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Tuition fees and educational attainment

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Abstract
Following a landmark court ruling in 2005, more than half of Germany's universities started charging tuition fees, which were subsequently abolished until 2015. We exploit the unusual lack of grandfathering in these policies to show that fees increase study effort and degree completion among incumbent students. However, fees also decrease first-time university enrollment among high school graduates. Combining this enrollment impact with the effect on completion, we find that fees around the zero-price margin have only little effect on overall educational attainment. We conclude by discussing policies targeting the separate effect margins of fees and caution against a general abolition.

Keywords: higher education, fees
JEL: I23; I28

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1 Introduction

Do university tuition fees affect educational attainment? This question is important because many governments are currently debating whether or not to charge tuition fees for public higher education[1] and because educational attainment is closely linked to economic growth (e.g. Barro, 2001; Hanushek and Woessmann, 2008). In general, fees can affect attainment at two distinct margins. First, they can impact university enrollment among high school graduates, that is, the extensive margin. Second, they can affect degree completion among enrolled university students, that is, the intensive margin. The impacts at these two margins can notably go in opposite directions: for example, tuition fees might reduce enrollment due to deterrence effects, but increase degree completion because of improvements in educational quality or because students raise their study effort due to a sunk-cost effect. In this case, the overall impact on educational attainment would be ambiguous.

In this paper, we study how tuition fees affect educational attainment at both the extensive and the intensive margin. To that end, we examine policy changes that led to the introduction of fees at more than half of Germany's public universities in the mid-2000s, as well as their staggered abolition until 2015. We exploit variation in fees across universities and years generated by these reforms using appropriate difference-in-differences (DiD) designs and register data on the universe of students enrolled in higher education. We find that fees decrease first-time university enrollment among high school graduates but increase degree completion among enrolled university students. Combining these estimates in a simple accounting framework, we show that these extensive and intensive margin effects roughly offset each other. As a result, tuition fees do not change educational attainment in the population much.

The main part of our analysis focuses on the estimation of intensive margin impacts on degree completion. Identifying such impacts has been difficult due to a formidable empirical challenge:

[1] For example, governments in several German states first introduced university tuition fees in the mid-2000s and then abolished them again a few years later. Whereas Ireland abolished fees in 1996, the United Kingdom started charging them in 1998. In the United States, some states have recently abolished fees for public colleges and universities, and a number of high-profile politicians have advocated making public higher education free of charge nationwide.

because changes in tuition fees usually affect the composition of students at the extensive margin, any impacts on post-enrollment outcomes will reflect both intensive margin effects and this change in composition. We are able to overcome this challenge and estimate pure intensive margin effects due to a unique feature of the German context. There, tuition fees for public higher education were first legalized in a ruling of the Federal Constitutional Court in 2005. Following this law change, universities in seven out of 16 states started charging tuition fees. In an unusual lack of grandfathering, these fees applied not only to students who enrolled after this change, but also to those who had already begun their studies. By focusing on these latter, incumbent students, we are able to hold student composition fixed and estimate the pure intensive margin impact of fees on degree completion. We also exploit the later, staggered, abolition of fees to support our analysis. The abolition again applied directly and without grandfathering. We regard the introduction as the preferred natural experiment, mostly because the Constitutional Court ruling caught students by surprise, which precludes anticipation effects, and because of additional empirical challenges that arise when studying the abolition effects, which we describe in more detail below.

Our regressions use register data covering the universe of students enrolled in higher education in Germany. This has the advantage that we can follow students even if they change university, which enables us to capture potential general equilibrium responses to the introduction of fees. In our main analysis of the intensive margin effects of the introduction of fees, the primary outcome variable measures degree completion at any German university within six years of initial enrollment.[2] Similarly, our treatment variable captures the introduction of fees at the university of initial enrollment within the first six years. To ensure that we estimate pure intensive margin effects, we restrict our sample to students who initially enrolled at university between 1995 and 2004, before tuition fees were legalized. We estimate difference-in-differences specifications that exploit variation in the introduction of fees for incumbent students in 2007 across universities. Note that because tuition was charged as 500 Euro per semester rather than per credit or per degree, different

[2] As we describe in Section 2, the majority of programs during our study period had a nominal duration of four-and-a-half to five years, although in practice most students took longer to graduate. This motivates our focus on completion within six years.

cohorts of students faced different total amounts of fees. For example, students who enrolled in 2001 and completed their degree within six years paid at most 1,000 EUR (that is, two semesters in 2007), whereas students who enrolled in 2004 paid up to 4,000 EUR (from 2007 to 2010). In our regressions, we exploit this variation in treatment intensity across cohorts to examine how the impact of fees depends on the amount paid.

The results reveal that tuition fees substantially increase degree completion at the intensive margin, with the size of the effect varying with the amount of fees: for example, completion rises by 2.8 percentage points (pp) for students who have to pay up to 1,000 EUR, and by 5.9 pp for students who have to pay up to 4,000 EUR. Strikingly, this effect appears to persist over time, with the completion rate remaining higher even eleven years after students first enrolled at university. This finding strongly suggests that fees do not merely lead to faster completion, but to an actual higher incidence of completion.

In further analyses, we examine potential mechanisms behind this effect. We find little empirical support for the hypothesis that our results are due to an increase in per-student resources and a resulting improvement in educational quality. While we cannot rule these factors out, we do find compelling evidence for a different mechanism: students increase their study effort in response to fees, with treated students spending eleven percent more hours per week on their studies. An increase in study effort and faster completion are rational responses of forward-looking students to the introduction of fees. In contrast, the higher incidence of completion is more difficult to explain in the absence of improvements in educational quality. We briefly discuss alternative explanations such as sunk-cost effects, which could also account for our findings.

As the final step in our study of intensive margin effects, we consider the staggered abolition of fees until 2015. Using a recently developed version of the difference-in-differences estimator appropriate for this staggered setup (De Chaisemartin and d'Haultfoeuille, 2020), we show that the abolition of fees has symmetric effects on on-time completion for the then prevalent Bachelor degrees: Specifically, the abolition of fees reduced degree completion among incumbent students, who had first enrolled when fees were charged, by 2.3 pp. This finding highlights that intensive margin effects around the zero-price margin hold independently of whether students first enrolled when fees were charged or not. It also lends additional credibility to the assumption in our main analysis that there are no other treatment-group-specific policy changes that drive the completion effects, as it is highly unlikely that such changes would occur in lockstep with both the introduction of fees and their staggered abolition.

