shows the effect of sim. BAfoeG (in 100 €) on Labor Supply, Extensive Margin. (1) is a baseline probit, without any controls. (2) introduces the respective controls. (3) shows that, given the same controls, probit and LPM yield similar results. In the IV setup (2SLS), (4) & (5) show that the coefficient of interest is significantly smaller in absolute values. Euros are expressed in 2002 euros, adjusted using the relevant price index.

Looking onto Table 12 (Appendix), a similar picture is visible for the intensive margin, i.e. for those students who work. While the initial pooled specification suggest an effect between -1.4 hrs. ($p < .01$) -1.6 hrs. ($p < .01$) for a 100 € increase in BAfoeG, the IV setup with mech. BAfoeG as instrument suggests that hours only decrease by 0.2 hrs. (no year dummies; $p < .10$) to 0.3 hrs. (with year dummies; $p < .10$).

Assuming an average hourly wage of 14 € for students, this implies that in the observed (endogenous) scenario, a 100 € increase in BAfoeG leads to a reduction in labor supply equivalent to 85–97 €. Consequently, the student is only slightly better off in purely monetary terms. However, in the IV setup, which captures the purely exogenous variation, the labor supply reduction is only 12–18 €. If this difference in estimated effects is attributed solely to simultaneity and not to other unobserved factors, it suggests that combining an increase in BAfoeG with a relaxation of the own income deduction could increase the students' financial resources more sustainable.

In summary, this analysis indicates that higher BAfoeG generally reduces student labor supply, with a negative incentive effect observed across all specifications as well as for the intensive and extensive margin. A high share of this observed negative effect is not visible in the IV setup, suggesting that it is in deed driven by endogeneity.

5 Discussion and Limitations

The primary aim of this thesis was to investigate the relationship between higher means-based student aid (BAfoeG) and decisions regarding enrollment, non-take-up, and labor supply. The general findings indicate that higher levels of BAfoeG could lead to a higher probability of enrollment. Furthermore, the IV-approach used in this study suggests that non-take-up exists across the entire BAfoeG spectrum, and the negative incentive effects on labor supply are smaller than expected. Yet, it remains unclear how robust these results are, as well as how potential effects differ between subgroups and across different time frames.

5.1 Enrollment

While the estimated effect of a 1–1.3 pp increase in enrollment probability aligns well with European literature (e.g. Nielsen et al., 2010), it is interesting to note that actual within differences (i.e. mech. BAfoeG) do not seem to drive students' decisions. These results are consistent with the general argument that students may misperceive actual changes at the individual level and are often unaware of their eligibility for aid (see Riedmiller (forthcoming); Riedmiller and Strobel (forthcoming); and implicitly in Herber and Kalinowski, 2019). Thus, it appears that only the communicated maximum levels of BAfoeG influence the general market demand.

This market demand and the enrollment elasticity differ between subgroups. Arguably the most prominent finding is that it seems to rather affect non-recipients than BAfoeG recipients – a counterintuitive result. This difference in effects could either fit into the misperception narrative, suggesting that further research should distinguish between ex-ante "believers" and "non-believers" with respect to their BAfoeG reception. It could also mean that higher student aid leads to higher market tightness, with negative externalities heterogeneously affecting different subgroups (similar to those shown in the search-and-matching labor market context, e.g. Gautier, 2002). These negative externalities could, for instance, stem from more applications, raising the probability of a non-match with a potentially higher gradient for low GPA students. In order to examine such effects one would need subject and application data.

The heterogeneity in effects is also visible across further subgroups. As shown in Table 17 (Appendix), a slightly positive effect that is not significantly different from zero appears in all specifications for first generation students. This effect suggests an increase of 0.1–0.6 pp for a 10 % increase in BAfoeG max. rate, which, in economic values, represents a rather small effect. For the migration background subgroup in Table 18 (Appendix), the effect is slightly negative in all specifications without health information ($\hat{\beta} = -.0002$; $p = ns.$) and slightly positive when controlling for health ($\hat{\beta} = .0012$; $p = ns.$). This implies that a higher real BAfoeG max. rate is correlated with a higher probability of studying only for non-first-generation, non-migration-background non-recipients. Hence, another application of the "Matthew-effect" (Joensen and Mattana, 2024, p.3), i.e. the privileged receive even more, is indicated. From a policy stand-point this would question the current design and targeting.

Yet, it should be noted that the findings should be regarded as correlational rather than causal, primarily due to the observational nature of the data. This limitation includes the potential for unaccounted time trends. Particularly in the case of the max. rate, it is not possible to include yearly fixed effects as this would capture the same variation and, hence, OLS would not identify any potential causal effects. In this thesis' model such a variation, e.g. by more pupils graduating high school, is to some extend already captured by design of the subset. However, examining specification (6) from Table 2 with different start and end points reveals the underlying issue (visualized in Figure 7 and detailed in Table 13, Appendix). As shown in Figure 7, plotting every estimated $\hat{\beta}$, where the difference between start and end years is at least three years, demonstrates that these results are not robust. The average estimated beta does not significantly differ from zero. Consequently, a spurious correlation cannot be ruled out[10], and the results should only be used as preliminary indications for further research.

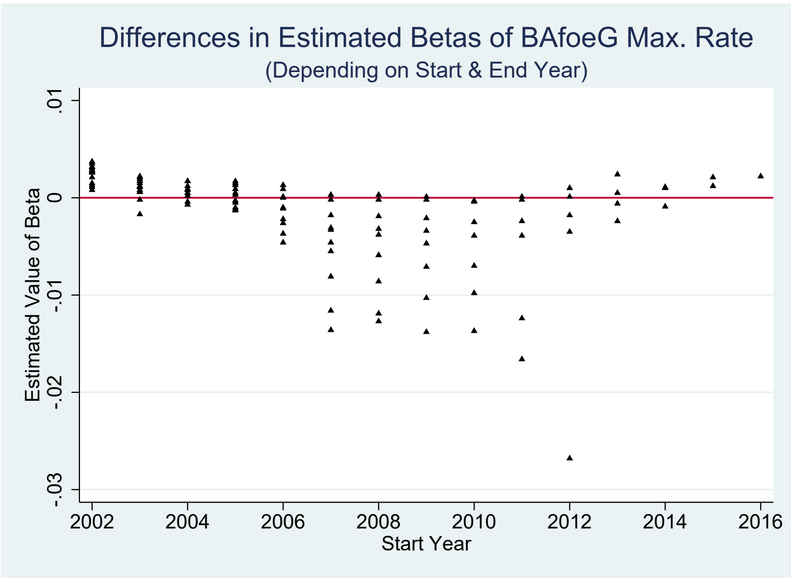

Figure 7: Enrollment: Differences in Estimated Betas in the Model with FE Estimation depending on Start- & End Year

[10]Placebo regressions on PR(Living in State X) or Pr(HH Help), however, yield non-sign. effects close to zero.

Further, one could argue that the grant-loan-share should be included, as implicitly suggested by Joensen and Mattana, 2024, and that the variation through the VPI may be misleading for students' decisions. As shown in Table 16 (Appendix), including the individual grant share results in statistically and economically insignificant outcomes ($\hat{\beta} = -0.0001$; $p =$ ns.). This specification should be considered with caution, as the grant share is highly correlated with specific circumstances (e.g. insurance situations or children), leading to a negative coefficient that lacks economic reasoning. Using changes in nominal wages instead of the VPI to better capture the "outside option" in real BAfoeG indicates a much smaller effect across the full sample ($\hat{\beta} = -0.0003$; $p < 0.01$) for the maximum rate (see Table 14, Appendix) and a pure null effect for mech. BAfoeG (see Table 15, Appendix). Thus, it should be concluded that the model is not robust and may potentially exclude time-variant variation correlated with the error term (e.g. preference changes).

Combined with the general limitations of this thesis' approach, the policy takeaways are therefore limited. However, certain patterns persist across all specifications, suggesting areas for further research and consideration. Specifically: (1) Individual within-group differences in BAfoeG do not appear to have a significant impact, indicating incomplete information; (2) the effect on potential target groups (recipients, first-generation students, those with a migration background) is consistently smaller; and (3) younger individuals seem to respond more elastically to changes.

5.2 Non-Take-Up

The main results with respect to non-take-up indicate that the pooled IV-probit estimation leads to a smaller AME than both the pooled probit estimation and Herber and Kalinowski (2019). However, it still suggests a small negative relationship between BAfoeG and non-take-up (1.3 pp for every 100 €). It needs to be determined where the difference to Herber and Kalinowski (2019) stems from and what could be the potential reasons for the within differences in this thesis.

Herber and Kalinowski (2019) suggest an AME of 4.4 pp for the subsample 2002-2013. Referring to specification (4) in Table 20 (Appendix), this thesis estimates only an AME of 1.8 pp for the same period. This discrepancy may be partly due to Herber and Kalinowski (2019) including more variables, particularly psychological determinants, which implicitly reduces variation as they have to drop more observations due to their exclusive data approach. While their model's estimates suggest no endogeneity as there is no shift in the coefficient when including the instrument, their tests indicate endogeneity. However, their over-identification with two instruments, the small sample size, and their slightly different specification of mech. BAfoeG with respect to the living situation might be driving factors. Additionally, they manually correct for small deviations around the cutoff to increase their beta error rate, which is not done in this thesis. Especially this approach-difference with respect to corrections could lead to bias in either approach. Their method might underestimate non-take-up for smaller BAfoeG levels ($\Rightarrow \beta \uparrow$), whereas this thesis's approach could yield the opposite bias. Therefore, one could argue that their estimate represents the upper bound, while this thesis's estimate represents the lower bound.

