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Tuition Fees and Academic (In)Activity in Higher Education

How Did Students Adjust to the Abolition of Tuition Fees in Germany?

Johannes Berens, Leandro Henao, Kerstin Schneider¹

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Abstract: Five years after introducing tuition fees, the German state of North-Rhine Westphalia (NRW) issued their abolition in March 2011. Using student-level panel data, we assess the effects on academic activity and performance in two universities in NRW: a *state university* and a *private university of applied sciences* (UAS). We find that the increasing dropout rates at the state university do not necessarily point to lower ability or motivation, as an important share corresponds to ghost students, i.e., inactive students who use the university as a bridge to their professional or academic careers. In addition, we estimate causal effects only on active students who were susceptible to a behavioral adjustment. We consider cohorts that enrolled with tuition fees and employ two-way fixed effects models that account for effect heterogeneity across universities, cohorts, and study semesters. Students at the state university did not register for fewer exams but passed about 10% fewer credit points per semester after the reform, which is explained by a student effort effect. At the private UAS, students decreased their academic performance even more and were 9 p.p. more likely to withdraw from a registered exam. Prospective graduates drive the effects at both institutions, which explains the increased time-to-completion.

Keywords: tuition fees, academic activity, ghost student, TWFE, effect heterogeneity.

JEL classification: H52; I23; I28; H75.

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1. Introduction

Human capital plays a vital role in economic growth, leading modern economies to spend considerable resources on promoting higher education, among other education policies. High university-level attainment is the focus of the European Union's (EU) policy agenda.² However, prolonged study duration and decreasing success rates may threaten this objective in Germany.³ One way policymakers could foster student success is through funding: directly financing the institutions or channeling the funding through the students. In particular, a central instrument is a decision for or against student tuition fees, which has been a highly controversial topic in Germany.⁴ Tuition fees were in place in some federated states between 2006 and 2013, and while most authors have focused on the effects of student fees on enrollment, attainment, or self-reported academic activity, we examine two novel questions. First, we describe the changes in cohort composition around the abolition of tuition fees regarding academic activity level. Second, we analyze the causal link between this policy change and academic activity and performance adjustments at the individual level while accounting for cohort-specific and semester-specific effect heterogeneity, contrasting with other studies that rely on aggregated educational attainment to evaluate education policy changes.

Using tuition fees as a policy instrument must be considered carefully. On the one hand, fees might keep talented students from demanding higher education and adversely affect social mobility and diversity. On the other hand, student fees might have an efficiency-enhancing effect by signaling the value of education. In addition, in Germany, financial benefits of the student status yield incentives to enroll at a university, regardless of the intention to complete the studies. Not only do those monetary incentives distort the allocation of public funds, but they also distort the perception of successful and unsuccessful student careers. If students enroll but never intend to graduate, those students eventually drop out. In other words, the magnitude of the increasing student attrition problem in Germany, where dropout rates are reported to be around 30%, may be significantly smaller. Leaving the institution without a degree is not necessarily a symptom of failure. Instead, it depicts the strategic behavior of students enrolling in a study program as a bridge to their ulterior academic or professional careers. These individuals ought to be distinguished from students who aim at a degree but are unsuccessful.

This paper exploits the variation from abolishing moderate student fees of no more than EUR 500 per semester in Germany. We use a panel of individual student data collected by all higher education institutions in Germany (§3 HStatG, 1990). In contrast with other studies that have assessed the effects of the

² The EU set a higher education attainment goal of 45% by 2030 (The Council of the European Union, 2021).

³ Between 2010 and 2020, dropout rates increased from 35% to 39% (Heublein et al., 2020), and the median time to completion increased from 6.4 to 7.9 semesters (Ministry of Education, 2020).

⁴ Also, within the OECD, there is no standard rule regarding tuition fees, but rather some variation. According to OECD (2020) reports, public institutions charge no tuition fees for Bachelor or equivalent programs in nearly one-third of the member countries. In a similar number of countries, annual tuition fees are below USD 2,000, while in the remaining countries, fees range from about USD 2,600 to more than USD 8,000 per year.

introduction of tuition fees on students in the outdated pre-Bologna system, which was in place until the winter term of 2008/2009 in North Rhine-Westphalia (NRW), we estimate the effects of their abolition on students enrolled in the current two-tier system, with a particular focus on Bachelor students. Moreover, even though individuals enrolled in private higher education institutions account for over 11% of the students in Germany, most studies have neglected them when analyzing academic activity and educational attainment. We provide novel evidence on cohort composition changes and behavioral adjustment among students in a private university of applied sciences (UAS). Having the complete academic history of the students up to their disenrollment (graduation or dropout) allows us to describe the changes in student attrition and study duration in the context of the reform and approximate the causes behind them. Furthermore, unlike other studies, our estimation sample comprises the complete student body at both institutions, even though we consider several sources of effect heterogeneity, such as activity levels, cohorts, study progress, sex, or nationality.⁵

We can access rich demographic and academic information on students from two representative higher education institutions in NRW: a state university and a private UAS. Methodologically, we employ novel two-way fixed-effects (TWFE) estimators to investigate the effects of this policy change in a staggered treatment introduction framework. We address treatment effect heterogeneity by utilizing the estimator developed in Callaway & Sant’Anna (2021), which produces cohort-semester-specific treatment effects through 2×2 s difference-in-differences (DID) estimators. In addition, this empirical approach allows the estimation of average treatment effects on the treated (ATT) under more general conditions than in the classical TWFE and other state-of-the-art methods (e.g., Borusyak et al., 2023; Chaisemartin & D’Haultfœuille, 2020; Gardner & Jul, 2021), such as when the parallel-trends assumption only holds conditionally on covariates.

We use administrative data to track whether students registered, passed, failed, or withdrew from an exam. With this information, we can cluster former students of both higher education institutions according to their activity level. *Active* students register for at least one exam per semester, whereas *ghost* or *inactive* students do not register, take or withdraw from any exam during enrollment. Thus, in the first part of this paper, we extract some conclusions regarding the composition changes in the cohorts that enrolled right after the abolition of tuition fees. The observed increase in dropout rates at the state university can be largely attributed to ghost students, i.e., students who take advantage of the education system as a transitional step to their professional or academic careers. Indeed, the dropout share among active students becomes smaller after the reform. At the private UAS, the changes in cohort composition take longer to become visible.

⁵ In the winter term of 2021/2022, international students accounted for about 12% of all students in German higher education institutions. This share is above 19% in Berlin, and in NRW, it is 10%.

We use three outcome variables to assess the causal impact of the reform: credit points per semester to measure academic performance; and exam registrations and the probability of withdrawing from an exam to measure academic activity. The effects of this policy differ regarding the type of institution and the study progress of the cohorts. Considering only active students susceptible to a behavioral adjustment, our analyses provide evidence that the abolition of tuition fees is linked to lower academic performance among enrolled students: individuals complete, on average, 10% CP less per semester at the state university. While these effects are present even in older cohorts and become more prominent in the periods after treatment exposure, the changes in exam registrations and exam withdrawal remain, in all cases, statistically insignificant. In other words, the drop in academic performance is not correlated with a drop in formal academic activity but instead points to a student effort effect. At the private UAS, students react by formally registering for fewer exams and increasing exam withdrawal by 9 p.p. In this case, the drop in academic performance, which is much more significant in the first cohort to be treated than in older cohorts, cannot be untangled from the lower activity levels, which can be related to a strategic postponement of graduation.

In short, besides the increase in inactive or ghost students who would leave the university in any scenario, the reform explains the increase in time-to-completion at both universities through lower academic performance. In addition to accounting for effect heterogeneity from cohort and study-progress effects, we perform a subgroup analysis. By demographic groups, German nationals, female students, those who were younger than 25 at the moment of enrollment, and students with general university entry qualification exhibit a larger drop in academic performance at the state university after the abolition of tuition fees. At the private UAS, male students, those with alternative high school tracks, as well as German nationals show the stronger effects.

This paper contributes to the literature on the impact of financial incentives on student performance. Even in countries like the United States, where tuition fees have been part of the higher education systems, this policy instrument receives much attention. Allen & Wolniak (2019) find that a USD 1,000 increase in tuition fees is correlated with a 4.5% drop in campus ethnic diversity among full-time first-year students. In Europe, tuition fees are less common. Using individual-level administrative data from a state university in Spain, Beneito et al. (2018) estimate the effects of tuition fees on students' academic effort. They find that individuals who pay fees reduce the times they register for a single course before passing it and are more likely to pass in the first registration and improve their academic grades. Garibaldi et al. (2012) exploit data from Bocconi University to show that a EUR 1,000 increase in tuition fees in the last regular year of the program has a negative effect on the probability of late graduation. Ketel et al. (2016) investigate sunk-cost effects when tuition fees are reduced or abolished through a randomized experiment in a Dutch university but found no evidence of such costs. Sá (2019) provides evidence that tuition fees have a negative effect on applications to higher education, using data from the United Kingdom.

In Germany, federal law ruled out tuition fees between 1976 and 2005. However, between 2006 and 2014, state universities in some federal states charged tuition fees but eventually returned to the policy of no tuition fees. Researchers have been particularly interested in the effects of tuition fees on student enrollment, location choice, educational attainment, time to graduation, or students' budgets. Due to strict data protection laws, many of the answers to these questions have been based on either aggregated data (Alecke et al., 2015; Bietenbeck et al., 2023; Bruckmeier et al., 2015; Hübner, 2012) or survey data (Bahrs & Siedler, 2019; Dietrich & Gerner, 2012; Thomsen & Haaren-Giebel, 2016), with few exceptions (Dwenger et al., 2012). The way tuition fees were introduced and afterward abolished in some German states provided an ideal setting to approach questions of causality between this policy instrument and certain educational outcomes. Based on survey data Bahrs & Siedler (2019) provide evidence that tuition fees decrease enrollment intentions among secondary school graduates, especially from low-income households. Dietrich & Gerner (2012) and Hübner (2012) find that tuition fees reduce the likelihood that a high school graduate chooses higher education.

Alecke et al. (2015) present evidence for a negative effect of tuition fees on enrollment of male students and exclusively derived from interstate migration. Similarly, Bruckmeier & Wigger (2014) find no significant effect on aggregate enrollment in states with tuition fees. Dwenger et al. (2012) exploit the geographical dimension of this policy change and find that individuals from fee-charging states have a lower probability of applying for an institution in their home state after introducing tuition fees. A paper by Bietenbeck et al. (2023) is most closely related to our study. These authors focus on cohorts that started university in the outdated pre-Bologna programs before 2005 to show that tuition fees decrease university enrollment among secondary school graduates (extensive margin) but increase program completion among enrolled university students (intensive margin). In contrast with their work, we do not rely on self-reported student time use to approximate student effort but observe actual semesterly student activity and performance among students enrolled in the current Bachelor-Master system. Moreover, when discussing the adverse effects on the extensive margin, the authors cannot make any claim about the changes in academic activity levels. With this information, it would be possible to assess whether the average increase in educational attainment due to the introduction of tuition fees can be traced back to a smaller share of ghost students, an actual increase in student effort, or a combination of both. Lastly, Bruckmeier et al. (2015) also use this policy change as an exogenous shock in a natural experiment to show that introducing tuition fees at public universities decreased the time to graduation in the affected states.

The present paper also contributes to the literature on student attrition. Student enrollment is only the first step to increasing the proportion of the population with higher education. Students also need to graduate. Regarding the reasons for dropping out, Heublein (2014) enumerates the most common answers: performance problems, financial hardship, low study motivation, study conditions, or failure in

examinations. Heublein et al. (2020) report a 27% dropout rate in bachelor programs based on a representative survey of the 2018 graduate cohort. Blüthmann et al. (2012) apply clustering algorithms to survey data concerning dropout motivations. They find that 21% drop out due to strategic reasons, mainly students not intending to graduate. This is not only a German phenomenon. Pedziwiatr & Kugiel (2015) study the case of Indian students who registered at Polish universities to establish themselves in the EU and never intended to complete their studies. Cakitaki et al. (2021) analyze the ghosting behavior of Australian students who remain enrolled in one or more courses but exhibit no observable engagement in learning or assessment activities (non-participating enrollments). The OECD (2011) explicitly refers to fake students attracted to in-study benefits not tied to study success or engagement, for instance, in the Slovenian higher education system.