We then turn our attention to the analysis of extensive margin effects. To that end, we merge the student register data with information on the number of high school graduates by state and year. We define treatment as an indicator for tuition fees being charged by universities in the state and year of high school graduation, and we construct our dependent variable as the fraction of high school graduates enrolling at any university in Germany within one year. To identify the causal effect of fees on enrollment, we estimate difference-in-differences regressions that exploit the variation across states and years due to the introduction of fees. Our headline estimate shows that fees decrease enrollment by 3.9 pp.

Finally, we combine our estimates of extensive and intensive margin effects to gauge the overall impact of tuition fees on educational attainment. Because fees reduce enrollment but increase degree completion, the direction of this impact is ambiguous. In a simple accounting framework, we show that because these opposing forces roughly offset each other, fees do not change the number of university graduates in the population much. At the same time, the public cost of higher education decreases substantially with the introduction of fees.[3] We conclude by discussing targeted policy options for the opposing effects at the extensive and intensive margins.

This paper relates to a large and growing empirical literature on how costs of higher education affect student outcomes. This research mostly considers settings in which colleges and universities charge tuition fees, but in which there is exogenous variation in financial aid for specific groups of students. One important strand of this literature estimates extensive margin effects on enrollment.

[3] Because fees were abolished again only a few years after they were introduced, most students who enrolled under the fee regime experienced the (announcement of) the abolition, meaning that we are unable to estimate the combined extensive and intensive margin impact on attainment directly. A major advantage of studying extensive and intensive margin impacts separately is that it allows us to gain a better understanding of the different components of the (lack of an) overall effect of tuition fees on educational attainment.

For example, Dynarski (2003) studies the Social Security Student Benefit Program, which until 1982 subsidized college for children of deceased, disabled, or retired Social Security beneficiaries in the United States. She finds that a 1,000 USD increase in grant aid raises enrollment by 3.6 pp. Other papers similarly exploit variation in the eligibility for need-based or merit-based aid and show that reductions in the net cost of college raise enrollment (e.g. Kane, 2003; Cornwell, Mustard, and Sridhar, 2006; Barr, 2015; Castleman and Long, 2016). In a survey of this literature, Deming and Dynarski (2010) conclude that eligibility for 1,000 USD of financial aid increases enrollment by around 4 pp on average.[4]

A related emerging strand of research focuses instead on identifying pure intensive margin effects. Using data from private Bocconi University in Milan, Garibaldi et al. (2012) show that final year students who face higher fees in case of continued enrollment are more likely to graduate on time. Conversely, Denning (2019) finds that non-traditional older students in Texas who become eligible for additional financial aid (and thus face lower costs) are also more likely to graduate on time. Similar positive intensive margin impacts of aid on degree completion are documented by Barr (2019) for veteran students in the United States and by Murphy and Wyness (2016) for students at nine English universities. In contrast, Fricke (2018) finds no significant effect of increased fees on on-time completion at the University of St. Gallen in Switzerland.

Finally, a number of studies estimate the effects of aid and tuition fees on degree completion. Because such changes in the net cost of higher education usually affect enrollment, most of these estimates reflect a combination of extensive and intensive margin effects, which are difficult to disentangle. For example, Dynarski (2003) shows that a 1,000 USD increase in aid leads to an additional 0.16 years of schooling, an impact that blends the rise in enrollment mentioned above with any intensive margin responses affecting college persistence. Castleman and Long (2016)

[4] A few studies similarly show that "sticker price" tuition fees decrease enrollment in the United States (Kane, 1995; Denning, 2017). Murphy, Scott-Clayton, and Wyness (2019) study the introduction of fees and a change in the student loan system in England and find that university enrollment there has risen despite fees. Finally, two studies use aggregate, state-level data to examine the introduction of tuition fees in Germany: whereas Hübner (2012) finds that enrollment decreased by 2.7 pp, Bruckmeier and Wigger (2014) identify a smaller, statistically insignificant effect of 0.9 pp. Unlike these last two papers, our extensive margin analysis uses individual-level data on university students and also differs in some other aspects, which we describe in detail in Section 5.

similarly document positive effects of a grant on both enrollment and completion in Florida. In another study, Denning, Marx, and Turner (2019) show that grants increase completion for low-income four-year public college students in Texas. In contrast, Fack and Grenet (2015) find that a need-based grant program in France raises enrollment but not on-time graduation, and Angrist et al. (2016) document that a scholarship program in Nebraska increased enrollment but lowered on-time completion. Deming and Walters (2018) combine intensive and extensive margins effects to draw inference on the overall impact of fees on educational attainment. The authors use budget cuts in the US states as Bartik-type instrument for changes in tuition and university spending. Their findings suggest that changes to tuition fees have no impacts on enrollment and completion rates once university spending is accounted for.

Our paper complements and contributes to the existing literature in three main ways. First, we study price changes at the no-fee-to-fee margin, rather than impacts of financial aid or other changes to existing fees. The effect of introducing fees might differ from that of modifying financial aid due to non-linearities, because of general equilibrium responses, and because the zero price might be special (Shampanier, Mazar, and Ariely, 2007). The introduction of fees is also much more salient than variation in financial aid, for which information costs often play an important role (e.g. Bettinger et al., 2012; Barr and Turner, 2018; Dynarski et al., 2018). Second, we estimate effects for the population of students in an entire country, rather than for particular subgroups of students. This allows us to gauge how introducing or abolishing fees affects educational attainment under weaker assumptions for external validity. Third, unlike previous research, we are able to estimate causal impacts at both the extensive and the intensive margin. This is important because these effects have independent mechanisms with different policy implications, which we discuss in more detail in the conclusion. Moreover, our finding that the effects at both margins go in opposite directions implies that looking at only one of these margins, as some of the previous literature has done, might lead to erroneous conclusions about the overall impact of tuition fees on educational attainment.