The theoretical argument for the within difference between a pooled probit and IV-probit estimation stems from the fact that the observed (sim.) BAfoeG is conditional on the labor supply decision, and, thus, is lower. It is crucial to be precise here – non-take-up includes only the students who would still receive BAfoeG despite working (yellow curve, Figure 2) and choose not to take it up. Therefore, the difference in the estimates for "mechanical non-take-up" (black curve) (see Table 21; $\hat{\beta} = -0.0226$; $p < 0.01$) captures this endogenously induced non-take-up. Consequently, the initial regression explicitly excludes the argument that low BAfoeG level students work more (which is additionally indicated by the higher $\hat{\beta}$ in Table 21, Appendix), but rather implies that students around the cutoff may not precisely know if they would receive BAfoeG, find the bureaucracy not worth it, or do not want to take on debt for small amounts[11].

Further differences are visible in Table 19 (Appendix) using a similar argument for different subgroups. For first-generation students, this effect is much more pronounced ($\hat{\beta} = -0.0216$; $p < 0.05$), while for students with a migration background ($\hat{\beta} = 0.0005$; $p = \text{ns}$) and the combination of both ($\hat{\beta} = 0.0022$; $p = \text{ns}$), no significant effect is observed. This could be mistakenly interpreted as first-generation students being less well-informed. However, this is not the conclusion that can be drawn. Rather, it suggests a knowledge gap within the group. Assuming that all groups around the cutoff are equally poorly informed due to the complex system in Germany, it suggests that students with a migration background are less well-informed across the entire distribution. The same argument applies when examining Table 20 (Appendix) and the different estimates for time periods. While non-take-up seems to be a cutoff phenomenon from 2002 to 2013 ($\hat{\beta} = -0.0179$; $p < 0.05$), it appears to be a more general phenomenon in recent years ($\hat{\beta} = -0.026$; $p = \text{ns}$), also indicated by the generally high numbers (as shown by year dummies or graphically in Figure 2).

While one could argue that, from a social planner's point of view, non-take-up rates around the cutoff might be negligible as those students, in theory, can manage by working, non-take-up rates among highly funded students (i.e., those with high need) are more concerning. Thus, these findings suggest several policy implications. First, targeted informational campaigns could be useful to address the specific knowledge gaps identified. This could be e.g. an automatically calculated BAfoeG amount sent per post when graduating. Additionally, simplifying the application process and reducing bureaucratics could make it easier for students to apply for and receive BAfoeG. However, the distributional effects of non-take-up and increasing take-up remain uncertain.

It is important to recognize that pooled regressions in panel data come with certain risks. Specifically, pooled regressions may overlook the potential for time-invariant unobserved heterogeneity and might not adequately account for the dynamics within the panel, which could lead to biased or inconsistent estimates. Further, testing for identification in this setup, especially using pooled IV methods, is complex and non-trivial. Effects might be overestimated if certain groups, such as slower students who stay in the sample longer, disproportionately influence the results. Additionally, the probit IV-regression relies on distributional assumptions that are not thoroughly discussed here. At least no differences between the initial IV-probit results and the appendix results (2SLS IV in LPM) exist. Still, these results should be considered correlational with a small shot for causal identification.

[11] While it may seem economically irrational to forgo BAfoeG, considering that 50 % is a non-repayable grant and the remaining 50 % can be saved, survey responses by Riedmiller and Strobel (forthcoming) still support this argument.

5.3 Labor-Supply

The main results in the pooled IV regression suggest a negative incentive effect of a decrease of 1.5–1.8 pp (ext. margin) and 1.4–1.6 weekly hrs. (int. margin) on average for a 100 € increase in BAfoeG. This is much smaller than the observed correlation, which includes the endogenous variation. Compared to the general economical literature, e.g. Vivalt et al. (2024), this effect seems rather large. In their RCT, an increase of 1000 $ for low income groups leads to an effect of 2 pp & 1.3–1.4 weekly hrs. (Vivalt et al., 2024). Under neglection of potential effect convexity, the effect for students seems roughly 10 times as strong as for the general (low-income) population. This, however, does not seem unreasonable as the opportunity costs might be higher for students when working combined with a potential projection bias with respect to potential long-run effects of working while studying. Especially Mattana and Joensen (2022) show that a causal relationship between student job and labor market outcome might exist.

Table 22 (Appendix) and Table 23 (Appendix) show that there is some heterogeneity in the observed effect. Specifically, first-generation students seem to react much more elastically along the intensive margin to changes in their funding ($\hat{\beta} = -0.4259$; $p < 0.05$), while students with a migration background ($\hat{\beta} = -0.1925$; $p = ns$) and the combination of both ($\hat{\beta} = -0.2733$; $p = ns$) show less elasticity (see Table 22, Appendix). The non-significance of the latter effects might be attributable to the loss in statistical power, but it could also indicate that first-generation students face higher opportunity costs of working. These higher opportunity costs could stem from either a greater need for time to study, mediated through social barriers, or from the necessity to rely on student jobs that are not connected to their long-term labor market goals. However, the changes along the ext. margin for first generation students ($\hat{\beta} = -0.0259$; $p < 0.01$), students with migration background ($\hat{\beta} = -0.0216$; $p < 0.01$) and the combination ($\hat{\beta} = -0.0322$; $p < 0.01$) suggest that the higher opportunity costs might exists across all groups.. Yet, one should be aware that the difference in point estimates is only indicating a difference, which is not statistically secured as the 95%-CI overlap in all cases.

To assess the robustness of the general findings, different time specifications and the maximum rate as an instrument were used. It remains questionable how robust the results are to variations in the start and end years. In this estimation approach, it is also possible to include year dummies (see the difference between specifications (4) and (5) in Table 12, Appendix & Table 6), which account for some static variation across years. As illustrated in Figure 8 (and detailed in Table 25), which displays the estimates from the baseline specification (see specification (6) in Table 6) for every combination of start and end years that are at least three years apart, there is still evidence of time-based heterogeneity. However, in contrast to the enrollment case, the average coefficient β is significantly different from zero, with particularly small intervals in the early 2000s leading to potentially biased estimates. Additionally, the high estimated values around the financial crisis can be explained by the possibility that more students may have relied on student jobs, which were less relevant to their long-term goals. Thereby, these students might have reacted more elastically to changes in funding in their labor supply. Hence, the results seem robust.

Table 24 shows the differences for both the ext. ($\hat{\beta} = -0.0477$, $p = ns$) and int. margin ($\hat{\beta} = -1.2972$, $p = ns$) when using the BAfoeG max. rate as an instrument instead of mech. BAfoeG. Neither of these effects is statistically significant and the instrument's lower explanatory power is evident from the first-stage results (0.0074 vs 0.0054 and 0.0073 vs 0.0045). This somewhat questions the exogeneity assumption of the instrument used. Although the mech. BAfoeG should be considered as exog. policy package variation while the max. rate instrument only captures the exog. max. rate part of this variation. Hence, the neglection of the full policy package results in a weaker instrument and, consequently, different estimates. However, if one questions the instrument's exogeneity of mech. BAfoeG, this should not be viewed as counter-evidence due to the imprecision of the estimates. On the other hand, it also does not support robustness of the presented results.

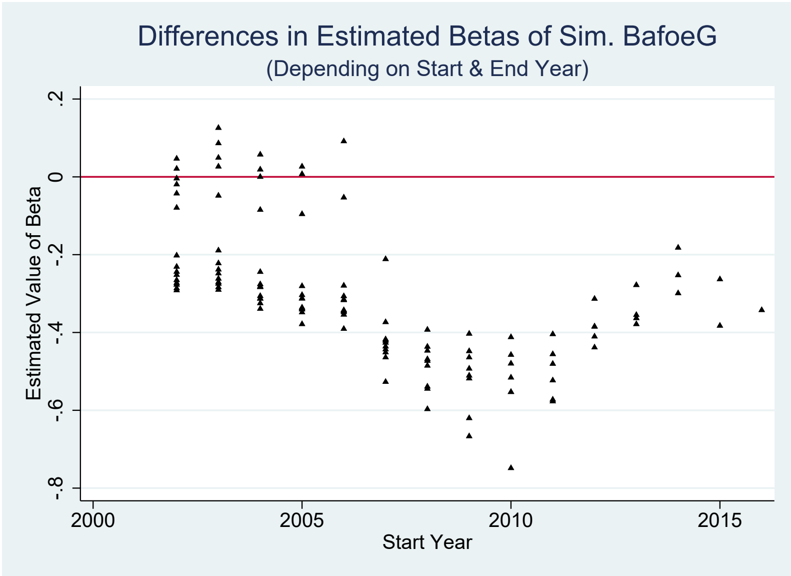

Figure 8: Labor Supply: Differences in Estimated Betas in the Model with IV Estimation depending on Start- & End Year

The policy implications, if this thesis were to identify causal effects, are still not entirely clear-cut. On the one hand, a decrease in labor supply due to increased student funding might lead to long-term positive effects, such as improved academic performance and, hence, better career outcomes (which would align with Bettinger et al., 2019). Conversely, there is also the concern that reduced labor supply might have negative impacts if it leads to missed opportunities for gaining valuable work experience (following the argument in Mattana and Joensen, 2022).

However, this thesis suggests three implications: (1) Labor supply is endogenously influenced by the design of student aid and should, therefore, be considered in policy effect estimations. (2) Relaxing the own-income constraint while increasing student aid could reduce potential negative effects on labor supply. (3) Higher student aid might steer students' labor supply towards more career-oriented jobs, as the relative benefits of such jobs decrease compared to the opportunity costs. However, these effects appear to be highly heterogeneous across different subgroups, potentially leading to significant distributional consequences. Yet, these implications are by no means convincingly defendable based on this thesis only and need further research, e.g. using a quasi-experimental setup, like Denmark's 1998 reform, combined with subject and work data, where one could classify jobs as career oriented.