2. The Institutional Framework

German System of Higher Education

German education policy in the past decades focused on increasing the system's capacity rather than reducing the proportion of students who left their institutions without obtaining a degree.⁶ In particular, education policy is strictly regulated at the level of the federal states. Each state is responsible for designing its schooling and university system, but there are also commonalities. Generally, schoolchildren are allocated to different school tracks at the age of 10 years. After obtaining university entry qualification, high-school leavers can choose a higher education track in universities or universities of applied sciences (Kehm, 2013).⁷ Typically, universities are more academically oriented and are the more prestigious institutions. Universities of applied sciences are considered less theoretically demanding and more practically oriented than universities.

This paper focuses on tuition fees as a policy instrument, hence the funding of higher education, which is predominantly public in Germany.⁸ Moreover, the student status in Germany offers attractive benefits. Besides a wide range of discounts, students younger than 25 are eligible for child benefits and family health insurance. In addition, students have access to public transport in the federal state at a very low cost.⁹ Lastly,

⁶ Wolter (2004) notes that, in the 1970s and 1980s, the first federal law on higher education led to 30 new universities and 100 UAS to serve the ever-increasing number of students.

⁷ Higher education institutions in Germany may be state or non-state owned. In the winter term of 2021/2022, 342,586 students were enrolled in non-state or private higher education institutions, accounting for about 11% of all higher education students.

⁸ In NRW, the state we focus on, there are 41 state institutions of higher education: 14 universities, 16 universities of applied sciences, seven third-level institutions for arts and music, and 4 state-refinanced universities of applied sciences as of 2021. In addition, there is one private university, 23 purely private universities of applied sciences, and 5 administration universities.

⁹ The cost for the student ticket is 213 Euros per semester (6 months) at the SU in 2022. This compares to a 2.920 Euro for a regular ticket (SchönesJahrTicket NRW, 12 month, all of NRW).

international students obtain a residence permit with a work permit for at most 120 days per year. Besides the required university entry qualification and some degree programs with restricted admission, there are no barriers to enrollment. These financial and non-financial incentives to enroll, combined with a policy of not monitoring student achievement, give rise to ghost students, i.e., students that enroll but never intend to graduate. Those students, whose proportion is unknown, make the analysis of student success or student dropout even less straightforward.¹⁰

Tuition Fee Reforms

Despite being ruled out by federal law since 1976 (Bruckmeier et al., 2015), tuition fees in state universities were often suggested as a tool to improve teaching quality and incentives for higher education students. Making access to higher education costly should serve as a stimulus to the competition between institutions, even contribute to social redistribution, and provide price signals which make students consider opportunity costs more thoroughly when deciding about their studies. On the other hand, establishing tuition fees could become an obstacle to serving as many students as possible by increasing capacities and supporting social mobility. The political and academic discussions eventually led six federated states to challenge that law before the Federal Constitutional Court (FCC) in 2003. On 26 January 2005, the FCC ruled that the German states were to determine autonomously whether their public universities would charge tuition fees. Seven states chose to introduce tuition fees, but the decisions did not coincide or had the same scope. In particular, the state of NRW announced in March 2006 that its state universities would be allowed to charge fees up to EUR 500 per semester from the winter term of 2006/2007 (Alecke et al., 2015). In addition, two semesters later, a few private universities of applied sciences started charging tuition fees to students in those programs that the state had historically subsidized or *refinanced*.

The elimination of tuition fees in these federated states occurred just as asymmetrically between 2008 and 2014 (Bietenbeck et al., 2023), mainly due to political pressure and as an instrument in state election campaigns. NRW abolished tuition fees in public universities in March 2011, only five years after their introduction for state universities (GV NRW, 2011): newcomers and incumbent students would not have to pay for their studies from the winter term of 2011/2012 onward. Similarly, the state signed refinancing contracts in 2010 with four non-state universities of applied sciences so that they would not charge tuition fees in their historically subsidized study programs from the summer term of 2011. Whereas the abolition of tuition fees in state universities implied savings of EUR 500/semester for the students, tuition fees in private universities are much higher than that. Fees in most of these institutions are charged on a monthly basis, as opposed to semester tuition fees in state universities, and the amount can range from EUR 400 to

¹⁰ Although researchers in higher education have been aware of the issue of park or ghost students for many years, most German studies have been restricted to specific subject groups or university programs, such as Business Education, Social and Economic Sciences, Sociology, STEM, or Physics.

600/month as of 2021/2022. However, study programs at private institutions that were (partly) refinanced only charged EUR 500/semester for those programs.

Universities have yet to fully endorse the abolition of student fees because they benefited from extra revenue that was earmarked to improve the quality of the study programs. Therefore, the abolition of tuition fees led to state-specific programs that would, by and large, make up for the loss of student contributions at the universities. The state of NRW introduced the Quality Improvement Funds program as a substitute for the tuition fees that had been abolished. As were the tuition fees, these funds were exclusively earmarked for improving the quality of the study programs, and university-level commissions controlled the expenditures. Research activities, as well as administrative tasks, were explicitly excluded, as the only focus was the improvement of teaching activities and student conditions.

Other Reforms: Suspension of Compulsory Military Service and Civilian Service

One obstacle to identifying the effect of abolishing student fees is the more or less simultaneous reform of compulsory military service that might have affected male students. As mandatory enlistment of young men is bound to impact their educational and labor market trajectories¹¹, our analyses account for this reform, and we provide some background information upfront. In mid-December 2010, Germany suspended compulsory military service for men as of 1 July 2011, i.e., by the same time that tuition fees were abolished in NRW. Along with the suspension of military conscription in Germany, civilian or community service was suspended as well. In general, young men between 18 and 23 could be drafted. The duration of this compulsory service has been modified over the years. In 2002, it was shortened from twelve to nine months. Exceptions were made for medical reasons, marriage, holocaust victims and their relatives, or criminals. Women were also allowed to enter the *Bundeswehr* voluntarily, but only men were compelled. We account for the suspension of compulsory military service and civilian service, by performing a cohort-by-cohort analysis to examine the potential effect heterogeneity. Moreover, in addition to controlling for age-sex effects, we run our estimations on male and female students separately.

¹¹ Teachman (2005) finds a negative effect on schooling of the veteran status in the United States during the Vietnam War. However, military service may encourage young individuals to enroll in a post-secondary program afterwards. Brawner et al. (2017) highlight the military's role in widening educational prospects. Keller et al. (2009) extend the analysis to OECD countries but find that the intensity of military draft implementation negatively affects post-secondary educational attainment. For low-education enlistees, Card & Cardoso (2012) find that peacetime military service positively impacts wages at mid-career, probably due to the basic skills and occupational training they receive.

3. Data and Descriptive Analysis

Administrative Student Data

Our analyses utilize standardized administrative data collected by two higher education institutions in the state of NRW: a state university and a private university of applied sciences. According to the Higher Education Statistics Act (HStatG, 1990), all universities must gather and store demographic, academic, administrative, and performance-related information on the student level. Some variables are pre-determined and invariant: personal information (age, sex, birthplace, type of health insurance), previous education (type and place where university entry qualification was obtained, prior academic experience in semesters), and enrollment information (program, type and time of enrollment). The student performance data, reported at the end of each semester, include average grades, the average number of credit points (CP) earned, the number of exam registrations, and the number of passed, withdrawn, or unattended exams.¹²

Lastly, while direct information on family income or socioeconomic status is unavailable in the dataset, several variables can be used as useful proxies. Migration background is often associated with lower socioeconomic status and may be a helpful indicator of family income. Similarly, the type and grade of university entry qualification (UEQ)¹³ may reflect differences in educational opportunities and resources available to individuals (Dumont et al., 2019), which can be related to socioeconomic status. Moreover, municipality-level average income can also provide valuable information about students' economic conditions, which can be linked to their family income and overall socioeconomic status. Additionally, health insurance status may serve as a proxy for income, as individuals with higher income are likelier to have private health insurance.

Sample Restrictions

For our empirical analysis, we exclude enrollments in the pre-Bologna *Diplom/Magister* programs, i.e., individuals enrolled before the summer term of 2008 (HG NRW, 2006).¹⁴ Moreover, in our estimation sample, we only included Bachelor students aged between 18 and 55 years at the enrollment date, but we

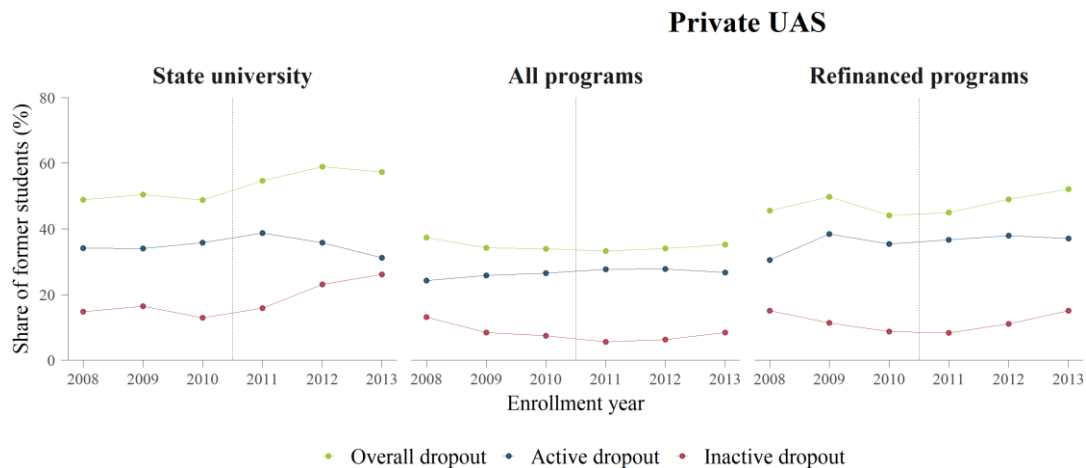
¹² See Berens et al. (2019) for further details on the variables.

¹³ The most popular secondary-level track is the *Gymnasium*. More recently, comprehensive schools where UEQ can be acquired have also faced increasing demand. Lastly, there are other paths for obtaining restricted UEQ after completing specific programs at lower school tracks (Schindler & Reimer, 2011).

¹⁴ Even though we estimate the effects of the abolition of tuition fees for students who enrolled before the winter term of 2011/2012 at the state university, and the summer term of 2011 at the private UAS, we show descriptive statistics of all the cohorts up to 2013. Due to another reform, which shortened the length of secondary schooling, the composition of the student body changed in 2013. The so-called G8 reform reduced the duration of secondary schooling from 9 to 8 years for students attending *Gymnasium*. Because of this reform, in 2013, two cohorts finished school and entered university simultaneously.

also present results for the unrestricted sample.¹⁵ In addition, as we cannot differentiate CP regularly achieved from credit transfers when changing a university during a study program, we generally exclude students with more than 60 CP in one semester from our estimation sample. We also provide results for different definitions of this threshold, for which 30 CP is the benchmark.¹⁶ Lastly, we account for the fact that the effect on student performance may be driven by inactive or *ghost* students, i.e., students who never enrolled for any exam when they were enrolled. For this purpose, we additionally estimate our empirical model on sub-samples restricted to active students, both graduates and dropouts.

Figure 1: Dropout Shares by Activity Level



Notes: These figures illustrate the share of former students enrolled between 2008 and 2013 in all Bachelor programs at the state university and the private UAS and the subset of refinanced Bachelor programs at the private UAS by status. The figures display overall dropout shares and dropout shares by activity level. Active dropouts registered for at least one exam each semester. Inactive dropouts did not register, take or withdraw from any exam.

Figure 1 illustrates how large the phenomenon of ghost students is in these two German universities. Before the abolition of tuition fees, the share of former students who dropped out of the state university without registering for a single exam or completing any credit points was about 15%. Interestingly, the reform in 2011 was followed by an increase in the share of inactive dropouts up to about 25%. Simultaneously, the share of active but unsuccessful students or active dropouts fell drastically. While this is merely descriptive evidence, we observe that the increasing trend in dropout rates at the state university (from 50% to 60% between 2010 and 2012) may be explained by a larger share of students who enrolled in a study program but were utterly inactive during the time they remained enrolled. The absence of tuition fees provided a further incentive to enroll in a higher education institution to individuals waiting to be accepted in their desired university or program or while searching for a job or vocational training position.

¹⁵ Figure 6 in the Appendix illustrates the distribution of the age at enrollment at the state university and the private UAS.

¹⁶ Figure 7 provides the estimated distribution of the number of credit points per semester at the state university and the private UAS.

At the private UAS, the dropout and inactive shares are smaller than at the state university. However, if we restrict the analysis to the refinanced programs, i.e., where tuition fees were abolished, dropout shares are similar to those at the state university, but still small shares of ghost students. This suggests that the abolition of tuition fees in this handful of programs may have impacted the cohort composition differently: it did not immediately attract a larger share of ghost students. Theoretically, keeping inactive students in the state university and private UAS estimation samples should lead to an underestimation of the effects, as these individuals cannot show any behavioral adjustment, which is why we exclude them from our baseline computations.