2 Institutional setting

2.1 University education in Germany

University education in Germany is organized by the 16 states, which largely finance the public institutions located on their territory. Private higher education plays only a minor role, with 1.1 percent of students attending a private university in 2007 (Federal Statistical Office, 2008). Universities offer a wide range of academically oriented programs, which mainly admit graduates of academic-track high schools from all states.[5]

During our main study period, universities mainly awarded three kinds of degrees: studies in humanities would usually lead to a *Magister*; studies in law, medicine, and teaching would lead to a *Staatsexamen*; and studies in most other subjects would lead to a *Diplom*. The majority of programs admitted students at the beginning of the winter term in October and had a nominal duration of four-and-a-half to five years, split into nine to ten semesters, although in practice most students took longer to graduate (see Section 3.2). Within each program, students first had to complete the required coursework in order to be admitted to a final exam, which usually involved a series of written and oral tests and the defense of a thesis. Upon passing the final exam, they would then be awarded the degree. In the late 2000s and early 2010s, as part of the harmonization of European higher education systems, universities gradually shifted from this system of single-tier degrees to a system of two-tier Bachelor and Master degrees. This shift took place mainly after the general tuition fees were introduced.

[5] In a few exceptional cases, graduates from non-academic-track high schools are allowed to enroll at university. Moreover, graduates from such high schools can acquire the qualifications required for university admission later on via educational upgrading. Besides universities, there are technical colleges (*Fachhochschulen*), which mainly offer programs that teach professional skills in fields such as business and engineering. These programs also admit individuals with completed vocational education and are typically shorter than the ones offered by universities. In this paper, we focus on students at universities only. See Dustmann, Puhani, and Schönberg (2017) for further information on the German high school system and its different tracks.

2.2 Student finances

The extent to which tuition fees influence enrollment at university and degree completion likely depends on students' financial situation. In what follows, we therefore present some stylized facts about student finances. A first important fact is that the majority of students move out of their parents' home when starting university, with only 22.8 percent of students still living with their parents in 2006 (Isserstedt et al., 2007). Thus, most students have to pay rent and cover their own living expenses while at university. Moreover, all students have to pay a mandatory administrative charge, which is levied directly by the university and which averaged 145 EUR per semester in the mid-2000s (Bruckmeier, Fischer, and Wigger, 2013). This charge contributes to the financing of the university's administration and the student affairs office, and paying it often entitles students to a ticket for free local public transportation. Taken together, students' total expenses in 2006 averaged 739 EUR per month, or about 9,000 EUR per year, according to a large student survey (Isserstedt et al., 2007).

There are three main ways in which students finance their expenses.[6] First, most students receive a monthly allowance from their parents (52 percent of students' average income). Second, many of them work part-time jobs during the semester or the summer break (24 percent). Third, a means-tested federal aid program, which does not differ between universities, helps students from poorer families pay for their studies (14 percent). Finally, ten percent of students' average income derives from other sources, including private savings and contributions from relatives. Note that unlike in many other countries, private student loans play a negligible role in Germany, with fewer than three percent of students taking out a loan in 2006.

2.3 Tuition fees

Until 2005, general tuition fees at public universities in Germany were prohibited by federal law. This law was challenged by some state governments in the early 2000s and was eventually

[6] All figures on expenses mentioned in this paragraph are based on a representative student survey conducted in 2006 and described in Isserstedt et al. (2007).

struck down by the Federal Constitutional Court in 2005 on the grounds that it violated states' constitutional right to formulate their own education policies. Following this decision, seven out of 16 state governments introduced general tuition fees for higher education.[7] In five of these states, laws directed universities to charge 500 EUR per semester from their students. In the other two states, universities were allowed to set their own level of fees up to a maximum of 500 EUR, and in practice most institutions levied this maximum amount (Hübner, 2012).

General tuition fees were first charged from newly-enrolled students from 2006 or 2007 onward, depending on the state. Moreover, in all seven reform states, all other already-enrolled students also had to pay tuition from 2007 onward. In Section 4, we exploit this unusual lack of grandfathering to estimate pure intensive margin effects of fees. Finally, we note that in all states some groups of students were exempted from paying fees, including students with disabilities and students with children. Moreover, the introduction of fees was accompanied by the establishment of credit schemes, which allowed students to take out loans for the amount of tuition they had to pay under favorable conditions (see Bahrs and Siedler, 2019). Appendix Table A.1 provides a detailed overview of the regulations concerning tuition fees by state and year.

While federal law prohibited *general* tuition fees before 2005, it did allow *targeted* tuition fees for so-called "long-term students" who are enrolled well beyond the nominal duration of their degree program. Using this exception, 12 states had started charging such long-term fees already before the introduction of general tuition fees. The long-term fees usually amounted to 500 EUR per semester and were first charged two years after a student had reached the nominal study duration, that is, about six-and-a-half to seven years after initial enrollment.[8] After the 2005 ruling, long-term fees were abolished and replaced by general tuition fees in the seven reform states, all of which had previously levied long-term fees. Given that the amount to be paid was usually the same, this change made little difference for affected long-term students. Below, we use

[7] These states were Baden-Württemberg, Bavaria, Hamburg, Hesse, Lower Saxony, North Rhine-Westphalia, and Saarland. At the time of introduction, all of these states had center-right governments, which traditionally have been more in favor of charging tuition fees for higher education.

[8] See Appendix Table A.1 for an overview of regulations concerning long-term fees by state and year.

this insight to conduct a placebo check that supports the validity of our empirical strategy. We also show that regulations concerning long-term fees are not driving our main results, which exploit the introduction of general tuition fees.

From the point of view of universities, the general tuition fees introduced after 2005 represented a new source of income. However, state laws strictly regulated how this money could be spent. Specifically, universities were required to spend the fees to improve study conditions, and could usually not use them to increase their research budgets or to hire new professors. In practice, this meant that most institutions used the tuition money to upgrade the technical equipment in classrooms, buy new books for and extend the opening hours of university libraries, and hire short-term staff for the teaching of additional tutorial sessions (Deutscher Bundestag, 2009). These investments could potentially have affected completion rates, an issue that we return to when discussing the potential mechanisms behind our results in Section 4.5.