5.4 General Limitations

Besides the already mentioned limitations, there are additional general limitations to consider. First, this analysis focuses only on rather small changes in the student aid (both on the real and nominal level), which may be negligible and could lead to an overestimation of their effects in real life. Specifically, the analysis is capturing periods with a significant decline in real BAfoeG over time, with only a few increases. Hence, both internal and external validity of the findings could be limited.

Second, the external validity of the findings is also rather limited, as BAfoeG serves as a specific example of means-tested student aid, which is enormously complex. While the results may offer insights into similar means-tested programs, the effects might not be directly applicable to other contexts or countries with different aid structures. Therefore, caution should be exercised when generalizing these result.

As the system is very complex, previous interpretations in this thesis rely on one simplification with respect to the policy package. Mech. BAfoeG captures the actual difference in euros for individual students. Thus, it considers the whole policy package rather than just the pure increase in funding, which is distributed unevenly. While this could in fact be extrapolated to other student aid setups, this thesis also highlighted the potential effects of information deficits for students. This knowledge gap questions both the external and internal validity of any results obtained.

Third, one parameter often overlooked in every estimation, including this thesis, is the repayment cap. Since 2002, this cap has remained fixed at 10,000 €. Consequently, depending on the ex post length of their studies, students may experience varying grant percentages, as indicated in Figure 9. This variability is not validly includable in any SOEP analysis. However, given the complexity of the system, most students do not know the specific ruling ex ante. Therefore, while this aspect might be tolerable, it remains a potential bias and questions the internal validity of the results.

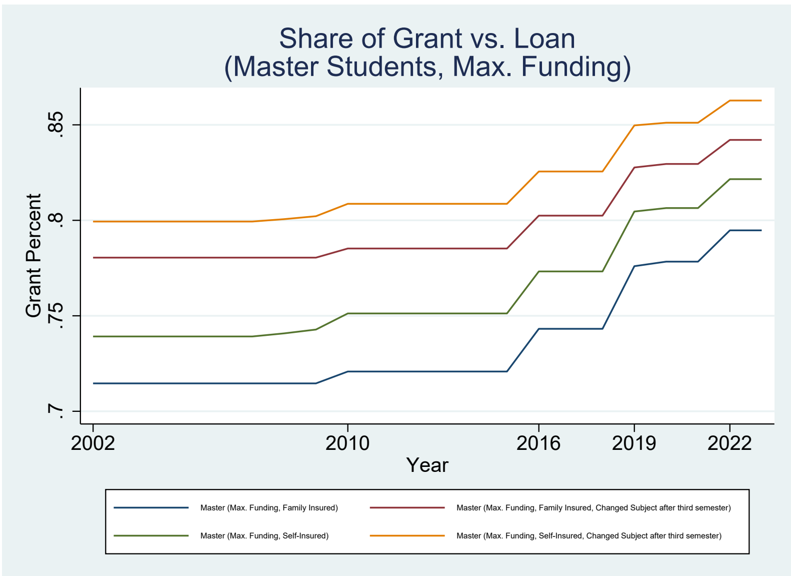

Figure 9: Grant- vs. Loan-Share in Different Scenarios

In the SOEP, data on subject, degree, and wealth are not available. This is a major limitation. The lack of subject data could bias the results in any direction, as it correlates with study duration and the time required per week. Additionally, wealth and study duration are exclusion criteria for BAfoeG eligibility. Therefore, the estimations in this thesis may slightly overestimate the rates of recipients and, consequently, potential effects.

Combined with the inclusive data cleaning approach to avoid any bias by exclusion, it is not surprising that a higher beta error rate is reported. Consequently, this approach may lack precision, as seen in the case of enrollment. While this trade-off is always prevalent in observational data, it indicates the need for more precise identification such as RCTs or quasi-experiments. As seen before, the robustness of the results is mixed and any generalization is highly questionable.

6 Summary and Concluding Remarks

This thesis has highlighted that complex student aid designs like BAfoeG not only make it hard to causally identify effects but also lead to potential adverse policy effects (like non-take-up or indirectly negative labor supply effects). The results indicated a (non-robust) positive relationship between higher max. rate BAfoeG and enrollment, a non-falsifiable null effect of within individual but between year BAfoeG differences onto enrollment, that non-take-up is potentially driven by information asymmetries across the full BAfoeG spectrum and that negative labor supply effects of higher BAfoeG exist.

While one could based on the complexity and the reliance on observational data (extended by simulations) speculate that the indicated effects and differences between subgroups are valid, it fundamentally rests on the assumption that individuals are aware of these aid designs. This raises a more general fundamental question and a further layer on the screening-issue within means-testing: should the state ensure that policies are understandable and validly measurable with respect to their outcomes?

Still, the underlying uncertainty about the supposed state's goal remains. How is the policy preference in the equality-efficiency trade-off? What does efficiency mean in the context of student aid? While one could argue that the brightest students should be able to study, this is not trivially true. However, while answers partly rely on political preferences, the economic design heavily relies on the measurability of state interventions to compare it to laissez-faire outcomes.

Yet, every correlational effect mentioned in this thesis (and most further papers) is a short-term effect. Therefore, there is considerable uncertainty about long-run effects and the variability of effects over different periods. This is particularly true concerning career planning and long-term decision-making. Planning with respect to subject choice is not yet well understood. Additionally, potential adverse effects within the search and matching model framework have not been adequately researched. Furthermore, the economy is implicitly considered a "closed economy" in the macroeconomic sense, assuming that students choose their education and careers within a single country. Given the increased mobility, especially within the EU, this is a potentially strong assumption. Hence, further research layers are needed to develop evidence-based policies that utilize the positive externalities and insurance effects of higher education spending.

References

Andersen, T. M. (2008). The scandinavian model—prospects and challenges. *International Tax and Public Finance*, 15:45–66.

Andersen, T. M. (2018). Human capital, inequality, and growth. In *The Welfare State Revisited*, pages 165–188. Columbia University Press.

Arendt, J. N. (2013). The effect of public financial aid on dropout from and completion of university education: evidence from a student grant reform. *Empirical Economics*, 44:1545–1562.

Bargain, O., Immervoll, H., and Viitamäki, H. (2012). No claim, no pain. measuring the non-take-up of social assistance using register data. *The Journal of Economic Inequality*, 10:375–395.

Baumgartner, H. J. and Steiner, V. (2005). Student aid, repayment obligations and enrolment in higher education in germany-evidence from a" natural experiment". *Journal of Contextual Economics–Schmollers Jahrbuch*, (1):29–38.

Bettinger, E. et al. (2004). *How financial aid affects persistence*, volume 10242. National Bureau of Economic Research Cambridge, MA.

Bettinger, E., Gurantz, O., Kawano, L., Sacerdote, B., and Stevens, M. (2019). The long-run impacts of financial aid: Evidence from california's cal grant. *American Economic Journal: Economic Policy*, 11(1):64–94.

Black, S. E., Denning, J. T., Dettling, L. J., Goodman, S., and Turner, L. J. (2023). Taking it to the limit: Effects of increased student loan availability on attainment, earnings, and financial well-being. *American Economic Review*, 113(12):3357–3400.

BMBF (2016). Federal Ministry of Education and Research. Private Förderer geben 87 Millionen Euro für talented Studierende. Press Release 054/2016. https://www.bmbf.de/bmbf/shareddocs/pressemitteilungen/de/private-foerderer-geben-87-mil-o-fuer-talentierte-studierende.html. Accessed: July 2024.

BMF (2024). Federal Ministry of Finance. Programmablaufplan. https://www.bundesfinanzministerium.de/Web/DE/Themen/Steuern/Steuerarten/Lohnsteuer/Programmablaufplan/programmablaufplan.html. Accessed: May 2024.

Bruckmeier, K., Pauser, J., Walwei, U., and Wiemers, J. (2013). Simulationsrechnungen zum Ausmaß der Nicht-Inanspruchnahme von Leistungen der Grundsicherung: Studie im Auftrag des Bundesministeriums für Arbeit und Soziales zur Abgrenzung und Struktur von Referenzgruppen für die Ermittlung von Regelbedarfen auf Basis der Einkommens-und Verbrauchsstichprobe 2008. Technical report, IAB-Forschungsbericht.

Bruckmeier, K. and Wiemers, J. (2012). A new targeting: a new take-up? non-take-up of social assistance in germany after social policy reforms. *Empirical Economics*, 43(2):565–580.

Buettner, T., Erbe, K., and Grimm, V. (2019). Tax planning of married couples and intra-household income inequality. *Journal of Public Economics*, 179:104048.

Deming, D. and Dynarski, S. (2009). Into college, out of poverty? policies to increase the postsecondary attainment of the poor. Technical report, National Bureau of Economic Research.

Deming, D. and Dynarski, S. (2010). College aid. In *Targeting investments in children: Fighting poverty when resources are limited*, pages 283–302. University of Chicago Press.

Destatis (2024a). Federal Statistical Office of Germany. Verdienste nach Branchen und Berufen – Durchschnittliche Bruttomonatsverdienste, Zeitreihe. https://www.destatis.de/DE/Themen/Arbeit/Verdienste/Verdienste-Branche-Berufe/Tabellen/liste-bruttomonatsverdienste.html. Accessed: June 2024.

Destatis (2024b). Federal Statistical Office of Germany. Verpraucherpreisindex für Deutschland. https://www-genesis.destatis.de/genesis/online?sequenz=tabelleErgebnis&selectionname=61111-0001&startjahr=1991. Accessed: May 2024.

Dynarski, S. M. (2003). Does aid matter? measuring the effect of student aid on college attendance and completion. *American Economic Review*, 93(1):279–288.

Esping-Andersen, G. (1990). *The Three Worlds of Welfare Capitalism*. Princeton University Press, Princeton, NJ.

Federal Government of Germany (2023). Dreiundzwanzigster Bericht nach § 35 des Bundesausbildungsförderungsgesetzes zur Überprüfung der Bedarfssätze, Freibeträge sowie Vomhundertsätze und Höchstbeträge nach § 21 Absatz 2. Drucksache 20/9870, Deutscher Bundestag, Berlin, Germany. 20th Legislative Period.