Descriptive Statistics

Table 1 summarizes the data and points to the differences between the two institutions in our dataset. Regarding the student body composition, the state university attracts younger students than the private UAS, where the students are, on average, two years older. The share of male students is more prominent (65%) at the private UAS than at the state university (47%). PUAS students also have a much more heterogeneous academic background. They have 0.15 semesters more of academic experience from other universities and are 17 p.p. more likely to have obtained a non-general UEQ at institutions different from Gymnasiums. On the other hand, students at the state university remain, on average, longer enrolled (8 semesters), complete less CP, pass fewer exams, and obtain worse grades in their first semester than those at the private UAS.

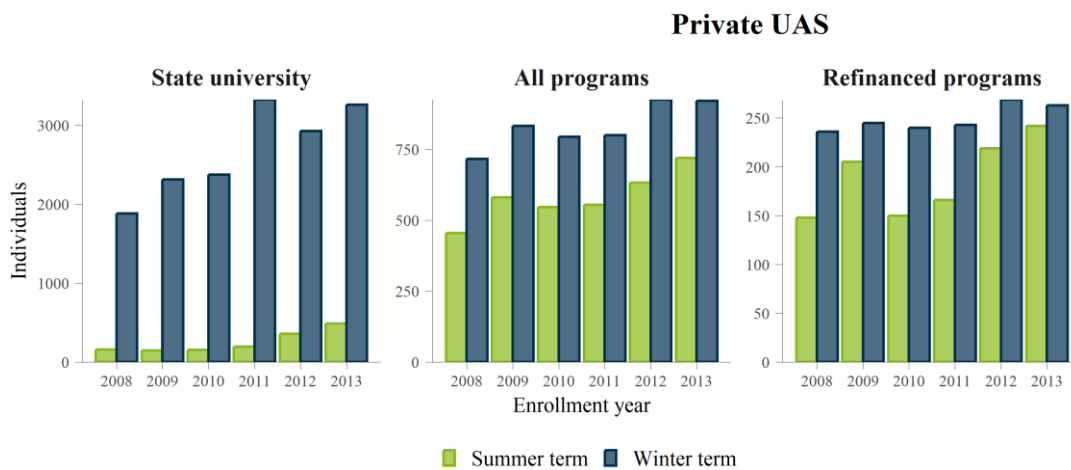
Table 1: Descriptive Statistics by Institution

Variable	State university			Private UAS			Diff.	p-val.
	Mean	SD	N	Mean	SD	N		
Age at enrollment	21.56	3.85	17,586	23.59	4.42	8,480	-2.03	0.00
Male	0.47	0.50	17,586	0.65	0.48	8,480	-0.17	0.00
Foreign	0.10	0.30	17,586	0.11	0.31	8,480	-0.01	0.04
Migration background	0.33	0.38	17,586	0.35	0.39	8,480	-0.02	0.00
Distance in km	44.95	117.72	17,586					
Privately insured	0.13	0.34	17,586					
Age out of bound	0.04	0.19	17,586	0.02	0.12	8,480	0.02	0.00
General UEQ	0.66	0.47	17,586	0.49	0.50	8,480	0.17	0.00
Acad. exp. in sem.	1.13	3.35	17,586	1.27	3.04	8,480	-0.15	0.00
Winter enrollment	0.91	0.28	17,586	0.59	0.49	8,480	0.33	0.00
Second study	0.07	0.25	17,586					
STEM program	0.62	0.49	17,586	0.39	0.49	8,480	0.23	0.00
CP in sem. 1	11.71	13.28	17,586	16.89	15.32	8,480	-5.17	0.00
GPA in sem. 1	2.57	0.73	11,311	2.38	0.68	7,119	0.19	0.00
Dropout share	0.52	0.50	17,586	0.35	0.48	8,480	0.17	0.00
Study duration in sem.	7.87	5.39	17,586	6.51	3.57	8,480	1.36	0.00
Study duration > 10 sem.	0.24	0.43	17,586	0.08	0.27	8,480	0.16	0.00
Study duration > 14 sem.	0.11	0.31	17,586	0.03	0.18	8,480	0.08	0.00
Over 30 CP in one sem.	0.49	0.50	17,586	0.49	0.50	8,480	0.00	0.78
Over 60 CP in one sem.	0.07	0.26	17,586	0.03	0.16	8,480	0.04	0.00

Notes: This table presents descriptive statistics (mean, standard deviation, and number of observations) for variables in the sample of students enrolled between 2008 and 2013 in all Bachelor programs at the state university and the private UAS. It also presents the results of mean-difference tests (estimate and p-value) for all variables available at both universities.

Figure 2 shows the number of new enrollments in the two universities by cohort. In the relevant period, the public university received on average about 2,700 new students each year, whereas the average number of new enrollments at the private UAS was 1,200. Especially at the state university, we observe an increase in enrollments in 2011 and 2012. Regarding the enrollment schedule, the private institution displays a more balanced distribution between winter and summer enrollment than the state university. Summer enrollments at the state university are not that common because study programs typically start in the winter semester.

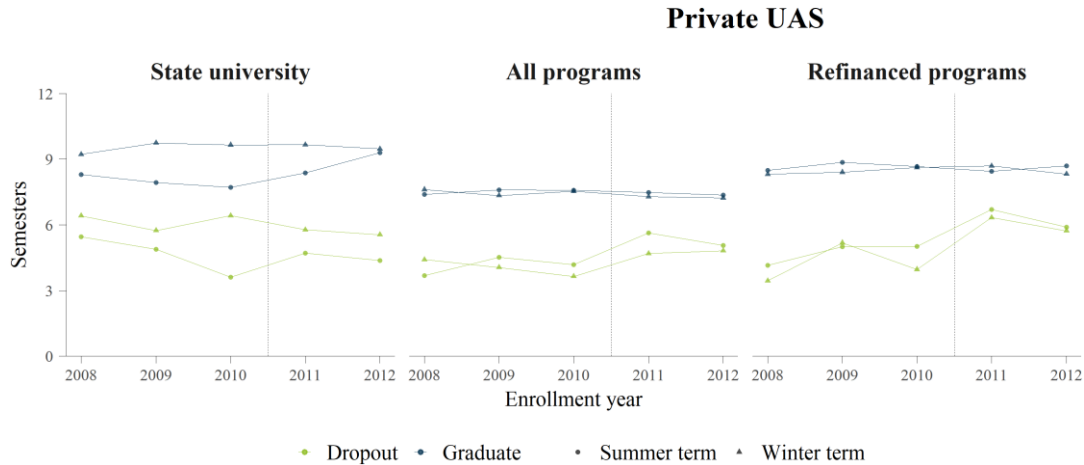
Figure 2: Enrollment Schedule



Notes: These figures illustrate the absolute number of new enrollments between 2008 and 2013 in all Bachelor programs at the state university and the private UAS and the subset of refinanced Bachelor programs at the private UAS, indicating whether they enrolled in a winter or a summer term.

Moreover, as Figure 3 shows, individuals remain, on average, longer enrolled at the state university than at the private UAS, even though the regular duration of a Bachelor’s program in most cases is six semesters at both universities. There are substantial differences between summer and winter cohorts at the state university. The few students who enroll in a summer term leave the institution quicker than those in the winter. We also observe that the average time to drop out at the private UAS increases substantially at the private UAS after the abolition of tuition fees in the refinanced programs. However, to provide a more transparent comparison across cohorts, we also contrast the students’ academic outcomes concerning their study semester, not their enrollment year. Figure A8 in the Appendix provides an overview of two critical dimensions related to study success: academic activity, measured in the number of exam registrations, and academic performance, measured in the number of credit points. We observe that all cohorts display an inverted U-shape pattern, where the highest peak of activity is reached in the fifth semester after many inactive students have dropped out.

Figure 3: Time to Drop Out and Graduation



Notes: These figures illustrate the study duration in semesters of all former students enrolled between 2008 and 2012 in all Bachelor programs at the state university and the private UAS and the subset of refinanced Bachelor programs at the private UAS by status and enrollment schedule.

Figure A6 in the Appendix provides descriptive evidence of how the composition of the student cohorts, concerning their age at enrollment, changed during the sample years. There are remarkable differences between the summer and winter cohorts. Summer cohorts at the state university enroll at much older ages, whereas this gap is not that relevant at the private UAS. Due to the effect of the military service reform, we expected to observe a change in the age distribution, especially for male students. We find descriptive evidence that this effect is not relevant at the private UAS. Focusing on the enrollments in the winter term, which are much more common than in the summer, the change at the state university is relatively small for all students. Differentiating by gender, this figure confirms that male students did enroll in the state university at slightly younger ages after 2010. However, the enrollment age of female students increased, and the age gap between male and female students is closed. As an institution that usually attracts students with some labor market experience and is, by definition, older, the private UAS barely shows changes in this respect.

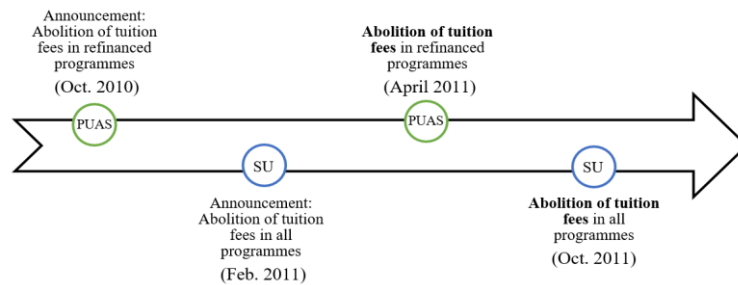
4. Empirical Strategy

Staggered Treatment Introduction Framework

We have presented descriptive evidence that the abolition of tuition fees is correlated with changes in the cohort composition of both universities regarding their academic activity. The observed increase in dropout rates at the state university (from 50% in 2010 to 60% in 2012) after the abolition of tuition fees does not necessarily mean, for instance, that the university started to attract students of lower ability. Instead, the policy change is correlated with a larger share of inactive dropouts. In other words, it attracted more students who would enroll for very few semesters without any intention to complete their degrees and then leave the institution after showing no academic activity. Now, we will focus on the cohorts who enrolled

just before the implementation of the reform. Keeping the cohort composition fixed, we can estimate the causal effect of the abolition of student fees on academic activity: how the students at these two universities adjusted their behavior. Our empirical approach allows us to examine these effects cohort by cohort and semester by semester, thereby explicitly considering the potential effect heterogeneity. Our empirical strategy is guided by the timing of the reform and the particularities of the two universities illustrated in Figure 4.

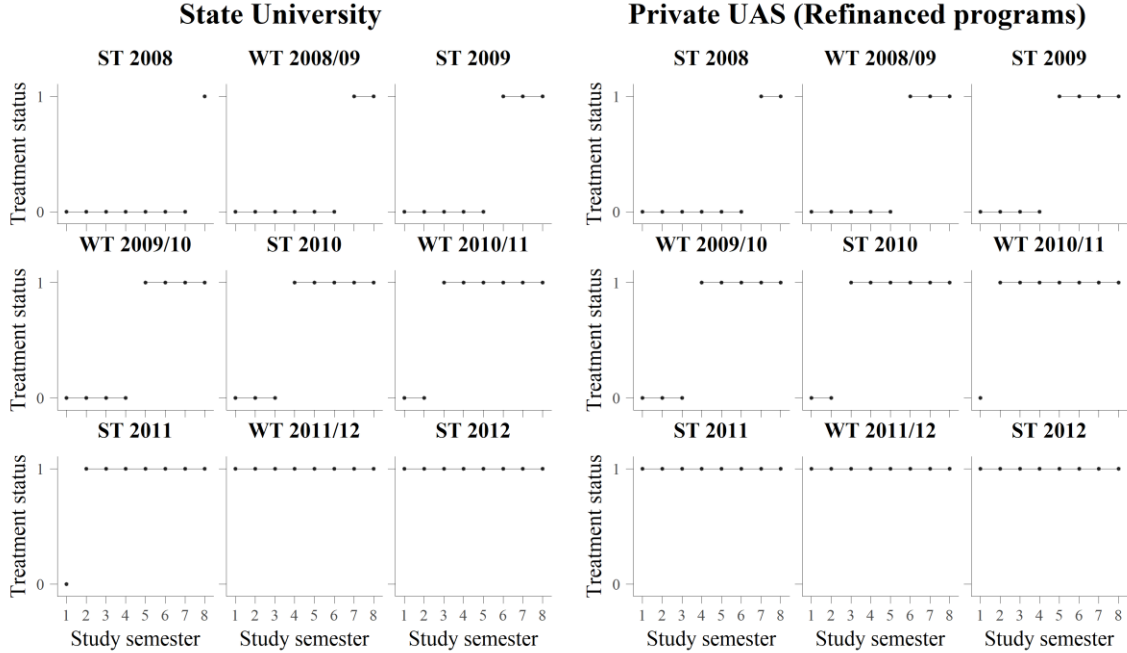
Figure 4: Reforms in Higher Education



Notes: This figure illustrates the tuition fee reform schedule at the state university (SU) and the private university of applied sciences (PUAS) in our data.