Among both policymakers and the public, the introduction of general tuition fees was highly controversial. Students, backed by labor unions, organized large nationwide protests, and some politicians from left-leaning parties challenged the new policy in state courts. The mounting public pressure eventually led to the abolition of fees in all states. Thus, Hesse abandoned fees in 2008, only one year after they were first charged, and the six other states followed suit between 2010 and 2014 (see Appendix Table A.1). As a consequence, general tuition fees are no longer charged at any public university in Germany nowadays.[9] In Section 4.6, we use this staggered abolition to validate our estimates based on the introduction of fees.

An important additional question matters for inference: were tuition fees effectively state- or university-level policies? The answer is: both. While policy changes were implemented at the state-level, universities still had some authority to decide over the spending of the additional income generated by fees. Moreover, in North Rhine-Westphalia and Bavaria universities could set the amount of the fees, even though the vast majority of universities decided to charge the

[9] While the public remains strongly divided over the issue, increasing financial pressure on universities has recently led policymakers to consider the re-introduction of fees. For a detailed discussion of the political economy of higher education financing in Germany, see Lergetporer and Woessmann (2021, 2022).

maximum (500 EUR) allowed by the state law.[10] In our main analysis, we cluster standard errors at the university level, reflecting the fact that students typically choose universities and degrees to study rather than states to move to. However, we also show that our results are robust to using other clustering choices, including clustering at the state level.

3 Data for the analysis of intensive margin effects

In this Section, we describe the data used in our main analysis of intensive margin effects of the introduction of fees. In our later analysis of the abolition of fees, we use data from the same sources but focus on a different sample, which we describe in Section 4.6 below.

3.1 Main data sources and variables

The main empirical analysis uses data from the German Student Register (RDC 2017b) and the Final Examinations Register (RDC 2017a) for the years 1995 to 2010. The Student Register covers the population of students enrolled at universities and technical colleges in Germany in a given year. It draws its data from these institutions' administrative records and includes individual-level information on current enrollment (subject, degree type), institution and year of initial enrollment, and demographic characteristics (gender, nationality, county of the high school). The Final Examinations Register covers the population of students who took a final exam at an institution of higher education in Germany in a given year. It contains similar variables to the Student Register and also provides information on the final grade achieved (four levels ranging from "very good" to "sufficient," conditional on passing the final exam).

Because of strict data protection laws, neither of the two registers contains individual student identifiers. Thus, students cannot be linked within the same register over time, or across the two different registers. However, since we observe the institution and year of initial enrollment in both registers, we can construct a panel at the starting-university-by-cohort level: for each cohort at each university, we know how many students initially enroll. Moreover, we know how many of them

[10] See DSW (2008) and Gensch and Raßer (2011) on how tuition fees were charged and spent at universities in North Rhine-Westphalia and Bavaria, respectively. For a federal overview, see Deutscher Bundestag (2009).

graduate in each subsequent year. This allows us to measure degree completion, our main outcome for the intensive margin analysis, as the share of students in a particular starting-university-by-cohort cell who graduate within a given time frame. While the regressions below are based on such aggregated data, in practice we recover the coefficients from the equivalent individual-level regressions by weighting observations by cell size.

Our main dependent variable captures cumulative degree completion within d years after initial enrollment and is constructed as follows:

$$C_{cu_s} = \frac{\sum_{\tau=1}^{d} \sum_{i \in c, u_s} Completion_i^{t=\tau}}{\sum_{i \in c, u_s} Enrolled_i^{t=1}}. \tag{1}$$

Here, i denotes students, $c \in [1995, 2004]$ denotes cohorts, u_s indicates the university of initial enrollment ("starting university"), $t = 1, 2, ...$ denotes years since initial enrollment and d denotes the (maximum) number of years considered. In our main analysis we focus on completion in six years ($d = 6$). $Enrolled_i^{t=1}$ is a dummy that measures first-year, freshman enrollment of student i as recorded in the Student Register. Thus, the denominator on the right-hand side of Equation 1 is equal to the size of cohort c at starting university u_s. $Completion_i^{t=\tau}$ is a dummy that measures successful degree completion by student i in year τ as recorded in the Final Examinations Register. Importantly, our population-level data allow us to observe this outcome even if student i changes to another university or technical college during the course of her studies. The numerator in Equation 1 thus equals the total number of degrees obtained by students in cohort c of starting university u_s within d years of their initial enrollment, and consequently C_{cu_s} is the share of students in this university-cohort cell who complete their degree within this time frame. Note that because we restrict our sample to students who first enrolled at university before the Constitutional Court ruling in 2005, the last cohort for which we can construct six-year completion rates is the cohort of 2004.

The focus on completion within six years is motivated by two facts. First, while majority of degrees offered in the early 2000s had a nominal duration of five years or less, in practice most students took longer to finish their studies and graduated in their sixth or seventh year, see Section 3.2. Second, most universities already charged long-term tuition fees of about 500 EUR per semester from students who were enrolled beyond their sixth year. For these students, there was thus little change when the general tuition fees were introduced in 2007. In additional analyses, we also estimate the impacts of tuition fees on degree completion within different time frames.

We define treatment as a dummy Fee_{cu_s} that takes value 1 if the university of initial enrollment u_s introduced tuition fees within six years after cohort c first enrolled and 0 otherwise. Note that because tuition fees were levied per semester, rather than per degree, the intensity of this treatment differs between cohorts: for example, whereas students who initially enrolled at a fee-charging university in 2001 had to pay at most 1,000 EUR when completing their degree within six years (with the exact sum depending on how many years they actually took to complete), the corresponding amount for students who initially enrolled in 2004 was 4,000 EUR. Our estimation strategy, which is described in detail below, exploits this cross-cohort variation in treatment intensity in addition to the cross-university variation in paying any fees versus none.