Fidan, M. and Manger, C. (2022). Why do german students reject free money? *Education Economics*, 30(3):303–319.

Gautier, P. A. (2002). Unemployment and search externalities in a model with heterogeneous jobs and workers. *Economica*, 69(273):21–40.

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

Goebel, J., Grabka, M. M., Liebig, S., Kroh, M., Richter, D., Schröder, C., and Schupp, J. (2019). The German Socio-Economic Panel (SOEP). *Jahrbücher für Nationalökonomie und Statistik*, 239(2):345–360.

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

Joensen, J. S. and Mattana, E. (2024). Student aid design, academic achievement, and labor market behavior: grants or loans? *Working Paper*.

Kleven, H. J. and Schultz, E. A. (2014). Estimating taxable income responses using danish tax reforms. *American Economic Journal: Economic Policy*, 6(4):271–301.

Lauer, C. (2002). Enrolments in higher education: do economic incentives matter? *Education+ Training*, 44(4/5):179–185.

Lochner, L. and Monge-Naranjo, A. (2012). Credit constraints in education. *Annu. Rev. Econ.*, 4(1):225–256.

Mattana, E. (2018). Literature review on financial aid to university students. *Baggrundsnotat, De Økonomiske Rads Sekretariat*, pages 1–29.

Mattana, E. and Joensen, J. S. (2022). Studying and working: Your student job affects your future labor market outcomes. *Samfundsøkonomen*, 2022(3):19–37.

Nielsen, H. S., Sørensen, T., and Taber, C. (2010). Estimating the effect of student aid on college enrollment: Evidence from a government grant policy reform. *American Economic Journal: Economic Policy*, 2(2):185–215.

Riedmiller, S. (forthcoming). 'I wont't get it anyway' - A field study on the role of misperceptions for financial student aid applications. *Working Paper*.

Riedmiller, S. and Strobel, S. (forthcoming). Identifying the Information Gap: Measuring the Role of Misperceptions in Student Aid Non-Take-Up. *Working Paper*.

Scott-Clayton, J. and Zafar, B. (2016). Financial aid, debt management, and socioeconomic outcomes: Post-college effects of merit-based aid. NBER Working Paper 22682, National Bureau of Economic Research.

Seegers, P. K., Vater, T. M., Menke, J., and Hartmann, S. (2023). Sonderpublikation zur Studienreihe Fachkraft 2030. https://jobvalley.com/pdf/whitepaper-verbraucherpreise.pdf. Accessed: July 2024.

SOEP (2023). Sozio-oekonomisches Panel (SOEP), Version 38, Daten der Jahre 1984-2021 SOEP-Core v38, EU-Edition). https://doi.org/10.5684/soep.core.v38eu. Accessed: March 2024.

Steiner, V. and Wrohlich, K. (2012). Financial student aid and enrollment in higher education: New evidence from germany. *The Scandinavian Journal of Economics*, 114(1):124–147.

Vivalt, E., Rhodes, E., Bartik, A. W., Broockman, D. E., and Miller, S. (2024). The employment effects of a guaranteed income: Experimental evidence from two u.s. states. Working Paper 32719, National Bureau of Economic Research.

A Further Figures

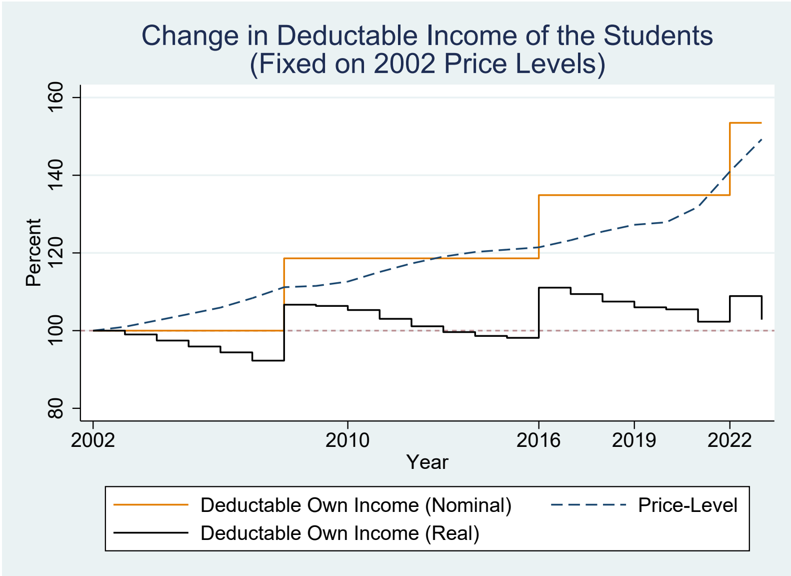

Figure 10: Parameter Change over Time in Deductable Income of the Students

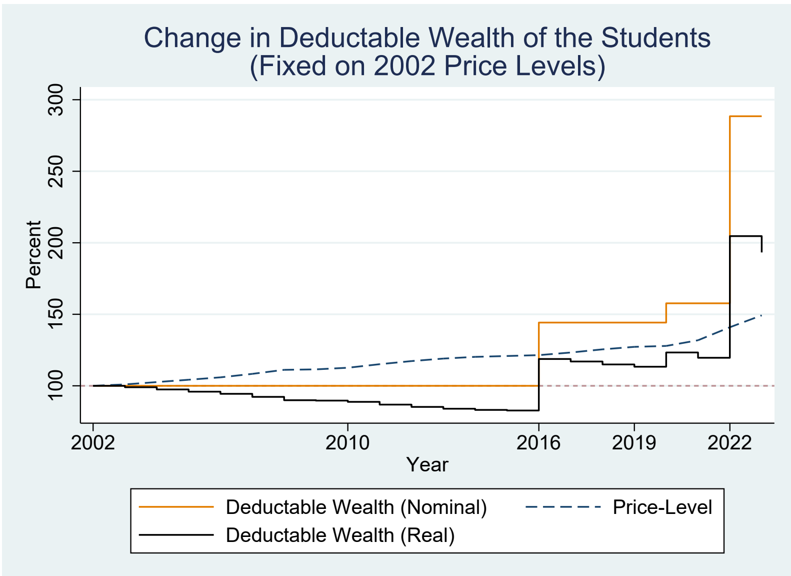

Figure 11: Parameter Change over Time in Deductable Wealth of the Students

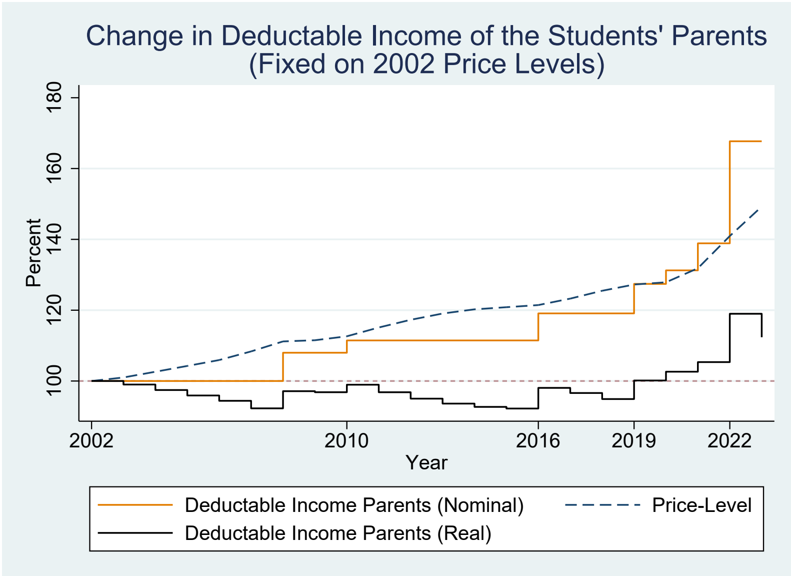

Figure 12: Parameter Change over Time in Deductable Income of the Students' Parents