Since we have a panel of students over the time they remain enrolled at their institution, we can estimate the reform’s effect by comparing groups of students who experience exposure to treatment at different points in their academic careers, using two-way fixed effects models. Furthermore, as noted above, we focus on the group of enrolled students and exclude new enrollments, which allows us to estimate the effects of the abolition fees on the intensive margin of academic activity and performance. As a result, our estimations are not affected by changes in the composition of the cohorts. The abolition of tuition fees took place in the summer term of 2011 in some study programs at the private UAS and in the winter term of 2011/2012 in all study programs at the state university. This implies that, while we cannot use a classical DID approach, we can exploit the panel structure of our data and the fact that each cohort was affected by the reform at different study semesters and thus find valid control units. Lastly, concerning the private UAS, we apply our empirical strategy only to the group of study programs affected by the reform (refinanced programs), as this subsample is more similar to the state university study programs regarding pre-reform tuition fee levels.

Figure 5: Treatment Schedule by Institution



Notes: This figure illustrates the tuition fee reform schedule by cohort at the state university and the private university of applied sciences (UAS) in our data. Here, the horizontal axis represents the students' relative study semester. Since the reform occurred in the winter term of 2011/2012 at the state university, and in the summer term of 2011 at the private UAS, these two cohorts are always treated.

Provided the estimated treatment effect is constant across groups and over time, TWFE models identify the effect on the outcome of interest, for instance, the number of credit points per semester, under the parallel trends assumption (Chaisemartin & D'Haultfoeuille, 2021).¹⁷ The literature has raised doubts about interpreting the estimated TWFE coefficients in such a setting, with staggered treatment introduction, as causal effects (Chaisemartin & D'Haultfoeuille, 2021; Sun & Abraham, 2021). Goodman-Bacon (2021) look at the static TWFE estimator and note that it is a variance-weighted average of all possible two-group/period DID estimators in the data. The TWFE estimates may thus differ from the sample-ATT, as these weights applied by ordinary least squares (OLS) are implicitly positive. Therefore, the TWFE estimates may not have the same sign as the average ATT. Moreover, Baker et al. (2022) provide a useful overview of the latest innovations concerning the estimation of ATT in staggered DID designs. This line of research points to the fact that in staggered treatment introduction frameworks, where treatment effects cannot be assumed to be homogeneous across time groups, classical DID estimates are prone to be biased.¹⁸

¹⁷ The basic specification of the TWFE is: $Y_{it} = \alpha_i + \alpha_t + \beta^{TWFE} D_{it} + u_{it}$ with β^{TWFE} being the parameter of interest. Here the subscripts i and t refer to the student and time, respectively. α_i is an unobserved individual-specific effect and α_t is an unobserved time-specific effect. D_{it} is a dummy variable that takes value one if the student is treated, i.e., the student does not have to pay tuition fees. Y_{it} represents a time-varying dependent variable.

¹⁸ Flexible TWFE regressions or event-study estimators are also problematic, as estimates from other periods contaminate the relative effects in a given period.

Figure 5 displays the complete treatment schedule for both institutions, regarding the cohorts that we can potentially include in our estimation sample. The left panel shows the timeline for the state university. Students in this pooled sample will be treated in different study semesters. For instance, the cohorts enrolled in the winter term of 2011/2012 and summer term of 2012 are always treated and thus excluded from our final estimation sample. The cohort enrolled in the winter term of 2010/2011 was first treated in the third semester but not treated in the first and second semesters. Similarly, the cohort enrolled in the winter term of 2009/2010 joined the treatment group in the fifth study semester. The cohort enrolled in the summer term of 2008 is the oldest in our sample and was first treated in semester 8.

Identification Strategy

We have illustrated how our setting differs from a canonical DID setup, mainly because we have variation in treatment timing. Thus, in this section, we elaborate on our empirical strategy to correctly identify treatment effects, which follows Callaway & Sant’Anna (2021). For each student, we observe the T semesters that they remain enrolled, and thus we denote a particular semester by t , where $t = 1, \dots, T$. Let D_{it} be a binary variable equal to one if student i is treated, i.e., exempted from paying tuition fees, in semester t and zero otherwise. We assume that no students are treated at *time* or semester $t = 1$, i.e., in their first semester, so we exclude cohorts exempted throughout all their study time. Moreover, once a student is treated, that student remains treated in the next period.¹⁹ We further define G as the study semester (first, second, third, etc.) when a student first becomes treated. Thus, for all students that are eventually treated, G defines which *group* or cohort they belong to. Next, we define G_g as a binary variable that takes value one if a unit is first treated in period g and C as a binary variable that is equal to one for students that are never treated.²⁰ In this setting, $Y_{it}(0)$ denotes student i ’s untreated potential outcome at semester t if they remain untreated through period T . Moreover, for $g = 2, \dots, T$, $Y_{it}(g)$ denotes the potential outcome that student i would experience at semester t if they receive the treatment in period g . In particular, we consider the following dependent variables in the main results: number of exam registrations, credit points, and probability of withdrawing from an exam. The observed and potential outcomes for each student i are related through Equation (1).

Since different potential outcomes cannot be observed for the same student in the same semester, Callaway & Sant’Anna (2021) proposes a natural generalization of the ATT parameter from the canonical DID setting to be suitable for setups with multiple treatment groups and periods. Equation (2) illustrates the average treatment effect for students who are members of a particular cohort g in a particular semester t , or

¹⁹ This assumption is in line with the characteristics of the reform, as not even long-term students are charged tuition fees after their abolition.

²⁰ This may happen, for instance, if a student had been disenrolled before the policy change took place.

cohort-semester average treatment effect. This parameter does not restrict treatment effect heterogeneity across cohorts or semesters, thus allowing us to assess how the treatment effects change over time for each cohort of students that is eventually exempted from paying tuition fees.

To identify the $ATT(g, t)$ parameters, Callaway & Sant’Anna (2021) impose a limited treatment anticipation assumption²¹ and, more importantly, a conditional parallel trends assumption based on either *never-treated* or *not-yet-treated* groups. Since we focus on not-yet-treated students in the main results of this paper, in Equation (3), we only elaborate on the latter type of parallel trends assumption.²² Thus, in each semester t , a student may either be member of group g with $G_g = 1$, or a member of the not-yet-treated group if $t \leq s$. Since this assumption holds after conditioning on a set of covariates X , we can control for cases with covariate-specific trends in the outcomes over time and, of course, if the distribution of covariates is different across groups.²³ In our case, X includes age at enrollment, a male dummy, previous academic experience in semesters, and type of UEQ. Lastly, Callaway & Sant’Anna (2021) impose an overlap assumption, which states that a positive fraction of the population starts treatment in semester g , and that, for all g and t , the generalized propensity score $p_{g,t}(X)$ is uniformly bounded away from one.

Based on these assumptions, Callaway & Sant’Anna (2021) identify in Equation (4) the $ATT(g, t)$ parameters, which are computed using doubly-robust estimation, where $m_{g,t}(X) = \mathbb{E}[Y_t - Y_{g-1}|X, D_t = 0, G_g = 0]$ is the outcome regression for the not-yet-treated group by semester t . Thus, the average treatment effect for students in cohort g is identified by taking the path of outcomes experienced by that cohort and adjusting it to the path of outcomes experienced by the not-yet-treated comparison group. Moreover, these estimates can be aggregated very flexibly to explore interesting questions regarding treatment effect heterogeneity. In our main results, we present the following aggregations. First, aggregations into overall treatment effect parameters, where we average all the $ATT(g, t)$ parameters together. Second, aggregations into cohort-specific treatment effect parameters, the average effect of participating in the treatment among units in cohort g , across all their post-treatment periods. Third, aggregations into event study-type parameters, which represent the average effect of participating in the treatment e time periods after the treatment was introduced across all groups that are ever observed to take part in the treatment for exactly e periods.²⁴

²¹ This assumption allows for anticipation behavior, providing that the anticipation horizon δ is known to the researcher. We set $\delta = 0$ in our empirical application.

²² Results when using *never-treated* students as comparison groups are available upon request.

²³ This feature of the Callaway & Sant’Anna (2021) estimator is valuable in our setting, as it allows us to control for changes in the cohort composition due to the military service reform, as pointed out in Section 3, and for other observable characteristics of the students which may influence their academic progress.

²⁴ The estimation of the $ATT(g, t)$ parameters and all aggregations have been performed on R using the software implemented by Callaway & Sant’Anna (2023).

In our robustness checks, we also provide the results obtained from other estimation methods which have been proposed to deal with the identification challenges in staggered treatment introduction frameworks. In particular, besides the naive TWFE estimator, we consider the two-stage DID estimator suggested by Gardner & Jul (2021), the imputation-based estimation method proposed by Borusyak et al. (2023), the interaction-weighted estimator by Sun & Abraham (2021), and the stacked regression estimator as pointed out by Baker et al. (2022). In contrast with the estimator proposed by Callaway & Sant’Anna (2021), the approaches by Gardner & Jul (2021), Borusyak et al. (2023), and Sun & Abraham (2021) rely on unconditional parallel-trends assumptions for identification, Borusyak et al. (2023) require no effect anticipation, and Sun & Abraham (2021) also assume treatment effect homogeneity within cohorts.²⁵

5. Results

Strategy Validation

Before analyzing the estimation results, we take a closer look at the evolution of the outcomes over time, i.e., study semester, to check whether the treated and control units from different cohorts evolved similarly for our main three outcomes: number of credit points, number of exam registrations, and the probability of withdrawing from an exam.²⁶ While the parallel trends assumption cannot be directly tested, the estimated $ATT(g, t)$ parameters provide an approximation. Figures **A9** and **A10** in the Appendix illustrate how conditioning on observable pre-determined characteristics of the students, i.e., age, sex, type of university entry qualification, previous academic experience and nationality, makes the fulfillment of this assumption among active students more plausible, especially at the state university.²⁷ Under the null hypothesis of the fulfillment of the parallel trends assumption in all periods, the estimated $ATT(g, t)$ for pre-treatment periods in red should be statistically insignificant (Callaway & Sant’Anna, 2021).²⁸ Considering 95%-confidence intervals, we observe that in all cohorts, these estimates are not statistically different from 0, except for the summer term of 2009 and the winter term of 2010/2011 cohorts at the state

²⁵ The estimation of these models has been performed using the software implemented by Berge (2023), Butts & Gardner (2023), and Butts (2022).

²⁶ Figure **A8** in the Appendix provides descriptive of the similar patterns found across cohorts.

²⁷ Upon request, we provide analogous illustrations for the unrestricted sample, graduates and dropouts subsamples, and the rest of the outcomes considered in this paper.