3.2 Sample selection and summary statistics

We mainly focus on first-time students who initially enrolled in a single-tier degree program in the winter term at any public university in Germany. We restrict our data on this population in several ways, either to ensure that we best exploit the policy variation for the estimation of intensive margin effects, or to resolve data issues. First, and most importantly, we concentrate on cohorts that started university in or before the year 2004. This ensures that all students in our sample had made their enrollment decision before the first laws introducing general tuition fees were passed, which allows us to estimate pure intensive margin impacts. Second, we focus on students who obtained their high school diploma in Germany and who have not previously completed another university degree, as this is the population for which we can estimate extensive margin effects on enrollment below. Third, our main analysis disregards students who first enrolled at a university in the state of

Hesse. The reason is that Hesse abolished tuition fees shortly after they were charged for the first time, which renders the treatment fundamentally different from that at fee-levying universities in the other six states.

Fourth, the construction of our main outcome variable requires information on the university and year of initial enrollment, and this information is missing for about 15 percent of observations in the Final Examinations Register. These missing cases are unevenly spread across universities and years, and are most likely due to data loss generated by a failed software update in these universities' administrative computer systems.[11] Thus, our main analysis excludes all universities with extreme shares of missing information. Specifically, we exclude institutions with more than 20 percent of missing cases in any year, or with more than ten percent of missing cases across all years. Note that this restriction only applies to *initial* enrollment; affected students can move and complete their degree at *any* university in Germany and remain included in our sample.[12]

The final estimation sample includes 731,352 students at 39 starting universities in nine states who first enrolled between 1995 and 2004.[13] Appendix Table A.2 lists the included universities, and Table 1 reports summary statistics for this sample. Slightly more than half of the students are female. 57 percent enrolled at universities that introduced fees, and for 25 percent this change happened within the first six years of their studies. Only 28 percent complete their degree within six years, a figure that reflects slow completion and low graduation rates at German universities in general. Substantiating this claim, Figure 1 shows that the cumulative completion rate is below 60 percent even ten years after initial enrollment. While we do not observe dropout in our data, it is likely that the majority of the remaining students never finish their degree: for example, among students who initially enrolled in 1996, the cumulative completion rate is 57.3% after ten years and 61.3% after 15 years, suggesting that only very few students complete their degree after more than ten years. Figure 1 also shows the timing of completion in our sample and reveals that students

[11] Personal conversation with Thomas Feuerstein from the Federal Statistical Office.

[12] In our Online Appendix, we discusses this issue in detail and includes a battery of robustness checks that confirm that our results are not driven by this or other data problems.

[13] The less restricted sample analyzed in the Online Appendix includes 1,272,465 students from 78 universities, i.e. all but seven public universities in Germany.

typically graduate between five and seven years after initial enrollment. This implies that they take between one and two years longer than the nominal study duration to finish their degree.

3.3 Additional data sources

In additional analyses, we use data from the German Student Survey and the German Microcensus to shed light on the potential mechanisms underlying the intensive-margin effects. The Student Survey samples representative cross sections of students and collects information on their time use, among other things (for a more detailed description of this study, see Multrus, Ramm, and Bargel (2010)). Our estimations use individual-level data for the three waves of the survey conducted between 2004 and 2010 (Georg, Bargel, and Bundesministerium fuer Bildung und Forschung, 2016a,b; Georg, Ramm, and Bundesministerium fuer Bildung und Forschung, 2016). We complement these data with information on student employment outcomes from the Microcensus, an annual one-percent census of households in Germany. Our analysis draws on scientific use files, which contain 70 percent subsamples of the actual Microcensus, for the years 2001-2010 (RDC, 2016). Furthermore, we obtained information on state-level GDP from the regional database of the Federal Statistical Office, and on state-level unemployment from the Federal Employment Agency, to use as controls in some of our regressions.

4 Effects at the intensive margin

4.1 Descriptive analysis

Before turning to regression results, we examine completion patterns in our sample descriptively. Figure 2 plots the share of students who complete a degree within six years by cohort, separately for universities that introduced fees ("fee universities") and those that did not ("non-fee universities"). The vertical dashed line in this graph marks the passing of the first laws establishing general tuition fees, and the vertical solid line marks the actual introduction of fees for incumbent students. The figure reveals that in the early 2000s, completion rates were somewhat higher at fee universities, a difference that stayed constant until 2005. In 2006, the gap between the two groups widened slightly, perhaps reflecting a small anticipation effect. Strikingly, completion rates at fee universities then rose differentially and markedly after the introduction of fees. This increase is visible already for the first affected cohort of 2001, and it is even more pronounced for the later cohorts, which were exposed to tuition fees for a longer period of time. Thus, the introduction of general tuition fees appears to have increased completion rates among affected students.

4.2 Empirical specification

We next examine whether the descriptive patterns in Figure 2 are confirmed in a regression analysis. We estimate versions of the following difference-in-differences model:

$$C_{cu_s} = \sum_{c=2001}^{2004} \beta_c^{int} \cdot Fee_{cu_s} + \theta_c + \omega_{u_s} + \varepsilon_{cu_s}. \tag{2}$$

Here, C_{cu_s} and Fee_{cu_s} are defined as above, θ_c is a vector of starting cohort dummies, ω_{u_s} is a vector of starting university dummies, and ε_{cu_s} is the error term. The key parameters of interest are the four β_c^{int}'s, which denote the cohort-specific effects of tuition fees on degree completion. These cohort-specific impacts account for the fact that students who enrolled in different years had to pay different amounts of fees, holding time to completion constant. Importantly, while the regression in Equation 2 is based on data at the university-cohort level, we weight our observations using cell size to recover the coefficients from the equivalent individual-level regression (that is, a regression where the dependent variable is a dummy for completion within six years). In our main specification, we cluster standard errors at the level of starting university, reflecting the influence universities had on whether and how much tuition was levied. In robustness checks, we also work with alternative ways of statistical inference.

With the inclusion of fixed effects for starting cohort and starting university, the two-way fixed effects (TWFE) specification in Equation 2 can account for general differences in completion rates over time and across universities.[14] Moreover, by focusing on cohorts which enrolled before 2005, we ensure that the students in our sample did not endogenously select into or out of fee-charging

[14] Note that since there is no staggered treatment adoption, this is the preferred estimator.

universities at the time of enrollment. The treatment is thus exogenous, and our main identification assumption is that completion rates would have followed the same trend in treatment and control universities in the absence of the introduction of fees. Below, we provide substantial evidence in support of this assumption.