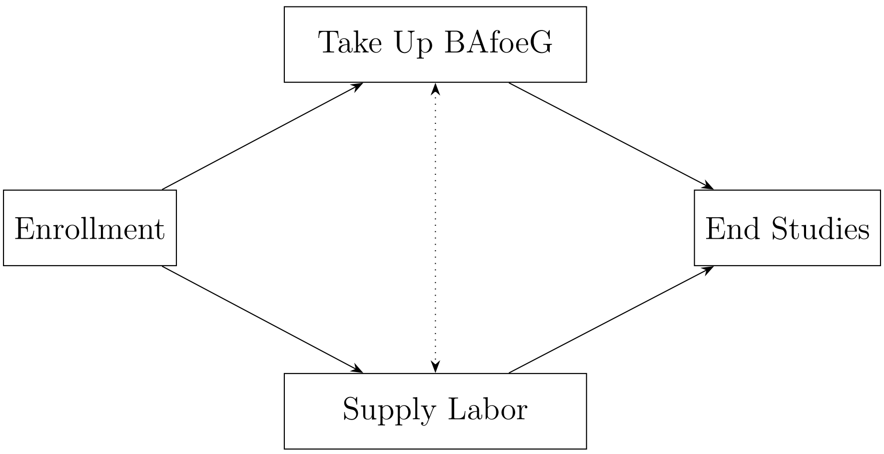

Figure 13: Students' Decision Nodes which Might be Affected by BAfoeG

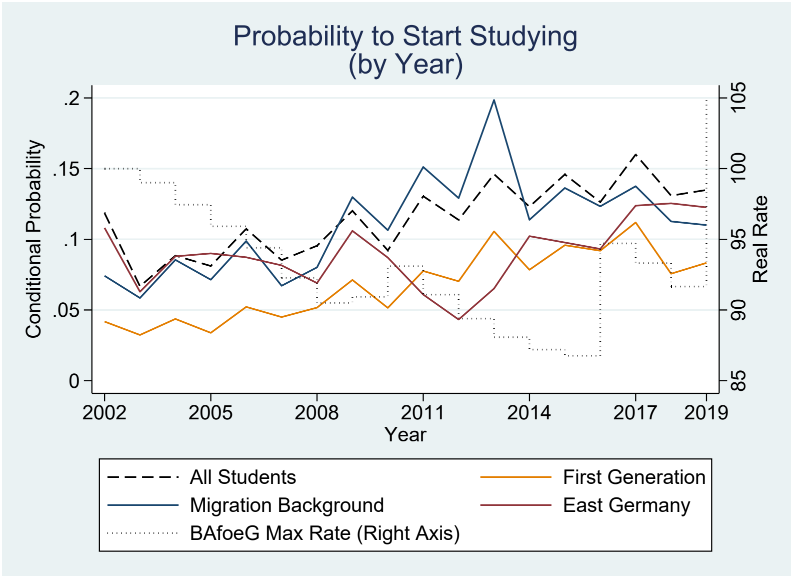

Figure 14: Probability to Start Studying by Year & Subgroup

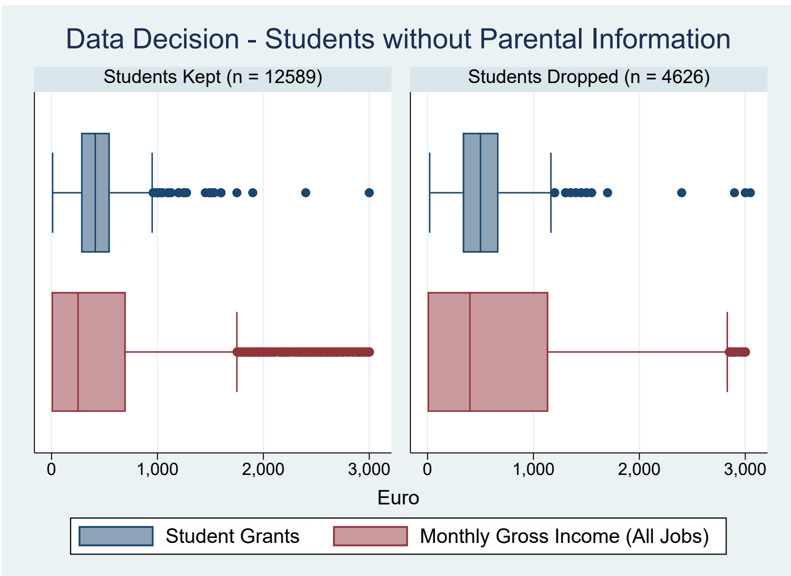

Figure 15: Income & Student Grants; Kept vs. Dropped Students

B Further Tables

Table 7: Overview of BAfoeG Variables

Variable	**Description**
BAfoeG Max. Rate	Individual max. value before deductions and allowances (considered as rate; based on § 12 BAföG)
Mechanical BAfoeG	Individual BAfoeG value without deduction of own income
Simulated BAfoeG	Individual BAfoeG value with deduction of own income
BAfoeG / Student Grant	This is based on the SOEP and includes the self-reported numbers. However, as it includes both BAfoeG and student grants in the question combined with a lot of missings, it is not used.

Table 8: Summary Statistics Non-Students (2002-2019)

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Individual Characteristics:					
Age	25458	25.7247	5.0839	17	35
Male	25458	.5171	.4997	0	1
Migration Background	25458	.2708	.4444	0	1
First Gen	25458	.6952	.4603	0	1
Income:					
Inc. Student (€, Gross, Mth.)	25458	1315.5583	1301.5073	0	49000
Labor Supply	25458	.7996	.4003	0	1
Labor Supply (Weekly Hours)	25458	27.4549	17.4159	0	80
Inc. Father (€, Gross, Mth.)	25458	2144.1482	3181.3766	0	104000
Inc. Mother (€, Gross, Mth.)	25458	1050.8017	1345.8818	0	25000
BAfoeG:					
BAfoeG (Mechanical)	25458	.6597	.4738	0	1
BAfoeG (Mechanical, €, Mth.)	16795	453.3889	226.5455	10	1553

Note: A person is considered a non-student if they are under 35 years old, have earned the right to study, but are not studying in the respective year and have not finished any post-secondary degree.

Table 9: Share of BAfoeG Recipients per Year – Actual vs. Simulated

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Received Student Grants (actual)	12589	.2361	.4247	0	1
- 2002	589	.2309	.4218	0	1
- 2003	601	.2696	.4441	0	1
- 2004	660	.2591	.4385	0	1
- 2005	633	.2243	.4175	0	1
- 2006	641	.234	.4237	0	1
- 2007	614	.2117	.4089	0	1
- 2008	607	.2273	.4195	0	1
- 2009	599	.2321	.4225	0	1
- 2010	615	.2325	.4228	0	1
- 2011	644	.264	.4411	0	1
- 2012	637	.2873	.4529	0	1
- 2013	772	.2798	.4492	0	1
- 2014	764	.2421	.4287	0	1
- 2015	812	.2389	.4267	0	1
- 2016	794	.2418	.4285	0	1
- 2017	886	.2167	.4122	0	1
- 2018	890	.191	.3933	0	1
- 2019	831	.1913	.3936	0	1
BAfoeG (simulated)	12589	.3220	.4673	0	1
- 2002	589	.3379	.4734	0	1
- 2003	601	.3411	.4745	0	1
- 2004	660	.2894	.4538	0	1
- 2005	633	.2717	.4452	0	1
- 2006	641	.259	.4384	0	1
- 2007	614	.2492	.4329	0	1
- 2008	607	.2685	.4436	0	1
- 2009	599	.2805	.4496	0	1
- 2010	615	.3122	.4638	0	1
- 2011	644	.3323	.4714	0	1
- 2012	637	.3312	.471	0	1
- 2013	772	.3795	.4856	0	1
- 2014	764	.3442	.4754	0	1
- 2015	812	.3387	.4735	0	1
- 2016	794	.3766	.4848	0	1
- 2017	886	.3533	.4783	0	1
- 2018	890	.3371	.473	0	1
- 2019	831	.3333	.4717	0	1

Note: Student Grants include state funding (BAfoeG) as well as other forms of stipends. BAfoeG amounts are calculated under the assumption of optimal behavior and always assume an application is made if the amount is greater than zero. Therefore, these figures may be higher than the actual amounts received.

Table 10: Overview of Core Variables and its SOEP Origin

Variable	**Description**
Age	Derived from variable D11101, representing the age of the participant in years.
Apprenticeship (Dummy)	Did an Apprenticeship overall, this year or last year. This is gathered by kal1c00i with i $\in$ [1,12].
BAfoeG (€, Mth.)	Indicates how much BAfoeG support the student received, based on variable kal2k03_v2. However, one has to be careful, as this includes in some waves "student grants" and not specifically BAfoeG, meaning merit-based or private grants could bias this upwards. Further, there are a lot of missings for students, who are supposed to have a value there (approx. 20 %) and some values don't survive a plausability check.
BAfoeG (Dummy)	Indicates whether the participant received BAfoeG support, based on variables plc0170 and plc0169_v1. Variable pab0037 is excluded as it is only relevant for data pre-2000. However, one has to be careful, as this includes in some waves "student grants" and not specifically BAfoeG, meaning merit-based or private grants could bias this upwards.
Educational Status (Ordinal)	Represents the highest level of education attained by the participant, based on variables plg0078_h and pgisced11. Data is forward imputed between waves in the panel. This is based on the ICED Scale with low, middle and high education.
Eligible for BAfoeG (Ordinal)	Asses Eligibility based on whether the participant is currently a student and resides within Germany (as indicated by plj0709). Eligibility also considers the participant's age, ensuring they are under 35 years old. Furthermore, adjustments are made based on the Year in University and Educational Level to exclude cases where it is certain that the standard period of study is not met.
First Gen (Dummy)	Neither parent has a tertiary degree.
Health Status (5 Levels)	The person's self reported health status derived from ple0008. From 1 "very good" to 5 "bad".
HH Constellation	Describes the composition of the participant's household or family structure, calculated from variables d11104, pgpartz, pid, and parid.
HH with Parents (Ordinal)	Living in the same household with parents. This is initially derived by '11104, pgpartz, hid, pid, and parid. Yet, this is very specific for the soep, there is a distinction between reported household and factual living adress. For the BAfoeG the latter mattes. Hence, this is over-reporting students living at home (as visible in Herber and Kalinowski (2019). Hence, this is corrected by any means possible. First, if reported state of mum and dad differ fromt he students reported state, then is rural or not does not match for both cases, then over the current location of the dad (fcurrloc) and mum (mcurrloc)
Income (€, Mth.)	Monthly Gross Income. All income types within the soep are considered. This means: plc0141 plc0142_h plc0070_h plc0186 plc0187 plc0189_h plc0190_v1 plc0190_v2 hlc0045_h hlc0081 hlc0082_h pglabgro pgsndjob pgsndjob1 pgsndjob2 pgsndjob3 divdy lossc renty lossr nursh ssold alg2 adchb chspt ielse imaty imilt istuy house ijob1 ijob2 iself i13ly i14ly ixmas iholy igray iothy iwidy ioldy icomp iprvp igrv1 igrv2 iciv1 iciv2 iwar1 iwar2 iguv1 iguv2 ivbl1 ivbl2 icom1 icom2 iprv1 iprv2 ison1 ison2 iunby ispou ichsu ialim iachm i11102 i11110 kal2a02 kal2a03_v2 kal1h02 kal2b02 kal2b03_v2 kal2c02 kal2c03_v2 kal2d02 kal2d03_v2 kal2e02 kal2e03_v2 kal2f02 kal2f03_v2 kal2k02 kal2k03_v2 kal2j02 kal2j03_v2 kal1a001_v2 kal1a002_v2 kal1a003_v2 kal1a004_v2 kal1a005_v2 kal1a006_v2 kal1a007_v2 kal1a008_v2 kal1a009_v2 kal1a010_v2 kal1a011_v2 kal1a012_v2 kal1b001 kal1b002 kal1b003 kal1b004 kal1b005 kal1b006 kal1b007 kal1b008 kal1b009 kal1b010 kal1b011 kal1b012. Missing values within one wave are imputed using a Heckmann correction, if the needed variables are given. W.r.t. BAfoeG it is obviously differentiated between the income types. Specified in Section 2.
Labor Supply	Weekly hrs. Worked. This is implictly derived from pgtatzeit pguebstd plb0176_h plb0186_h plb0197 plb0198 plb0573 plb0632 plb0633 plb0634. It is assumed that every person in the model has 80 hrs. per week. Hence, we drop any exceeding values. It combines working hours for different jobs (e.g. first and second job).
Living in City (Ordinal)	Specifies whether the participant resides in a rural or non-rural area, based on variable regtyp. If missing, imputed from t-1 or t+1.
Male (Dummy)	Indicates the participant's sex, with 0 representing female and 1 representing male. Data is imputed over time and, if totally missing, randomly assignment as 0 or 1. Changes in reported gender are not preserved; only the latest entry is used, overwriting any previous entries.
Migration Background (Dummy)	First or second degree migration background, based on pid/mid and migback.
Partner/Marital Status (Dummy/Ordinal)	Indicates the current partnership or marital status of the participant, derived from variables pid, parid, d11104, and pgpartz.
State (& East Germany)	Indicates the specific state or region where the participant resides, derived from variables bula_h and sampreg. If missing, imputed from t-1 or t+1.
Student (Dummy)	Equivalent to Glocker (2011) "If a person is observed as having studied in the months between October and March, they count as a student in the winter term (Wintersemester). Being a student in the summer term (Sommersemester) implies having studied between April and September." Based on variables pab0013, plg0078_h, pgisced11, plg0014_v7, plg0014_v5, plg0014_v6. Adjusted to exclude individuals with arbeitszeit_w_akt ¿ 40 hours per week. Additional corrections for eligibility for winter and summer terms, and high school completion status.
Tax Class (Ordinal)	This comes from "steuerklasse". However, if this is not given, we assign a Tax class under optimal behavior w.r.t. III and V this is relevant. Still, this might be biased as you actively have to decide it (see e.g. the argument in Buettner et al., 2019).
University Type (Ordinal)	Represents the type of University attended by the student. Includes students who are currently studying, based on variables plg0014_v7, plg0014_v6, and plg0014_v5. Missing data is imputed from the last waves or assumes normal university attendance. Excludes individuals pursuing promotions.
Year in University	Counting years since reported studying. This count only includes periods when the student was actively enrolled in studies.