²⁸ The post-treatment estimates in blue suggest that after being exempted from paying tuition fees, active students at the state university reacted by decreasing their performance measured in credit points.

university and the summer term of 2010 cohort at the private UAS, for which there are two periods where the pre-treatment $ATT(g, t)$ are, however, very small.²⁹

Effects on Academic Activity and Performance

Based on the assumptions discussed in Section 4, we can interpret the cohort-semester ATT or $ATT(g, t)$ in a causal way. The next step consists in aggregating these estimates into parameters that are easier to interpret and thus more useful to policy-makers. The first parameter we are interested in is the *overall* effect of being exempted from paying tuition fees, which can be computed as the weighted average of the $ATT(g, t)$, where larger groups receive larger weights as in Equation.³⁰

Table 2: Estimated Average Treatment Effects on Credit Points per Semester

Subsample	(1) TWFE	Callaway & Sant'Anna			(5) IBE	N	Pre-means
		(2) Cond.	(3) Uncond.	(4) 2SDID			
<i>State University</i>							
All students	-0.16 (0.20)	-1.05*** (0.32)	-0.95*** (0.30)	-1.35*** (0.08)	-2.01*** (0.78)	5,742	10.6
Active students	-0.23 (0.21)	-1.13*** (0.32)	-1.04*** (0.33)	-1.52*** (0.15)	-2.39*** (0.87)	4,614	13.2
Dropouts	-0.38 (0.32)	-0.24 (0.53)	-0.25 (0.50)	-1.03*** (0.37)	-1.19 (1.16)	2,209	9.5
Graduates	-0.21 (0.28)	-1.53*** (0.43)	-1.41*** (0.40)	-1.73*** (0.11)	-3.32*** (1.21)	2,405	16.6
Unrestricted sample	0.43 (0.28)	-1.67*** (0.41)	-1.64*** (0.38)	-1.60*** (0.31)	-3.56* (1.82)	7,074	12.0
<i>Private UAS</i>							
All students	-1.54*** (0.40)	-2.55*** (0.56)	-2.58*** (0.50)	-2.55*** (0.43)	-1.20* (0.64)	1,191	6.8
Active students	-1.66*** (0.41)	-2.65*** (0.56)	-2.66*** (0.53)	-2.66*** (0.51)	-1.40** (0.65)	1,064	7.6
Dropouts	-1.72*** (0.64)	-2.30* (1.20)	-1.97** (0.93)	-1.27** (0.50)	-1.19 (0.94)	421	5.5
Graduates	-1.84*** (0.45)	-2.56*** (0.58)	-2.84*** (0.56)	-3.39*** (1.12)	-2.81*** (0.70)	643	9.0
Unrestricted sample	-1.93*** (0.47)	-3.41*** (0.66)	-3.52*** (0.70)	-3.56*** (0.79)	-2.16*** (0.80)	1,206	7.5
Controls	No	Yes	No	No	No		
Student fixed effects	Yes	No	No	Yes	Yes		
Semester fixed effects	Yes	No	No	Yes	Yes		

This table summarizes the estimation results of a selection of DID models applied on different definitions of the sample of students enrolled between 2008 and 2011. The cohorts of always-treated students (WT 2011/2012 at the state university and ST 2011 at the private UAS) are excluded. Our baseline results are based on the method suggested by Callaway & Sant'Anna (2021).

²⁹ In Table **B7** in the Appendix, we exclude these cohorts for which the parallel-trends assumption may be violated and find that our results are pretty similar.

³⁰ $\theta^o = \frac{1}{\kappa} \sum_{g \in \mathcal{G}} \sum_{t=2}^{\mathcal{T}} \mathbb{1}\{t \geq g\} ATT(g, t) \mathbb{P}(G = g | G \leq \mathcal{T})$, where $\kappa = \frac{1}{\kappa} \sum_{g \in \mathcal{G}} \sum_{t=2}^{\mathcal{T}} \mathbb{1}\{t \geq g\} \mathbb{P}(G = g | G \leq \mathcal{T})$ makes sure that the weights sum up to one.

	Callaway & Sant'Anna						
Subsample	(1) TWFE	(2) Cond.	(3) Uncond.	(4) 2SDID	(5) IBE	N	Pre-means

As a robustness check, we provide results from alternative methods: a simple TWFE model, the two-stage DID model (2SDID) suggested by Gardner & Jul (2021), and the imputation-based estimation (IBE) method suggested by Borusyak et al. (2021).

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Robust standard errors in parentheses.

In Table 2, we present these θ^o aggregations for different definitions of our estimation sample and two specifications of the parallel-trends assumptions in the Callaway & Sant'Anna (2021) estimator: unconditional and conditional on covariates. We also provide the results of a simple TWFE model³¹ with student and semester-fixed effects for comparison purposes. Lastly, we show the results from two alternative estimation methods suggested to overcome the limitations of the canonical difference-in-differences models in settings with staggered treatment introduction. In particular, we estimate the two-stage DID (2SDID) model by Gardner & Jul (2021) and the imputation-based estimation method (IBE) by Borusyak et al. (2023).³² All the estimates suggest a statistically significant negative effect on the average number of credit points per semester that the students complete, except the results from the naive TWFE estimator, where most estimated effects are negative but not statistically significant.

Models (2) and (3), which correspond to the Callaway & Sant'Anna (2021) estimator, indicate that while *All students* at the state university complete about 1 CP less per semester, these effects differ regarding their final status. As expected, the effects are driven by active students, especially prospective graduates, who may complete up to 1.5 CP less on average. In contrast, active dropouts, i.e., those who registered for at least one exam, show a negative behavioral adjustment, but the estimates are not statistically significant. Columns (4) and (5) provide further evidence for the robustness of these results. While the estimates are quantitatively larger, we observe that in all cases, it is prospective graduates who show the largest drop in academic performance: 1.7 CP according to the Gardner & Jul (2021) estimator and 3.3 CP according to Borusyak et al. (2023). These static estimates are also negative and significant at the private UAS: students reduce, on average, their academic performance by 2.6 CP per semester.³³ In this institution, it is also prospective graduates who exhibit the largest drop, according to all estimation methods, but future dropouts also react significantly.³⁴

³¹ $Y_{i,t} = \beta_0 + \beta_1 \cdot D_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$

³² Further estimation methods are available in the Appendix (see Table 6); in particular, the interaction-weighted estimator by Sun & Abraham (2021), and the stacked regression estimator as pointed out by Baker et al. (2022).

³³ Since there were programs at the private UAS that did not change their tuition-charging policy in 2011, we provide some placebo tests in Table B5 in the Appendix. Indeed, simulating the same reform in these study programs leads to positive but statistically insignificant results across all models except Borusyak et al. (2023).

³⁴ Table B7 in the Appendix presents analogous results when we exclude cohorts for which the parallel-trends assumption may be violated: the summer term of 2009 and the winter term of 2010/2011 cohorts at the state university and the summer term of 2010 cohort at the private UAS. The overall estimates remain negative and significant, except for the Borusyak et al. (2023) model. In our preferred specification, the Callaway & Sant'Anna (2021) estimator under a conditional parallel trends assumption, the point estimates are larger in absolute value. Active students reduce their academic performance on average by 2.5 CP per semester, about 10%, with respect to the pre-treatment level.

Now we consider the number of exam registrations per semester to measure academic activity. Interestingly, we observe in Table **B3** that the students at the state university do not show any behavioral adjustment. In other words, keeping cohort composition and the number of exam registrations fixed, we estimate a decrease in academic performance measured in credit points among prospective graduates, most likely due to reduced effort. In contrast, students at the private UAS register for fewer exams after the abolition of the tuition fees, which can explain the drop in credit points, as these two outcomes are correlated. The effects on the third outcome illustrated in Table **B4**, the probability of withdrawing from an exam, align with these findings. Students at the state university are 2.5 p.p. less likely to withdraw from an exam. Therefore, these students kept their exam registration patterns and had fewer incentives to formally withdraw from an examination than their fee-paying counterparts. Clearly, only lower student effort could explain how their performance worsens even after registering the same amount of exams and withdrawing from fewer than the control units. At the private UAS, the probability of withdrawing from an exam increased on average by 9.2 p.p. and up 12.7 p.p. among prospective dropouts.

In short, we have found evidence that the abolition of tuition fees led to behavioral adjustment among students at both institutions. Especially active students, who were most likely interested in completing their studies, reacted by decreasing their academic performance. In particular, the reduction in academic achievement among prospective graduates necessarily leads to a more extensive time to completion at both institutions. In particular, at the state university, the drop in academic performance measured in credit points per semester, while keeping the number of exam registrations fixed, points at a lower student effort. Lastly, future dropouts, active students with low-performance levels, become inactive even faster at the private UAS before leaving the institution, as their withdrawal probability increases after the reform. Figures **A11** and **A12** complete this picture by illustrating another type of aggregation of the estimated $ATT(g, t)$ parameters. In particular, we compute θ_{es} , i.e., the average effects across different lengths of exposure to the treatment (event study); here, the overall impact averages the effect of the treatment across all positive lengths of exposure.³⁵

On the one hand, these dynamic estimates provide further evidence of fulfilling the parallel trends assumptions. Indeed, these $\theta_{es}(e)$ parameters are not statistically significant for all the pre-treatment periods. On the other hand, regarding the post-treatment periods, we observe that the reform does not only affect the period when the abolition of tuition fees takes place but also in the subsequent semesters. At the state university, the effects disappear after the third semester after treatment. In contrast, the estimates at the private UAS become larger in absolute value and remain significant four semesters after treatment. Concerning the number of exam registrations

³⁵ $\theta_{es}(e) = \sum_{g \in G} \mathbb{1}\{g + e \leq T\} \mathbb{P}(G = g | G + e \leq T) ATT(g, g + e)$

By student subgroups, we observe in Figures **B14** and **B15** that older cohorts show the most statistically significant adjustment at the state university. In contrast, the WT 2010/2011 cohort drives the results at the private UAS. This implies that, at the state university, the reform substantially affects active students who are closer to graduation and take longer to complete their degree than students with similar study progress but are subject to tuition fee payments. In contrast, at the private UAS, those cohorts closer to graduation barely changed their behavior compared to younger cohorts. By demographic groups, German nationals, female students, individuals younger than 25 at the moment of enrollment, and students with general UEQ exhibit a more considerable drop in academic performance at the state university after the abolition of tuition fees. At the private UAS, male students, those with alternative high school tracks, and German nationals show more substantial effects.

6. Conclusion

Five years after the German state of North Rhine-Westphalia decided to introduce moderate student tuition fees of EUR 500 per semester, these were suddenly abolished in March 2011, among high controversy. Unlike related studies of the effects of these reforms, our focus is not on aggregated enrollment or general educational attainment but on observable academic performance and activity. By using a panel of individual data, this paper assesses the effects on the number of credit points (CP) per semester, the number of exam registrations, and the probability of withdrawing from an exam in students from two representative universities in the state of NRW: a state university and a private university of applied sciences (UAS). Thanks to the institutional setting and the unique data, we can quantify the share of inactive or *ghost* students in these institutions, i.e., those who never showed any academic activity during their time at the university, and estimate causal effects only on active students who were susceptible of a behavioral adjustment. While the share of ghost students appears to be strikingly high, particularly at the state university, the institutional setting at German universities with strong financial incentives for enrollment and no monitoring of achievement explains this phenomenon.

In a descriptive analysis, we assess the changes in cohort composition after tuition fees were abolished at both universities. The observed increase in dropout rates at the state university (from 50% in 2010 to 60% in 2012) after the abolition of tuition fees does not necessarily mean, for instance, that the university started to attract students with lower ability or motivation. The share of active students at the state university drops substantially; and more moderately at the private UAS. At the same time, the share of inactive students in the state university grew significantly from 2012 onward, but much less for the refinanced programs – where tuition fees were abolished – at the private UAS. This supports the hypothesis that some students enrolled for reasons other than completing a degree. In this manner, the observed increase in dropout rates at the state university after the reform can be put into context. Many students who enrolled and left the institution

two or three semesters later did not necessarily fail but took advantage of the higher education system as a bridge to their professional or academic careers. However, in the refinanced programs at the private UAS, we observe that a larger share of ghost students does not fully explain the reduction of the share of active students.

Our study completes this descriptive analysis by quantifying the causal effect of the abolition of student fees on academic performance (measured in credit points per semester) and academic activity (measured in the number of exam registrations and the probability of exam withdrawal) among Bachelor students. In our empirical strategy, we consider that students from different cohorts in different semesters of study progress and different types of higher education institutions may react differently to the reform. We exploit the panel structure of our data and the fact that every cohort was affected by the reform in a different phase of their studies, and thus, we find valid control groups. Since the relevant unit time here is the study semester, we must consider treatment effect heterogeneity in a framework featuring a staggered treatment introduction. We address the empirical challenges of such a setting by utilizing the DID estimator suggested by Callaway & Sant'Anna (2021). In this manner, we exploit all the information in these administrative data by controlling for pre-determined observable characteristics of the students. Excluding changes in the composition of the cohorts, we find that causal effects are different between universities and vary by cohort and study semester, and by sociodemographic groups.

At the state university, the estimated effect on performance across all treated units is about -1 CP per semester (10% less with respect to the control units), and the effects remain negative and significant even in older cohorts. As expected, the effects are driven by active students, especially prospective graduates, who may complete up to 1.5 CP less on average. In contrast, dropouts who registered for at least one exam show a negative behavioral adjustment but are not statistically significant. Concerning the number of exam registrations per semester, students at the state university do not show any behavioral adjustment. Thus, keeping cohort composition and the number of exam registrations fixed, we estimate a decrease in academic performance measured in credit points among prospective graduates, most likely due to reduced effort. The effects on the probability of withdrawing from an exam align with these findings. Students at the state university are 2.5 p.p. less likely to withdraw from an exam. Therefore, these students kept their exam registration patterns and had fewer incentives to formally withdraw from an examination than their fee-paying counterparts. Clearly, only lower student effort could explain how their performance worsens even after registering for the same amount of exams.