Finally, note that our estimates correspond to intent-to-treat effects for three reasons: first, in the main analysis, we always assign treatment based on the year and university of initial enrollment and do not condition on being enrolled in 2007, when fees were actually introduced. This implies that, for example, all students who in 2001 enrolled at a university which introduced fees are considered treated, even though some of these students graduated before 2007 and as a result never had to pay any fees. Second, students could potentially avoid paying tuition by switching to a non-fee university, an issue that we discuss in more detail below. Third, our data do not allow us to observe who actually paid tuition fees, and we know that particular subgroups of students, which we cannot identify in our data, were exempt from paying (see Section 2). For all three reasons, our estimates reflect a lower bound of the impact of tuition fees on degree completion.

4.3 Degree completion within six years

4.3.1 Main results

Table 2 reports our main results: cumulative degree completion within $d = 6$ years. Confirming the patterns in Figure 2, column 1 shows that tuition fees increase degree completion within six years. This effect appears to differ somewhat across cohorts, which are indicated in square brackets below the coefficients: for example, while completion rises by 2.8 percentage points (pp) for the 2001 cohort, the corresponding impact for the 2004 cohort is 5.9 pp. These differences are marginally statistically significant, with an F-test for the equality of the four coefficients in column 1 yielding a p-value of 0.06. One potential explanation for these differences is that later cohorts responded more strongly to the introduction of fees because they had to pay higher amounts (up to 1,000 EUR for the 2001 cohort versus up to 4,000 EUR for the 2004 cohort in the example above). But these later cohorts were also affected earlier during their studies and thus had more time to react to the policy change. Because amount of fees and timing of treatment move in lock-step, we are unable to fully disentangle the exact causes of these apparent cohort differences. What does become clear, though, is that the effect of fees is highly non-linear: there is a large impact of paying some tuition versus none, but as the amount of tuition and the number of years treated quadruples, completion rates rise less than one-for-one.

The remainder of Table 2 reports estimates of variations of Equation 2 which test the robustness of these headline results. Column 2 adds university-level controls for the share of female students and the shares of students enrolled in different degree types. Column 3 instead controls for state-level GDP and unemployment. None of this changes the estimated coefficients much. In column 4, we include separate linear trends for fee and non-fee universities in order to account for potential differences in the development of completion rates between them. This decreases the estimated effects slightly but leaves them economically and, mostly, statistically significant. Finally, column 5 shows that results are robust to including all the controls added in columns 2-4 in the same regression. In light of this relative stability of the estimates across different models, we choose the most parsimonious regression in column 1, which only includes starting cohort and university fixed effects, as our preferred specification.

We investigate heterogeneity and further outcomes. We estimate effects separately by gender, field of study or socio-economic background. The latter is proxied by the per-capital income in the county of the high school. The estimates are similar for all groups (see Appendix Table A.3). We also investigate additional outcomes, in particular grades and university switching. We do not find evidence that fees affected either of these (see Appendix Table A.4).

4.3.2 Event study and further robustness

Based on the headline results in column 1 of Table 2, we conducted a battery of additional analyses and sensitivity tests. Thus, to provide further evidence that our estimates are not driven by differential trends in completion rates, we recast our results in terms of a difference-in-differences event-study framework. For this purpose, we estimate an augmented version of Equation 2 that includes interactions between the treatment indicator and indicators for several lead and lag cohorts.

The starting cohort 1999 serves as baseline. If trends were similar across fee and non-fee universities before the policy change in 2007, we would expect the coefficients on the interactions between the treatment indicator and the six lead cohorts (who initially enrolled at university between 1995 and 2000) to be close to zero. Figure 3, which visualizes the results of this regression, shows that this is indeed the case: the effect of tuition fees only materializes after they were actually introduced in 2007, with at most a small, statistically insignificant anticipation effect in 2006.

The Appendix compiles results from several further robustness checks that validate our findings. First, Appendix Table A.5 shows that our results are not driven by the existence or introduction of long-term tuition fees. We also confirm that our estimates remain largely unchanged when we control for other policies that affected universities around the time that general tuition fees were introduced (such as the transition to a system of two-tier degrees), or when we relax some of the sample restrictions discussed in Section 3.2. Second, Appendix Table A.6 shows that our headline estimates remain statistically significant under different assumptions about the error structure, and when we cluster the error terms at the state level using different wild-cluster bootstrap procedures suggested by Cameron, Gelbach, and Miller (2008). Finally, our Online Appendix provides extensive evidence that missing information on university and year of initial enrollment in the Final Examinations Register is not driving our results. Specifically, we show that the incidence of missing information is unrelated to the treatment in our estimation sample and that we can replicate our headline estimates using various alternative sample restrictions or imputation procedures.

4.4 Degree completion within different time frames

Table 3 reports estimates of the effect of tuition fees on degree completion within different time frames. In these regressions, the outcome is computed as the share of students in a starting-university-by-cohort cell graduating within the number of years d indicated in the column header, and treatment is defined as an indicator for the starting university introducing fees within this time frame after initial enrollment. Note that redefining the outcome in this way necessarily leads the effects to be identified from cohorts other than those indicated in Equation 2: for example, the effect of fees on completion within $d = 5$ years in 2008 is estimated for the $2008-5 = 2003$ cohort, whereas the effect of fees on within $d = 10$ years in 2009 is estimated for the $2009 - 10 = 1999$ cohort.[15]

A first interesting result in Table 3 is that students are more likely to complete their degree within time frames shorter than six years. Focusing on the effect in 2007, we see that fees increase completion within four years by 0.65 pp (column 2), completion within five years by 2.15 pp (column 3), and – as also shown in Table 2 – completion within six years by 2.82 pp (column 4). In contrast, we do not find evidence that fees affect completion within three years (column 1), most likely because graduating with a single-tier degree in such short time is very difficult. Going beyond impacts in 2007, the estimates show that for a given time frame, the increase in completion rates tends to be larger if the amount of fees is higher, corroborating the findings in Table 2.