Table 11: Enrollment: Pooled OLS & Probit with Max. Rate

	(1)		(2)
PR(Enrollment)	Probit	dydx	OLS
Real BAfoeG (Max. Rate)	-.0021	-.0004	-.0004
	(.0024)	(.0004)	(.0004)
First Generation	-.6151***	-.1028***	-.1231***
	(.0237)	(.0039)	(.0051)
Migration Background	-.0349	-.0058	-.0048
	(.0269)	(.0045)	(.0048)
East Germany	-.3191***	-.0533***	-.0631***
	(.0306)	(.0051)	(.005)
Gross Income T-1 (Main Job)	-.0005***	-.0001***	-.0001***
	(0)	(0)	(0)
N	28811		28811
R^2			.0992
Pseudo-R^2	.1562		
RMSE			.3000
Wald Chi^2	1847.82		

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This shows that an OLS within a LPM yields similar marginal effects for the real BAfoeG Max. Rate as probit on Pr(Enrollment).

Table 12: Labor Supply - Intensive Margin: Pooled OLS & IV

Labor Supply (Hrs)	(1) OLS	(2) OLS	(3) IV	(4) IV
Real BAfoeG (Simulated, in 100 €)	-1.4224***	-1.6306***	-.2084*	-.2917**
	(.0733)	(.0808)	(.1172)	(.1151)
First Generation		2.2152***	.7791**	.7618**
		(.3644)	(.371)	(.3677)
Migration Background		-.4899	-1.7223***	-1.8529***
		(.3644)	(.371)	(.3677)
Apprenticeship		√	√	√
Age		√	√	√
Sex		√	√	√
Living in City		√	√	√
Living with Parents		√	√	√
East Germany		√	√	√
Siblings		√	√	√
Year (Dummies)				√
N	12589	12589	12589	12589
Cluster	3824	3824	3824	3824
R^2	.0363	.1323	.1014	.1126
Wald Chi^2			602.31	719.15
RMSE	13.25	12.58	12.8	12.72
Instrument (First Stage)			.0073***	.0073***
			(.0001)	(.0001)

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table shows the effect of sim. BAfoeG (in 100 €) on Labor Supply, Intensive Margin. (1) is a baseline probit, without any controls. (2) introduces the respective controls. In the IV setup (2SLS), (3) & (4) show that the coefficient of interest is significantly smaller in absolute values. Euros are expressed in 2002 euros, adjusted using the relevant price index.

Table 13: Enrollment: Estimated Coef. in Model with FE Estimation Including Different Start & End Points

Start	End	Estimated Coef.	Start	End	Estimated Coef.
2002	2005	.0013	2006	2016	.0009
2002	2006	.0021	2006	2017	-.001
2002	2007	.0032***	2006	2018	-.0022***
2002	2008	.0037***	2006	2019	.0001
2002	2009	.0037***	2007	2010	-.0046**
2002	2010	.0035***	2007	2011	-.0136***
2002	2011	.0031***	2007	2012	-.0116***
2002	2012	.0027***	2007	2013	-.0081***
2002	2013	.0026***	2007	2014	-.0055***
2002	2014	.0026***	2007	2015	-.0033***
2002	2015	.0029***	2007	2016	.0003
2002	2016	.0028***	2007	2017	-.0018**
2002	2017	.0015***	2007	2018	-.0031***
2002	2018	.0008	2007	2019	-.0002
2002	2019	.0011**	2008	2011	-.0127***
2003	2006	-.0017	2008	2012	-.0119***
2003	2007	.0008	2008	2013	-.0086***
2003	2008	.0017**	2008	2014	-.0059***
2003	2009	.0022***	2008	2015	-.0038***
2003	2010	.0021***	2008	2016	.0003
2003	2011	.0015**	2008	2017	-.0019**
2003	2012	.0011	2008	2018	-.0032***
2003	2013	.0011	2008	2019	-.0002
2003	2014	.0012*	2009	2012	-.0138***
2003	2015	.0017***	2009	2013	-.0103***
2003	2016	.0019***	2009	2014	-.0071***
2003	2017	.0006	2009	2015	-.0047***
2003	2018	-.0002	2009	2016	.0001
2003	2019	.0006	2009	2017	-.0021**
2004	2007	-.0004	2009	2018	-.0034***
2004	2008	.0009	2009	2019	-.0002
2004	2009	.0017*	2010	2013	-.0137***
2004	2010	.0017*	2010	2014	-.0098***
2004	2011	.0008	2010	2015	-.007***
2004	2012	.0002	2010	2016	-.0004
2004	2013	.0002	2010	2017	-.0025***
2004	2014	.0005	2010	2018	-.0039***
2004	2015	.0012	2010	2019	-.0003
2004	2016	.0017***	2011	2014	-.0166***
2004	2017	.0002	2011	2015	-.0124***
2004	2018	-.0007	2011	2016	.0001
2004	2019	.0005	2011	2017	-.0024**
2005	2008	.0009	2011	2018	-.0039***
2005	2009	.0017	2011	2019	-.0002
2005	2010	.0013	2012	2015	-.0268***
2005	2011	-.0003	2012	2016	.001
2005	2012	-.0012	2012	2017	-.0018*
2005	2013	-.001	2012	2018	-.0035***
2005	2014	-.0005	2012	2019	.0001
2005	2015	.0005	2013	2016	.0024**
2005	2016	.0015**	2013	2017	-.0006
2005	2017	-.0003	2013	2018	-.0024**
2005	2018	-.0013*	2013	2019	.0005
2005	2019	.0003	2014	2017	.001
2006	2009	.0013	2014	2018	-.0009
2006	2010	.0000	2014	2019	.0011*
2006	2011	-.0037**	2015	2018	.0012
2006	2012	-.0046***	2015	2019	.0021***
2006	2013	-.0037***	2016	2019	.0022***
2006	2014	-.0026**			
2006	2015	-.0011			

Table 14: Enrollment: OLS/FE with Max. Rate using Nominal Wage Change instead of VPI

PR(Enrollment)	(1) OLS	(2) OLS	(3) FE	(4) FE	(5) FE	(6) FE	(7) FE
Real BAfoeG (Max. Rate)	-.0004***	.0001	.0002***	.0002***	.0003***	.0003***	.0002***
	(.0000)	(.0000)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)
Specification							
Set of Individual Controls		√					
Gross Income (Main Job, T-1)				√	√	√	√
HH with Parents				√	√	√	√
Apprenticeship (This Year)					√	√	√
Apprenticeship (Last Year)					√	√	√
Parental Log. Income						√	√
Health Status							√
Individual FE			√	√	√	√	√
N	28811	28056	28811	28811	28811	28811	26547
Cluster	9081	8726	9081	9081	9081	9081	8909
R^2	.0028	.2057	.0009	.0014	.0067	.0067	.0101
Prob > F	.0000	.0000	.0000	.0000	.0000	.0000	.0000

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the estimated effect of Real BAfoeG (Max. Rate) on the probability to enroll. Real BAfoeG is in this case not determined by VPI but instead by the change in nominal wages. (1) is a baseline OLS estimation without controls, while (2) contains the set of individual controls: gross income (T-1), HH with parents, age, age^2, parental income, apprenticeship (last year; current year & general), first generation, migration background, siblings, partner, children, living in the city, sex, state and east Germany. (3)–(7) are the estimations in a fixed effects model including individual fixed effects.