At the private UAS, these estimates are also negative and significant: students reduce on average their academic performance by 2.6 CP per semester, which implies a relative drop of 38%. In this institution, prospective graduates exhibit the most significant drop, but future dropouts also react significantly. Students at the private UAS register for fewer exams after the abolition of the tuition fees, which can explain the drop

in credit points, as these two outcomes are correlated. Lastly, the probability of withdrawing from an exam increased on average by 9.2 p.p. among prospective dropouts. Thus, the abolition of tuition fees led to a behavioral adjustment among students at both institutions. Especially active students reacted by decreasing their academic performance. The reduction in academic achievement among prospective graduates necessarily leads to a more extensive time to completion at both institutions. At the state university, the drop in academic performance is measured in credit points per semester while keeping the number of exam registrations fixed, points at a lower student effort. Future dropouts, active students with low-performance levels, become inactive even faster at the private UAS before leaving the institution, as their withdrawal probability increases after the reform.

Indeed, a few mechanisms could explain such a behavioral adjustment (Bietenbeck et al., 2023). First, after the abolition of tuition fees, active students may have decreased their academic effort due to a sunk-cost effect, i.e., the psychological cost of failing would be much lower. Moreover, rational students who aimed to reduce their time to completion and pay fewer tuition fees would no longer have such an incentive. Lastly, study conditions and teaching quality could have worsened due to the universities' loss of this income source. However, the federal state of NRW compensated the universities for the lost income, and, according to the best of our knowledge, there is no empirical evidence of changes in the quality of education supply before and after the abolition of tuition fees. Indeed, here we have found some significant student-level evidence of a reduction in academic performance and, more limitedly, academic activity. Considering that we estimate causal effects on cohorts with a fixed composition and we exclude inactive students, our results point to a decrease in effort at the university.

For education policy, our findings imply that the focus should be set on increasing academic activity rather than only curtailing dropout rates. If there are enough (financial) incentives to enroll at a higher education institution without the intention to graduate, an important share of students will keep feeding the university census but not the list of graduates. Thus, these institutions and the education administration would be directing the public funds to an irresponsive set of individuals who make use of the system for strategic reasons and not to those students who want to graduate but fail. On top of that, shunning a policy instrument like moderate student fees, for which there is no conclusive evidence regarding its effects on enrollment, contributed to the drop in academic activity and performance of actual students. Some students barely hanging on decreased their activity and performance, leaving the institution without a degree. Many students who had the ability and motivation to complete their studies postponed graduation. In some cases, such significant changes in the education system, like the abolition of tuition fees, barely affect students who have either made their dropout decision or are about to complete their studies.

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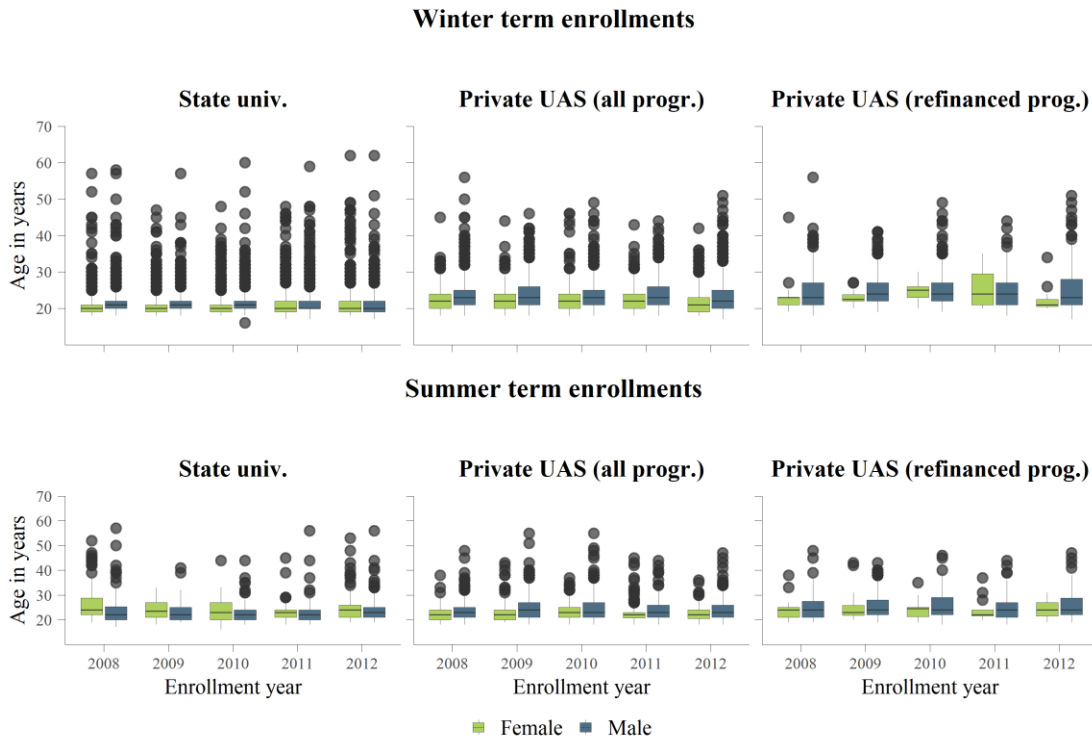
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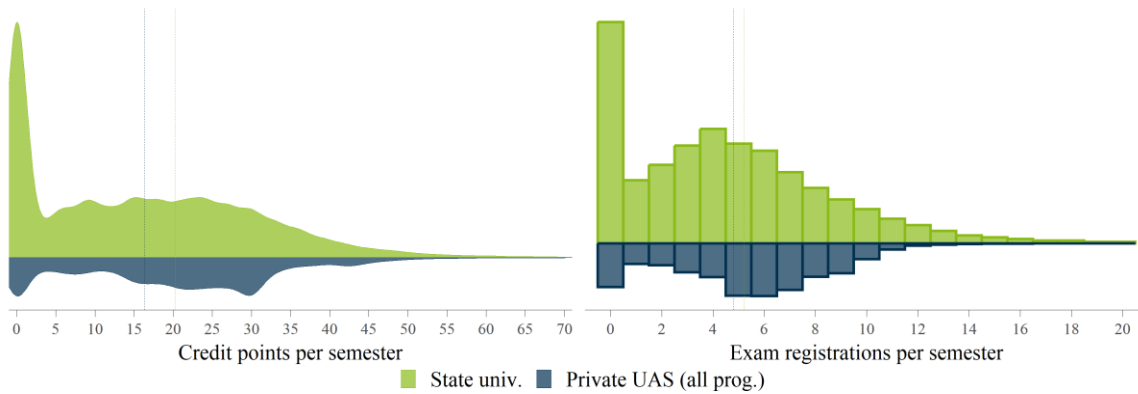
Appendix A. Figures

Figure A6: Distribution of the age at enrollment by sex



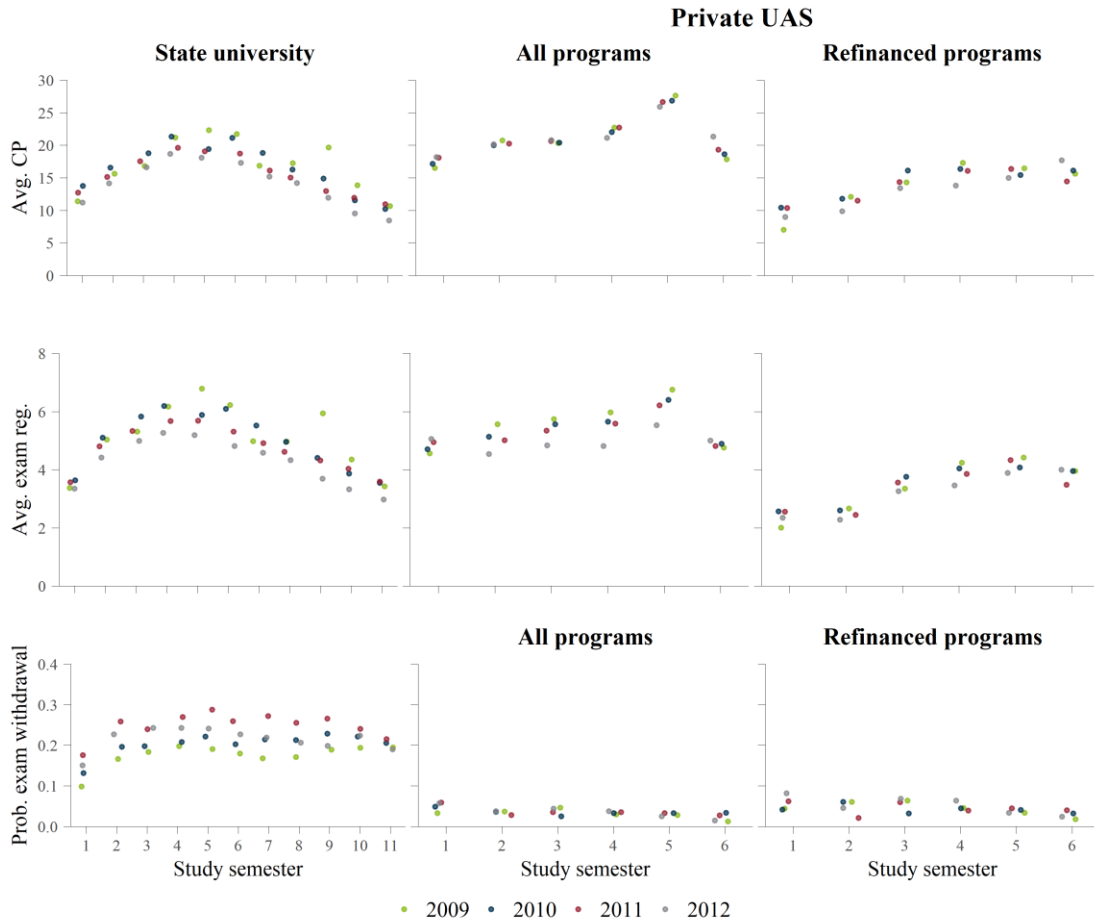
Notes: These figures illustrate the distribution of the age at enrollment share of former students enrolled between 2008 and 2012, in all Bachelor programs at the state university and the private UAS, as well as the subset of refinanced Bachelor programs at the private UAS.

Figure A7: Distribution of academic performance and academic activity



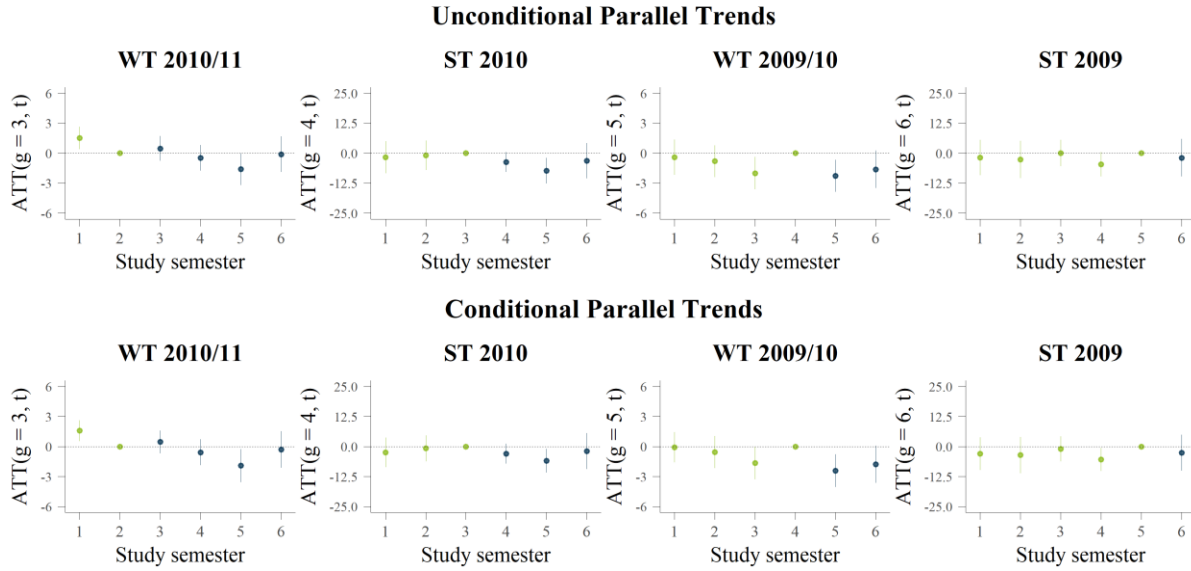
Notes: These figures illustrate the distribution of the number of credit points and the number of registered exams per semester of former students enrolled between 2008 and 2012, in all Bachelor programs at the state university and the private UAS.