A second key finding in Table 3 is that the introduction of general tuition fees does not seem to have affected cohorts which had reached their seventh or later year of studies in 2007. The likely reason is that universities that introduced general tuition fees in 2007 had charged long-term fees of 500 EUR per semester already before. Thus, little changed with the introduction of general tuition fees for the cohorts that had started university in or before the year 2000. The estimated impacts for these "placebo" cohorts, which are shaded in gray in the table, reflect this fact: most of the coefficients are close to zero, and none of them is statistically significant at conventional levels.

Beyond cross-cohort comparisons, an important feature of Table 3 is that it allows us to study dynamic impacts by tracking the effect of tuition fees for a specific cohort over time. This is possible due to the fact that across the different regressions, the same starting cohort identifies the effects for different amounts of fees: for example, students in the 2004 starting cohort identify

[15] More formally, in constructing the outcomes in Table 3, in Equation 1 we allow for different durations in completing the degree, i.e. $d \in [3, 11]$. The lower limit of $d = 3$ is due to our focus on students who initially enrolled in or before 2004; estimating the effect of fees on completion within two years would require us to include later cohorts, which potentially selected into universities. Similarly, the effect of paying fees for at most one year on completion within $d = 11$ years is estimated for the 1996 cohort, using the 1995 cohort (and the cohorts in non-fee universities) as control. Estimating longer-term effects would require data going back even further in time, which are not available. The fact that the different outcomes are measured for different cohorts, together with our focus on the cohorts initially enrolling in 1995-2004 (and graduating in 2001-2010), explains the differences in sample sizes across columns in Table 3.

the impact on completion within three years in column 1 and the impact on completion within six years in column 4. To explore dynamic effects, Figure 4 groups the estimates from Table 3 by cohort and plots them against years since initial enrollment. One thing that becomes clear from this visualization is that the impact of fees tends to materialize when cohorts are in their fifth and sixth year of studies. This makes intuitive sense since most degrees are designed to take at least four-and-a-half years, and in practice usually take between five and seven years to complete (see Figure 1).

Figure 4 also yields another important insight: the effect of fees on completion appears to be persistent. For example, the impact on completion within nine years for the 2001 cohort is only slightly smaller than the corresponding impact after six years. This is a crucial finding since it suggests that our estimates do not merely reflect faster completion, which would be a natural response to fees that are levied per semester, but also a higher incidence of completion. To provide further support for this interpretation, Appendix Table A.7 shows estimates from regressions like in Table 3 but in which the sample period is extended until 2012. This has the advantage that we can follow cohorts who were affected by the introduction of fees up to 11 years after their initial enrollment, but comes at the cost that the longer-term estimates could be influenced by the abolition of fees starting in 2010, which often also included abolition of the previous long-term fees (see Appendix Table A.1). The results reveal that students who had to start paying fees had higher completion rates even 10 and 11 years after they initially enrolled. Given that only very few students complete their degree after 11 years (see Section 3.2), this strongly suggests that the introduction of fees led to a permanently higher degree attainment among incumbent students.

4.5 Mechanisms

4.5.1 Study conditions

We now investigate potential mechanisms behind the effect of tuition fees on degree completion. One possibility is that these effects at fee-charging universities are due to improved educational quality.[16] As described in Section 2, universities were required by law to use fees to improve study conditions: Most of the money was invested into hiring tutors, or upgrading technological equipment and libraries. To test if these improvements drive higher completion rates, we would ideally like to estimate the impact of fees on the quantity and quality of such resources. However, suitable university-level data to test the role of improved educational quality do not appear to exist.

An alternative way to judge the relevance of improvements in study conditions is to revisit the estimates in Sections 4.3 and 4.4. While we can not rule out this channel as such, the respective findings suggest that it is rather unlikely to explain the entire rise in completion rates. On the one hand, hiring tutors and buying equipment and books takes time, and most universities only received the first tuition payments in mid or late 2007. Thus, additional resources could have affected students who graduated in or before 2007 for at most a few months, but Tables 2 and 3 still show large impacts of fees on these students' completion rates. On the other hand, the additional resources were not targeted at particular groups of students, but rather aimed at improving general study conditions. If resources were driving the increase in graduation rates, and if students are equally responsive to changes in resources throughout the course of their studies, we would therefore expect to see effects across all cohorts of students. However, Table 3 shows no impact on students in their seventh or later year of studies.

4.5.2 Study effort

Another plausible mechanism behind the rise in completion rates is that students changed their behavior when tuition fees were introduced. For example, they might have increased their study effort in response to the fact that they now had "skin in the game," with the aim of reducing their time to degree and thus the total amount of tuition to be paid.[17] Alternatively, the fees might have

[16] A number of papers suggest the importance of university resources for educational attainment (e.g. Bound and Turner, 2007; Bound, Lovenheim, and Turner, 2010, 2012; Deming and Walters, 2018).

[17] Of course, as described in Section 2, studying at university was costly in other ways and students therefore had "skin in the game" already before 2007. However, the intense debate about the introduction of fees in the media suggests that unlike costs for housing and foregone earnings, tuition fees were a particularly salient expense. Note also that like in many other settings, the fees were relatively small compared to the lifetime wage premium of a university degree vis-a-vis a completed vocational education, which a recent study estimates to be 387,000 EUR (Piopiunik, Kugler, and Wößmann, 2017).

triggered a sunk-cost effect that raised students' psychological cost of failing, thus motivating them to study harder (Thaler, 1980; Arkes and Blumer, 1985; Ketel et al., 2016).[18] Another possibility is that the move of the direct cost of studying above zero prompted a change in behavior, in line with previous literature documenting the role of zero as a "special price" (Shampanier, Mazar, and Ariely, 2007).

An increase in study effort could explain larger parts of the higher completion rates found above. We examine this potential mechanism using data from the German Student Survey, which contains detailed information on students' time use. We again focus on students who had enrolled in or before 2004 and who had not yet reached the seventh year of their studies and restrict the sample to universities located in states included in our main estimation sample.[19] Because the only wave of the survey that coincided with tuition fees being charged was conducted in the winter term of 2009, this implies that we focus on students enrolled in the fifth and sixth year of their studies.