Table 15: Enrollment: OLS/FE with Mechanical BAfoeG using Nominal Wage Change instead of VPI

PR(Enrollment)	(1) OLS	(2) OLS	(3) FE	(4) FE	(5) FE	(6) FE	(7) FE
Real BAfoeG (Mechanical, in 100 €)	-.0000***	.0000	.0000	.0000	.0000	.0000	.0000
	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)
Specification							
Set of Individual Controls		√					
Gross Income (Main Job, T-1)				√	√	√	√
HH with Parents				√	√	√	√
Apprenticeship (This Year)					√	√	√
Apprenticeship (Last Year)					√	√	√
Parental Log. Income						√	√
Health Status							√
Individual FE			√	√	√	√	√
N	28811	28056	28811	28811	28811	28811	26547
Cluster	9081	8726	9081	9081	9081	9081	8909
R^2	.0192	.2056	.0001	.0004	.0049	.0049	.0093
Prob > F	.0000	.0000	.8663	.1610	.0000	.0000	.0000

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the estimated effect of mech. BAfoeG on the probability to enroll. Mech. BAfoeG is in this case not determined by VPI but instead by the change in nominal wages. (1) is a baseline OLS estimation without controls, while (2) contains the set of individual controls: gross income (T-1), HH with parents, age, age^2, parental income, apprenticeship (last year; current year & general), first generation, migration background, siblings, partner, children, living in the city, sex, state and east Germany. (3)–(7) are the estimations in a fixed effects model including individual fixed effects. The effects are heavily rounded, e.g. (1) -2e-06***, which is the only sign. effect. Yet, this means that economically any effects are neglectable. Euros are expressed in 2002 euros, adjusted using the relevant price index.

Table 16: Enrollment: OLS/FE with Max. Rate Including Estimated Grant Share

PR(Enrollment)	(1) OLS	(2) OLS	(3) FE	(4) FE	(5) FE	(6) FE	(7) FE
Real BAfoeG (Max. Rate)	-.0036***	-.0012***	-.0002	-.0002	-.0001	-.0001	.0004
	(.0005)	(.0004)	(.0005)	(.0005)	(.0005)	(.0005)	(.0005)
Specification							
Set of Individual Controls		√					
Gross Income (Main Job, T-1)				√	√	√	√
HH with Parents				√	√	√	√
Apprenticeship (This Year)					√	√	√
Apprenticeship (Last Year)					√	√	√
Parental Log. Income						√	√
Health Status							√
Individual FE			√	√	√	√	√
Grant Share	√	√	√	√	√	√	√
N	18423	17894	18423	18423	18423	18423	17065
Cluster	6198	5932	6198	6198	6198	6198	6028
R^2	.0271	.1753	.0005	.0017	.0056	.0056	.0093
Prob > F	.0000	.0000	.0003	.0000	.0000	.0000	.0000

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the estimated effect of Real BAfoeG (Max. Rate) on the probability to enroll now including the estimated Grant Share. (1) is a baseline OLS estimation without controls, while (2) contains the set of individual controls: gross income (T-1), HH with parents, age, age^2, parental income, apprenticeship (last year; current year & general), first generation, migration background, siblings, partner, children, living in the city, sex, state and east Germany. (3)–(7) are the estimations in a fixed effects model including individual fixed effects.

Table 17: Enrollment: OLS/FE with Max. Rate for First Generation Students

PR(Enrollment)	(1) OLS	(2) OLS	(3) FE	(4) FE	(5) FE	(6) FE	(7) FE
Real BAfoeG (Max. Rate)	-.0023***	-.0009**	.0001	.0001	.0002	.0002	.0006
	(.0004)	(.0004)	(.0004)	(.0004)	(.0004)	(.0004)	(.0004)
Specification							
Set of Individual Controls		$\surd$					
Gross Income (Main Job, T-1)				$\surd$	$\surd$	$\surd$	$\surd$
HH with Parents				$\surd$	$\surd$	$\surd$	$\surd$
Apprenticeship (This Year)					$\surd$	$\surd$	$\surd$
Apprenticeship (Last Year)					$\surd$	$\surd$	$\surd$
Parental Log. Income						$\surd$	$\surd$
Health Status							$\surd$
Individual FE			$\surd$	$\surd$	$\surd$	$\surd$	$\surd$
N	19006	18503	19006	19006	19006	19006	17669
Cluster	5457	5235	5457	5457	5457	5457	5371
R^2	.0018	.1512	.0001	.0006	.0043	.0043	.0077
Prob > F	.0000	.0000	.8698	.0639	.0000	.0000	.0000

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the estimated effect of Real BAfoeG (Max. Rate) on the probability to enroll for first generation students. (1) is a baseline OLS estimation without controls, while (2) contains the set of individual controls: gross income (T-1), HH with parents, age, age^2, parental income, apprenticeship (last year; current year & general), first generation, migration background, siblings, partner, children, living in the city, sex, state and east Germany. (3)–(7) are the estimations in a fixed effects model including individual fixed effects.

Table 18: Enrollment: OLS/FE with Max. Rate for Migration Background

PR(Enrollment)	(1) OLS	(2) OLS	(3) FE	(4) FE	(5) FE	(6) FE	(7) FE
Real BAfoeG (Max. Rate)	-.0038***	-.0009	-.0003	-.0002	-.0002	-.0002	.0012
	(.0007)	(.0007)	(.0007)	(.0007)	(.0007)	(.0007)	(.0008)
Specification							
Set of Individual Controls		√					
Gross Income (Main Job, T-1)				√	√	√	√
HH with Parents				√	√	√	√
Apprenticeship (This Year)					√	√	√
Apprenticeship (Last Year)					√	√	√
Parental Log. Income						√	√
Health Status							√
Individual FE			√	√	√	√	√
N	7810	7574	7810	7810	7810	7810	7127
Cluster	2708	2598	2708	2708	2708	2708	2658
R^2	.0034	.1818	0.0001	.0008	.0048	.0048	.0096
Prob > F	.0000	.0000	.6208	.1625	.0009	.0020	.0004

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the estimated effect of Real BAfoeG (Max. Rate) on the probability to enroll for students with migration background. (1) is a baseline OLS estimation without controls, while (2) contains the set of individual controls: gross income (T-1), HH with parents, age, age^2, parental income, apprenticeship (last year; current year & general), first generation, migration background, siblings, partner, children, living in the city, sex, state and east Germany. (3)–(7) are the estimations in a fixed effects model including individual fixed effects.

Table 19: Non-Take-Up: Pooled IV for Different Subgroups

Pr(Non-Take-Up)	(1) IV	(2) IV	(3) IV	(4) IV
Real BAfoeG (Simulated, in 100 €)	-.0216**	.0005	.0022	-.0128*
	(.0102)	(.0131)	(.0185)	(.0073)
First Generation		-.0708*		-.0797***
		(.0361)		(.0238)
Migration Background	-.0133			-.0393*
	(.0318)			(.0238)
Apprenticeship	√	√	√	√
Sex	√	√	√	√
Living in City	√	√	√	√
Living with Parents	√	√	√	√
East Germany	√	√	√	√
Siblings	√	√	√	√
Year (Dummies)	√	√	√	√
N	2132	1410	901	3930
Cluster	969	639	406	1765
R^2	.0844	.0423	.0524	.0735
Wald Chi^2	124.58	50.43	48.6	177.32
RMSE	.47	.48	.48	.47
Instrument (First Stage)	.0091***	.0091***	.0091***	.0091***
	(.0001)	(.0002)	(.0002)	(.0001)

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the estimated effect of real sim. BAfoeG on the probability of not taking up BAfoeG, now using 2SLS instead of IV probit, for the subgroups first generation and migration background. (1) is only first generation, (2) is migration background, (3) is both, and (4) is the whole sample baseline. Euros are expressed in 2002 euros, adjusted using the relevant price index.

Table 20: Non-Take-Up: Pooled IV in Different Specifications

Pr(Non-Take-Up)	(1) IV	(2) IV	(3) IV	(4) IV	(5) IV
Real BAfoeG (Simulated, in 100 €)	-.0128*	-.0112	-.0096	-.0179**	-.0026
	(.0073)	(.0073)	(.0073)	(.0091)	(.011)
First Generation	√	√	√	√	√
Migration Background	√	√	√	√	√
Apprenticeship	√	√	√	√	√
Sex	√	√	√	√	√
Living in City	√	√	√	√	√
Living with Parents	√	√	√	√	√
East Germany	√	√	√	√	√
Siblings	√	√	√	√	√
Year (Dummies)	√	√		√	√
Grant Share		√			
Period	2002–2019	2002–2019	2002–2019	2002–2013	2014-2019
N	3930	3930	3930	2246	1684
Cluster	1765	1765	1765	1058	896
R^2	.0735	.0746	.0643	.0699	.0777
Wald Chi ^{2}	177.32	184.96	140.37	106.98	91.43
RMSE	.47	.47	.48	.48	.46
Instrument (First Stage)	.0091***	.0090***	.0090***	.0090***	.0090***
	(.0001)	(.0001)	(.0001)	(.0001)	(.0002)

Cluster Robust Std. Errors in Parentheses (on ID);* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the estimated effect of real sim. BAfoeG on the probability of not taking up BAfoeG, now using 2SLS instead of IV probit, in different (time) specifications. (1) is the baseline, (2) is including grant share, (3) is without the year dummies,(4) is the same time period as Herber and Kalinowski (2019), and (4) is the remaining (new) period. Euros are expressed in 2002 euros, adjusted using the relevant price index.