Figure A8: Academic Activity and Performance by Cohort



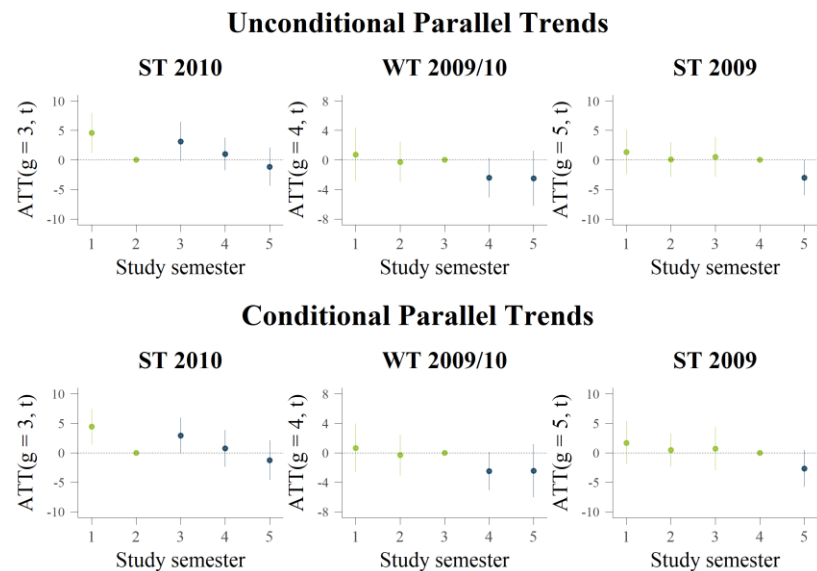
Notes: These figures illustrate the average number of credit points, the number of exam registrations, and the probability of exam withdrawal per semester of former students enrolled between 2009 and 2012, in all Bachelor programs at the state university and the private UAS, as well as the subset of refinanced Bachelor programs at the private UAS. To make the cohorts comparable, the horizontal axis indicates the relative study semester.

Figure A9: Cohort-Semester ATT - Credit Points per Semester (State Univ.)



Notes: These figures illustrate the estimated cohort-semester ATT or ATT(g,t) for each cohort in the estimation sample of the state university. The sample is restricted to former students enrolled between 2009 and 2011, who were between 18 and 55 years at the time of enrollment. Students who achieved more than 50 CP in a single semester are excluded. Point estimates and 95% confidence intervals are displayed.

Figure A10: Cohort-Semester ATT - Credit Points per Semester (Private UAS)



Notes: These figures illustrate the estimated cohort-semester ATT or ATT(g,t) for each cohort in the estimation sample of the refinanced programs at the private UAS. The sample is restricted to former students enrolled between 2009 and 2011, who were between 18 and 55 years at the time of enrollment. Students who achieved more than 50 CP in a single semester are excluded. Point estimates and 95% confidence intervals are displayed.

Figure A11: Dynamic Effects on Credit Points per Semester at the State University

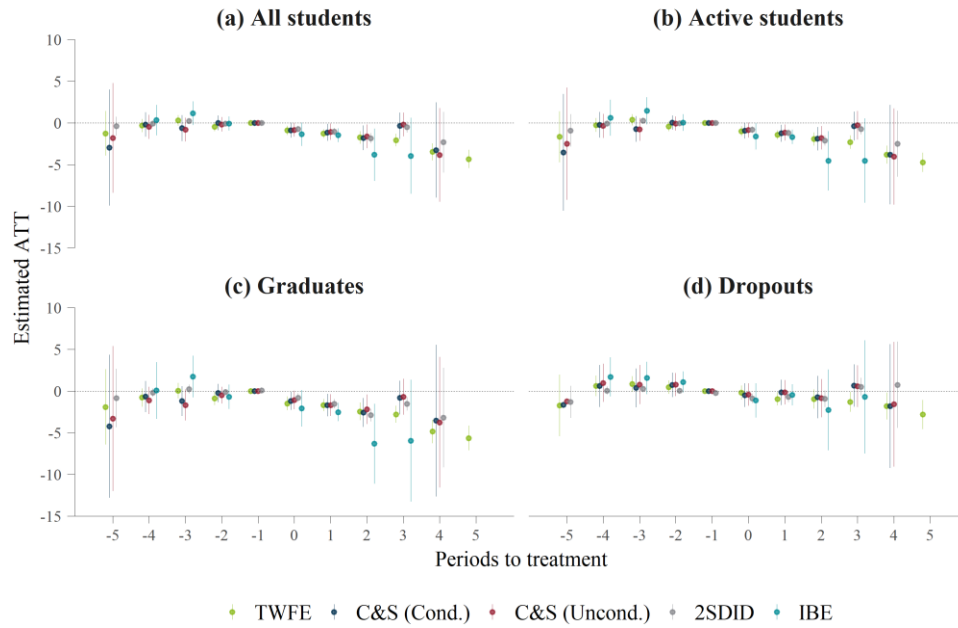


Figure A12: Dynamic Effects on Credit Points per Semester at the Private UAS

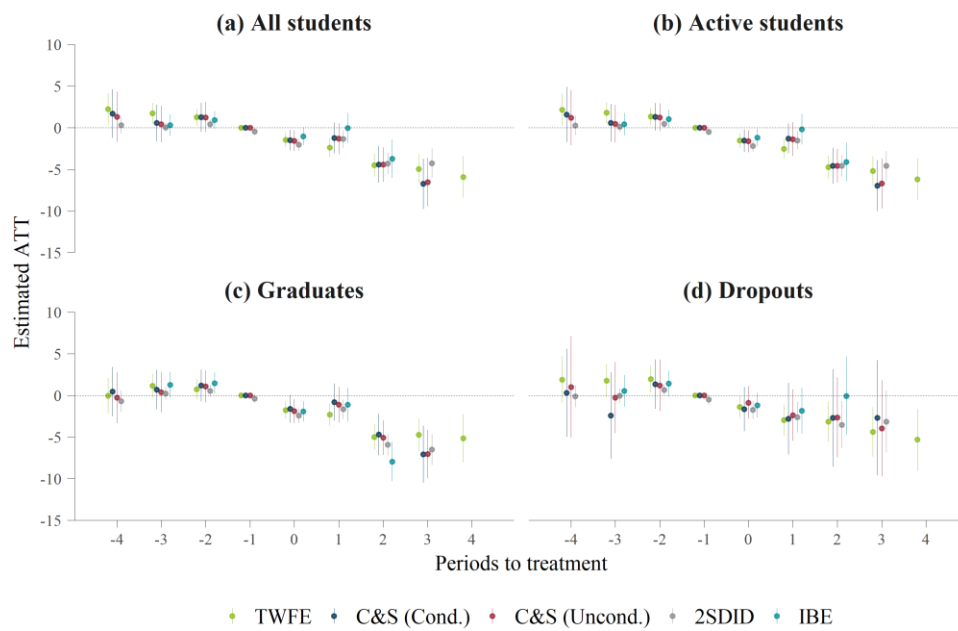


Figure A13: Dynamic Effects on Exam Registrations per Semester at the Private UAS

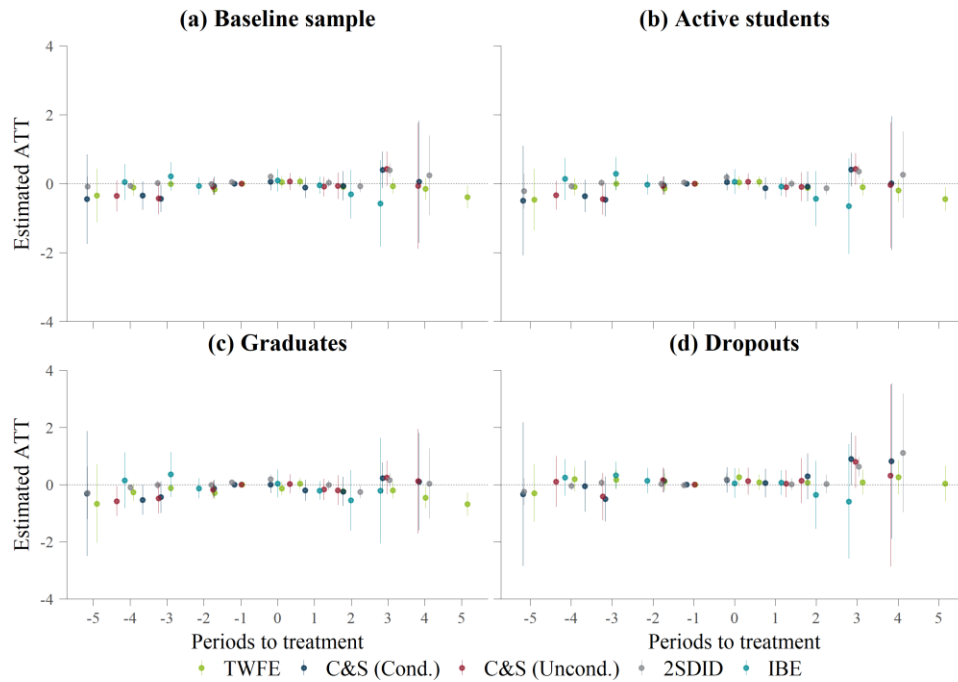
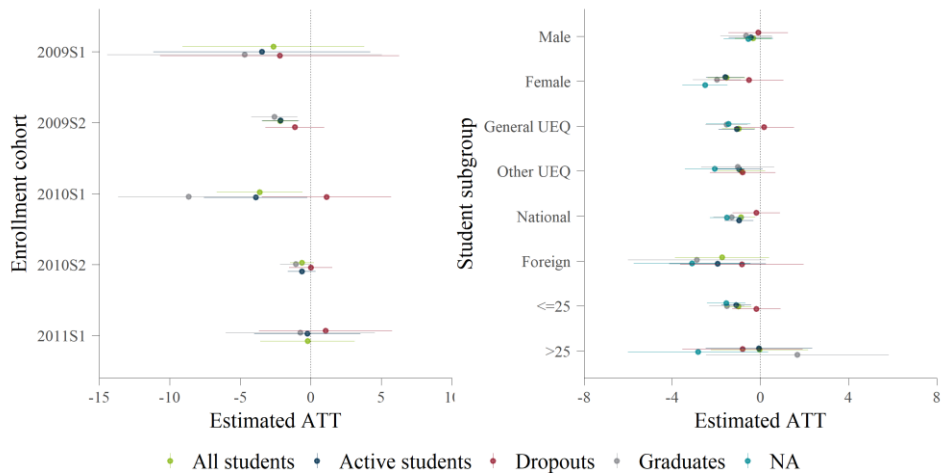
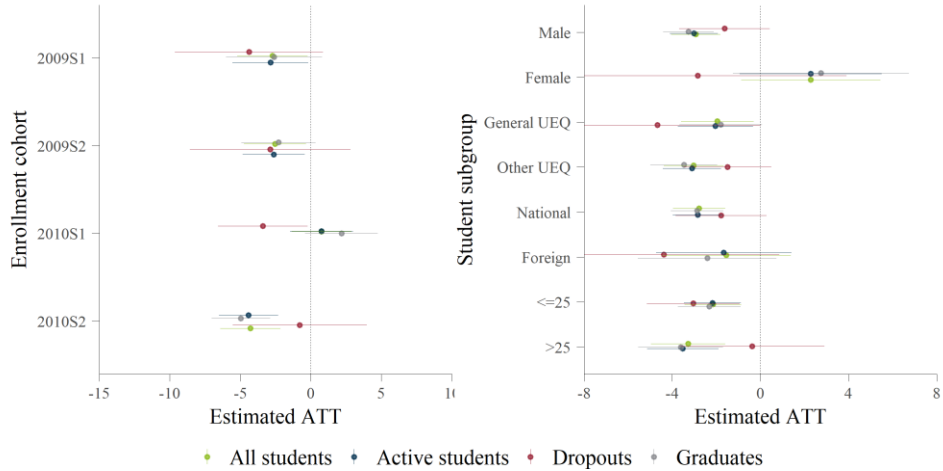


Figure A14: Subgroup Analysis of the Effects on Credit Points per Semester at the State University



Notes:

Figure A15: Subgroup Analysis of the Effects on Credit Points per Semester at the Private UAS



Notes:

Appendix B. Tables

Table B 3: Estimated Average Treatment Effects on Exam Registrations per Semester

Subsample	Callaway & Sant'Anna					N	Pre-means
	(1) TWFE	(2) Cond.	(3) Uncond.	(4) 2SDID	(5) IBE		
<i>State University</i>							
All students	0.22*** (0.06)	0.04 (0.10)	0.05 (0.09)	0.02 (0.08)	-0.05 (0.18)	5,742	3.1
Active students	0.21*** (0.07)	0.03 (0.09)	0.04 (0.09)	0.03 (0.09)	-0.11 (0.20)	4,614	3.8
Dropouts	0.14 (0.11)	0.28* (0.17)	0.21 (0.17)	-0.01 (0.09)	-0.05 (0.29)	2,209	3.5
Graduates	0.23*** (0.08)	-0.07 (0.12)	-0.04 (0.11)	0.01 (0.10)	-0.18 (0.27)	2,405	4.1
Unrestricted sample	0.41*** (0.08)	-0.15 (0.13)	-0.15 (0.12)	0.06 (0.09)	-0.11 (0.31)	7,074	3.4
<i>Private UAS</i>							
All students	-0.24** (0.10)	-0.54*** (0.12)	-0.53*** (0.13)	-0.54*** (0.08)	0.01 (0.14)	1,191	2.0
Active students	-0.26*** (0.10)	-0.56*** (0.13)	-0.55*** (0.13)	-0.53*** (0.09)	-0.03 (0.15)	1,064	2.2
Dropouts	-0.10 (0.19)	-0.21 (0.33)	-0.19 (0.27)	0.09 (0.15)	-0.01 (0.27)	421	2.2
Graduates	-0.36*** (0.10)	-0.58*** (0.13)	-0.62*** (0.13)	-0.74*** (0.19)	-0.32** (0.14)	643	2.2
Unrestricted sample	-0.33*** (0.11)	-0.73*** (0.17)	-0.74*** (0.16)	-0.77*** (0.14)	-0.22 (0.18)	1,206	2.1
Controls	No	Yes	No	No	No		
Student fixed effects	Yes	No	No	Yes	Yes		
Semester fixed effects	Yes	No	No	Yes	Yes		

This table summarizes the estimation results of a selection of DID models applied on different definitions of the sample of students enrolled between 2008 and 2011. The cohorts of always-treated students (WT 2011/2012 at the state univ. and ST 2011 at the private UAS) are excluded. Our baseline results are based on the method suggested by Callaway & Sant'Anna (2021). As

Subsample	Callaway & Sant'Anna					N	Pre-means
	(1) TWFE	(2) Cond.	(3) Uncond.	(4) 2SDID	(5) IBE		
a robustness check, we provide results from alternative methods: a simple TWFE model, the two-stage DID model (2SDID) suggested by Gardner & Jul (2021), and the imputation-based estimation (IBE) method suggested by Borusyak et al. (2021). *** p < 0.01, ** p < 0.05, * p < 0.1. Robust standard errors in parentheses.							

Table B 4: Estimated Average Treatment Effects on the Exam Withdrawal Probability

Subsample	Callaway & Sant'Anna					N	Pre-means
	(1) TWFE	(2) Cond.	(3) Uncond.	(4) 2SDID	(5) IBE		
<i>State University</i>							
All students	-0.018** (0.008)	-0.025** (0.010)	-0.027** (0.011)	-0.024*** (0.005)	-0.014 (0.023)	5,742	0.11
Active students	-0.020** (0.008)	-0.028** (0.012)	-0.029** (0.011)	-0.030*** (0.005)	-0.017 (0.027)	4,614	0.13
Dropouts	-0.023 (0.014)	-0.013 (0.022)	-0.016 (0.022)	-0.037*** (0.007)	-0.002 (0.046)	2,209	0.21
Graduates	-0.018* (0.010)	-0.035*** (0.012)	-0.036*** (0.012)	-0.033*** (0.007)	-0.027 (0.031)	2,405	0.07
Unrestricted sample	-0.012* (0.007)	-0.018* (0.009)	-0.018* (0.009)	-0.009 (0.006)	0.001 (0.019)	7,074	0.11
<i>Private UAS</i>							
All students	0.063*** (0.012)	0.092*** (0.015)	0.097*** (0.015)	0.085*** (0.014)	0.001 (0.014)	1,191	0.11
Active students	0.065*** (0.013)	0.094*** (0.014)	0.098*** (0.015)	0.100*** (0.016)	0.000 (0.015)	1,064	0.12
Dropouts	0.080** (0.037)	0.127** (0.055)	0.110** (0.054)	0.124*** (0.043)	0.010 (0.045)	421	0.19
Graduates	0.058*** (0.010)	0.088*** (0.013)	0.095*** (0.014)	0.098*** (0.022)	0.003 (0.002)	643	0.08
Unrestricted sample	0.061*** (0.012)	0.091*** (0.015)	0.096*** (0.014)	0.082*** (0.014)	-0.002 (0.015)	1,206	0.11
Controls	No	Yes	No	No	No		
Student fixed effects	Yes	No	No	Yes	Yes		
Semester fixed effects	Yes	No	No	Yes	Yes		

This table summarizes the estimation results of a selection of DID models applied on different definitions of the sample of students enrolled between 2008 and 2011. The cohorts of always-treated students (WT 2011/2012 at the state univ. and ST 2011 at the private UAS) are excluded. Our baseline results are based on the method suggested by Callaway & Sant'Anna (2021). As a robustness check, we provide results from alternative methods: a simple TWFE model, the two-stage DID model (2SDID) suggested by Gardner & Jul (2021), and the imputation-based estimation (IBE) method suggested by Borusyak et al. (2021).
*** p < 0.01, ** p < 0.05, * p < 0.1.
Robust standard errors in parentheses.

Table B 5: Estimated Average Treatment Effects on Credit Points per Semester

Subsample	Callaway & Sant'Anna					N	Pre-means
	(1) TWFE	(2) Cond.	(3) Uncond.	(4) 2SDID	(5) IBE		
<i>Private UAS</i>							
All students	0.27 (0.67)	0.22 (0.74)	0.86 (0.81)	0.64 (0.92)	2.37** (1.01)	1,191	6.8
Active students	0.26 (0.69)	0.09 (0.75)	0.79 (0.76)	0.81 (0.62)	2.42** (1.03)	1,064	7.6
Graduates	0.52	0.40	1.13	0.64	2.12**	421	5.5

Subsample	Callaway & Sant'Anna					N	Pre-means
	(1) TWFE	(2) Cond.	(3) Uncond.	(4) 2SDID	(5) IBE		
Unrestricted sample	(0.66) -0.99 (0.74)	(0.72) -0.05 (0.83)	(0.78) 0.48 (0.85)	(0.48) -0.09 (1.24)	(1.00) 1.97* (1.05)	643	9.0
Controls	No	Yes	No	No	No		
Student fixed effects	Yes	No	No	Yes	Yes		
Semester fixed effects	Yes	No	No	Yes	Yes		

This table summarizes the estimation results of a selection of DID models applied on different definitions of the sample of students enrolled between 2008 and 2011 at the private UAS. For this placebo test, we perform the estimation on the subset of students who were enrolled in programs which were not affected by the abolition of tuition fees. Our baseline results are based on the method suggested by Callaway & Sant'Anna (2021). As a robustness check, we provide results from alternative methods: a simple TWFE model, the two-stage DID model (2SDID) suggested by Gardner & Jul (2021), and the imputation-based estimation (IBE) method suggested by Borusyak et al. (2021).

*** p < 0.01, ** p < 0.05, * p < 0.1.

Robust standard errors in parentheses.

Table B 6: Estimated Average Treatment Effects on Credit Points per Semester

Subsample	Callaway & Sant'Anna				N	Pre-means
	(1) Stacked	(2) Cond.	(3) Uncond.	(4) IWM		
<i>State University</i>						
All students	-1.17* (0.48)	-1.05*** (0.32)	-0.95*** (0.30)	-0.25 (0.24)	5,742	10.6
Active students	-1.32** (0.50)	-1.13*** (0.32)	-1.04*** (0.33)	-0.29 (0.26)	4,614	13.2
Dropouts	-1.06*** (0.23)	-0.24 (0.53)	-0.25 (0.50)	-0.37 (0.37)	2,209	9.5
Graduates	-1.35 (0.79)	-1.53*** (0.43)	-1.41*** (0.40)	-0.17 (0.35)	2,405	16.6
Unrestricted sample	-0.72 (0.51)	-1.67*** (0.41)	-1.64*** (0.38)	-0.34 (0.31)	7,074	12.0
<i>Private UAS</i>						
All students	-1.37 (0.71)	-2.55*** (0.56)	-2.58*** (0.50)	-2.02*** (0.48)	1,191	6.8
Active students	-1.46 (0.74)	-2.65*** (0.56)	-2.66*** (0.53)	-2.15*** (0.48)	1,064	7.6
Dropouts	-0.63 (0.31)	-2.30* (1.20)	-1.97** (0.93)	-1.68** (0.75)	421	5.5
Graduates	-1.85 (1.05)	-2.56*** (0.58)	-2.84*** (0.56)	-2.61*** (0.53)	643	9.0
Unrestricted sample	-2.09* (0.81)	-3.41*** (0.66)	-3.52*** (0.70)	-2.71*** (0.63)	1,206	7.5
Controls						
Student fixed effects						
Semester fixed effects						

This table summarizes the estimation results of a selection of DID models applied on different definitions of the sample of students enrolled between 2008 and 2011. The cohorts of always-treated students (WT 2011/2012 at the state university and ST 2011 at the private UAS) are excluded. Our baseline results are based on the method suggested by Callaway & Sant'Anna (2021). As a robustness check, we provide results from alternative methods: a stacked regression estimator as in Baker et al. (2022), and the interaction-weighted model (IWM) by Sun & Abraham (2021).

*** p < 0.01, ** p < 0.05, * p < 0.1.

Robust standard errors in parentheses.

Table 7: Estimated Average Treatment Effects on Credit Points per Semester

Subsample	Callaway & Sant'Anna					N	Pre-means
	(1) TWFE	(2) Cond.	(3) Uncond.	(4) 2SDID	(5) IBE		
<i>State University</i>							
All students	0.12 (0.33)	-1.88*** (0.45)	-1.93*** (0.44)	-1.34*** (0.23)	-0.88 (0.96)	3,657	9.7
Active students	0.14 (0.35)	-2.01*** (0.51)	-1.99*** (0.49)	-1.81*** (0.35)	-0.81 (1.08)	2,843	12.5
Dropouts	-0.24 (0.50)	-0.68 (0.76)	-0.70 (0.73)	-0.97 (0.70)	1.25 (1.34)	1,404	9.4
Graduates	0.54 (0.47)	-2.53*** (0.62)	-2.53*** (0.63)	-1.89*** (0.50)	-1.28 (1.73)	1,439	15.5
Unrestricted sample	1.36*** (0.50)	-1.53** (0.66)	-1.71*** (0.64)	-0.97 (0.74)	-1.58 (1.52)	4,617	11.2
<i>Private UAS</i>							
All students	-2.03*** (0.44)	-3.62*** (0.63)	-3.72*** (0.62)	-3.00*** (0.38)	-2.09*** (0.73)	1,045	6.5
Active students	-2.16*** (0.45)	-3.75*** (0.59)	-3.82*** (0.59)	-3.19*** (0.46)	-2.34*** (0.75)	928	7.3
Dropouts	-1.58** (0.72)	-2.06 (1.59)	-1.91 (1.23)	-1.02*** (0.16)	-1.39 (1.11)	367	5.1
Graduates	-2.64*** (0.50)	-3.98*** (0.65)	-4.34*** (0.64)	-4.42*** (0.93)	-4.35*** (0.80)	561	8.8
Unrestricted sample	-2.46*** (0.53)	-4.69*** (0.83)	-4.89*** (0.78)	-4.17*** (0.87)	-3.29*** (0.96)	1,058	7.1
Controls	No	Yes	No	No	No		
Student fixed effects	Yes	No	No	Yes	Yes		
Semester fixed effects	Yes	No	No	Yes	Yes		

This table summarizes the estimation results of a selection of DID models applied on different definitions of the sample of students enrolled between 2008 and 2011. The cohorts of always-treated students (WT 2011/2012 at the state university and ST 2011 at the private UAS) are excluded. We further exclude cohorts for which the parallel-trends assumption may be violated: the ST 2009 and the WT 2010/2011 cohorts at the state university and the ST 2010 cohort at the private UAS. Our baseline results are based on the method suggested by Callaway & Sant'Anna (2021). As a robustness check, we provide results from alternative methods: a simple TWFE model, the two-stage DID model (2SDID) suggested by Gardner & Jul (2021), and the imputation-based estimation (IBE) method suggested by Borusyak et al. (2021).

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Robust standard errors in parentheses.