Table 21: Non-Take-Up Mechanical BafoeG: Pooled IV in Different Specifications

Pr(Non-Take-Up Mechanical BAfoeG)	(1) IV	(2) IV	(3) IV	(4) IV	(5) IV
Real BAfoeG (Simulated, in 100 €)	-.0226***	-.0189***	-.0192***	-.0235***	-.0133
	(.0071)	(.0071)	(.0071)	(.0088)	(.0104)
First Generation	√	√	√	√	√
Migration Background	√	√	√	√	√
Apprenticeship	√	√	√	√	√
Sex	√	√	√	√	√
Living in City	√	√	√	√	√
Living with Parents	√	√	√	√	√
East Germany	√	√	√	√	√
Siblings	√	√	√	√	√
Year (Dummies)	√	√		√	√
Grant Share		√			
Period	2002–2019	2002–2019	2002–2019	2002–2013	2014-2019
N	4978	4978	4978	2816	2162
Cluster	2054	2054	2054	1227	1066
R^2	.0750	.0759	.0659	.0718	.0761
Wald Chi ^{2}	175.29	189.82	135.59	108.82	89.24
RMSE	.46	.46	.47	.47	.45
Instrument (First Stage)	.0083***	.0083***	.0081***	.0085***	.0080***
	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the estimated effect of real sim. BAfoeG on the probability of not taking up BAfoeG, now using 2SLS instead of IV probit and mechanical BAfoeG's non-take-up. This explicitly includes the non-take-up decision due to labor choice, meaning if someone is working instead of taking up BAfoeG, they are considered a non-take-up. (1) is the baseline equivalent, (2) includes grant share, (3) excludes year dummies, (4) covers the same time period as Herber and Kalinowski (2019), and (5) is the remaining (new) period. Euros are expressed in 2002 euros, adjusted using the relevant price index.

Table 22: Labor Supply – Intensive Margin: Pooled IV for Different Subgroups

Labor Supply (Hrs)	(1) IV	(2) IV	(3) IV	(4) IV
Real BAfoeG (Simulated, in 100 €)	-.4259**	-.1925	-.2733	-.2917**
	(.181)	(.1745)	(.2507)	(.1151)
First Generation		-.0426		.7618**
		(.5893)		(.3677)
Migration Background	-2.2491***			-1.8529***
	(.5501)			(.3677)
Apprenticeship	√	√	√	√
Sex	√	√	√	√
Living in City	√	√	√	√
Living with Parents	√	√	√	√
East Germany	√	√	√	√
Siblings	√	√	√	√
Year (Dummies)	√	√	√	√
N	4274	2862	1442	12589
Cluster	1498	1004	551	3824
R^2	.1339	.101	.126	.1126
Wald Chi^2	333.5	188.39	120.75	719.15
RMSE	12.98	11.66	11.64	12.72
Instrument (First Stage)	.0073***	.0080***	.0080***	.0073***
	(.0002)	(.0002)	(.0003)	(.0001)

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the estimated effect of real sim. BAfoeG on labor supply, intensive margin. now using 2SLS instead of IV probit, for the subgroups first generation and migration background. (1) is only first generation, (2) is migration background, (3) is both, and (4) is the whole sample baseline. Euros are expressed in 2002 euros, adjusted using the relevant price index.

Table 23: Labor Supply – Extensive Margin: Pooled IV for Different Subgroups

Pr(Labor Supply)	(1) IV	(2) IV	(3) IV	(4) IV
Real BAfoeG (Simulated, in 100 €)	-.0269***	-.0216***	-.0322***	-.0179***
	(.0068)	(.0076)	(.011)	(.0045)
First Generation		.0110		.0101
		(.2653)		(.0160)
Migration Background	-.0543**			-.0596***
	(.0233)			(.016)
Apprenticeship	√	√	√	√
Sex	√	√	√	√
Living in City	√	√	√	√
Living with Parents	√	√	√	√
East Germany	√	√	√	√
Siblings	√	√	√	√
Year (Dummies)	√	√	√	√
N	4274	2862	1442	12589
Cluster	1498	1004	551	3824
R^2	.0954	.0729	.0974	.0742
Wald Chi^2	250.05	125.31	91.42	539.87
RMSE	.48	.48	.47	.48
Instrument (First Stage)	.0073***	.0080***	.0080***	.0074***
	(.0002)	(.0002)	(.0003)	(.0001)

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the estimated effect of real sim. BAfoeG on labor supply, extensive margin. now using 2SLS instead of IV probit, for the subgroups first generation and migration background. (1) is only first generation, (2) is migration background, (3) is both, and (4) is the whole sample baseline. Euros are expressed in 2002 euros, adjusted using the relevant price index.

Table 24: Labor Supply – Extensive & Intensive Margin: Differences in Instruments

	Extensive Margin		Intensive Margin	
	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
Real BAfoeG (Simulated, in 100 €)	-.0179***	-.0477	-.2917**	-1.2972
	(.0045)	(.1292)	(.1151)	(3.439)
Instrument	Mech. BAfoeG	Max. Value BAfoeG	Mech. BAfoeG	Max. Value BAfoeG
First Generation	√	√	√	√
Migration Background	√	√	√	√
Apprenticeship	√	√	√	√
Sex	√	√	√	√
Living in City	√	√	√	√
Living with Parents	√	√	√	√
East Germany	√	√	√	√
Siblings	√	√	√	√
Year (Dummies)	√	√	√	√
N	12589	12589	12589	12589
Cluster	3824	3824	3824	3824
R^2	.0742	.0799	.1126	.1392
Wald Chi^2	539.87	526.54	719.15	750.47
RMSE	.48	.48	12.72	12.53
Instrument (First Stage)	.0074***	.0054***	.0073***	.0045**
	(.0001)	(.0001)	(.0001)	(.0022)

Cluster Robust Std. Errors in Parentheses (on ID); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the estimated effect of real sim. BAfoeG on labor supply, extensive and intensive margin. In both cases, instead of Mech. BAfoeG the conservative instrument of changes in the max. rate are used (2) & (4). As visible in the first stage coef., this is a much weaker instrument, however, yielding in effect size different estimates. Yet, none of these estimates are statistically significant. Euros are expressed in 2002 euros, adjusted using the relevant price index.

Table 25: Labor Supply – Intensive Margin: Estimated Coef. in Model with IV Estimation Including Different Start & End Points

Start	End	Estimated Coef.	Start	End	Estimated Coef.
2002	2005	-.0426	2006	2017	-.3443**
2002	2006	.0468	2006	2018	-.3067**
2002	2007	.0209	2006	2019	-.3541***
2002	2008	-.004	2007	2010	-.2114
2002	2009	-.0193	2007	2011	-.4511**
2002	2010	-.0793	2007	2012	-.4639**
2002	2011	-.2021	2007	2013	-.527***
2002	2012	-.231	2007	2014	-.4261**
2002	2013	-.2865**	2007	2015	-.4435***
2002	2014	-.244*	2007	2016	-.436***
2002	2015	-.2653**	2007	2017	-.4209***
2002	2016	-.2728**	2007	2018	-.3733***
2002	2017	-.2771**	2007	2019	-.4175***
2002	2018	-.252**	2008	2011	-.5445**
2002	2019	-.2917**	2008	2012	-.5391**
2003	2006	.1258	2008	2013	-.5969***
2003	2007	.0863	2008	2014	-.4729***
2003	2008	.0491	2008	2015	-.4852***
2003	2009	.0265	2008	2016	-.4686***
2003	2010	-.0483	2008	2017	-.4466***
2003	2011	-.1891	2008	2018	-.3928***
2003	2012	-.2221	2008	2019	-.4366***
2003	2013	-.2836*	2009	2012	-.6202***
2003	2014	-.2381*	2009	2013	-.6667***
2003	2015	-.2617*	2009	2014	-.5107***
2003	2016	-.2706**	2009	2015	-.5182***
2003	2017	-.2747**	2009	2016	-.4928***
2003	2018	-.2482**	2009	2017	-.464***
2003	2019	-.2903**	2009	2018	-.4031***
2004	2007	.0574	2009	2019	-.4479***
2004	2008	.0189	2010	2013	-.7486***
2004	2009	.0000	2010	2014	-.5529***
2004	2010	-.0848	2010	2015	-.5531***
2004	2011	-.2443	2010	2016	-.5158***
2004	2012	-.276*	2010	2017	-.4798***
2004	2013	-.3392**	2010	2018	-.4119***
2004	2014	-.2833*	2010	2019	-.4574***
2004	2015	-.3063**	2011	2014	-.5770***
2004	2016	-.3127**	2011	2015	-.5725***
2004	2017	-.3129**	2011	2016	-.523***
2004	2018	-.2817**	2011	2017	-.4805***
2004	2019	-.3248***	2011	2018	-.4042**
2005	2008	.0265	2011	2019	-.4556***
2005	2009	.0072	2012	2015	-.4384**
2005	2010	-.0956	2012	2016	-.4103**
2005	2011	-.2807	2012	2017	-.385**
2005	2012	-.3119*	2012	2018	-.3134*
2005	2013	-.3785**	2012	2019	-.3845**
2005	2014	-.3129**	2013	2016	-.3785*
2005	2015	-.3359**	2013	2017	-.3549*
2005	2016	-.3409**	2013	2018	-.2781
2005	2017	-.3384**	2013	2019	-.3631**
2005	2018	-.3037**	2014	2017	-.2528
2005	2019	-.3483***	2014	2018	-.182
2006	2009	.0914	2014	2019	-.2991
2006	2010	-.053	2015	2018	-.2632
2006	2011	-.2795	2015	2019	-.3826*
2006	2012	-.3151	2016	2019	-.3424
2006	2013	-.3907**			
2006	2014	-.3168*			
2006	2015	-.3426**			
2006	2016	-.3476**			