Figure 5 plots the number of weekly hours students spent on different activities, separately for students at fee and non-fee universities. Panel A shows a marked increase in the hours spent on self-study for incumbent students at fee-charging universities after 2006. Panel B similarly shows an increase in the amount of time spent attending lectures at these universities relative to non-fee universities. Together, these changes imply a large differential increase in the amount of time that students at fee universities invest in their studies, as depicted in panel C. In contrast, panel D shows that the number of hours students spent working in the labor market hardly changes after the introduction of fees in 2007.

Table 4 reports results from difference-in-differences regressions that quantify these changes. Panel A shows estimates from specifications that only control for cohort and university fixed effects.[20] Tuition fees raise time spent studying outside of the classroom by 1.8 hours (column 1, not

[18] Ketel et al. (2016) test for sunk-cost effects in the context of four Dutch universities. The authors randomly assigned tuition discounts to students who had signed up for costly extra-curricular tutorials and measured their attendance. Although there was no effect on average, students identified as being prone to sunk-costs were less likely to attend the tutorials when receiving the discount, in line with sunk-cost theory.

[19] The effects on completion within six years are robust to restricting the main estimation sample to universities also included in the analysis of the German Student Survey.

[20] Due to the small sample size, we do not estimate separate effects by cohort like in the previous tables.

statistically significant at conventional levels) and time spent attending lectures by 1.4 hours (column 2). Total study time thus significantly increases by 3.2 hours (column 3), which corresponds to 11% of the mean. Column 4 shows that on average, fees do not affect students' labor supply, a finding that we confirm using a much larger sample of students in the German Microcensus, see Appendix Table A.8. This suggests that the increased study time is due to a reduction in free time. Panel B shows that these estimates are almost unchanged when we add individual-level controls for high school GPA, parental education, and demographic characteristics to the regressions. Overall, these results thus support the idea that the increased completion rates are partly due to a rise in study effort.

An important advantage of the Student Survey data is that they contain detailed information on students' socioeconomic background. In panel C, we use this information to examine whether the effect of tuition fees on time use differs by parental education. The results indicate that the effect is similar for students with and without college-educated parents: the interaction terms are mostly small relative to the main effects and never statistically significant at conventional levels. Thus, tuition fees appear to raise study effort independently of students' socioeconomic background, a result that will inform our policy discussion in Section 6.

4.6 Abolition of fees

In this section, we consider effects of the abolition of tuition fees, which took place between 2008 and 2014, depending on the state (see Appendix Table A.1). For this analysis, we only consider students in treatment states who started to study when fees were in place, i.e. in or after 2007. Afterwards, fees were only abolished and thus there is at most one change in the treatment indicator for each individual in the sample. Moreover, we focus on Bachelor students, as in the respective period the vast majority of first-time students enrolled in such degrees. We use cumulative Bachelor's degree completion after three years as outcome variable because the nominal study period for Bachelor's degrees is substantially shorter compared to the single-tier degree programs analysed above. The analysis considers a total of of 1,079,544 students who initially enrolled between 2007 and 2014 at 76 universities in fifteen states (excluding Hesse, as before).[21] Table 5 shows the results of the abolition of tuition fees on Bachelor's degree completion within three years. Column 1 is based on the standard TWFE DiD estimator, our initial main specification without control variables. Based on this estimator, we find that the abolition of tuition fees reduced BA degree completion by 2.76 pp.

However, the TWFE estimator is not preferred in this setting as, in contrast to the introduction, the abolition was staggered over time across states. In the TWFE estimator of column 1, cohorts in states that abolished tuition fees early (e.g. Saarland in 2010) serve also as control group for the ones in states that abolished tuition fees later (e.g. Bavaria in 2013). This is problematic if effects of fees change over time. To account for potentially dynamic effects of the abolition of fees, we therefore apply the DID_M estimator proposed by De Chaisemartin and d'Haultfoeuille (2020). This estimator is valid even when treatment effects change over time. It estimates the direct average treatment effect at the time of abolition for "switchers" from a fee to a no-fee system. The DID_M estimator uses students from classic control states in which tuition fees were never introduced and from treatment states around the year of abolition, a total of 342,770 observations, to estimate effects on 88,018 switchers.

Column 2 of Table 5 shows results based on the DID_M estimator and reveals that the abolition of tuition fees reduced Bachelor's degree completion within three years by 2.26 pp. The coefficient is rather similar to that obtained using the classical TWFE estimator in column 1, which suggests that treatment effect dynamics do not cause significant bias in the present setting. In terms of magnitude, the estimated effect corresponds to more than 11 percent of the overall Bachelor's degree completion rate at the time (compare Appendix Figure A.1). Therefore, it is very similar to the effects we find for the introduction of fees. Hence, effects of fees on cohorts that enrolled in the absence of fees (introduction) and on students that started at a state and in a year where fees where active (*"Fee Starter"*, abolition) appear symmetric.

[21] Here, we again do not observe the year of enrollment for all observations. However, the number of observations for which this is the case is now significantly lower, which allows to consider all states. Also, we calculate the starting year as difference between the year of completion and the number of years enrolled whenever missing (see Online Appendix Table O.3 for estimations using this approach in the analysis of the introduction of fees.)

Columns 3-7 assess the robustness of the DID_M estimate to the same categories of control variables used before. Controlling for Fee Starter in column 3 does not affect our estimates and emphasizes that we estimate effects for exactly this subset. Controlling for a set of university controls, state-level controls and group-specific linear trends in columns 4, 5 and 6 respectively, marginally increases our estimate. The estimate is largest when combining all the controls in column 7, which suggests a 3.18 pp reduction of BA completion rates in three years.

5 Effects at the extensive margin

In this section, we examine how the introduction of tuition fees affected enrollment at university. Specifically, we exploit variation in fees across states and years to estimate whether high school graduates from states that introduced fees were differentially likely to go to university, independently of the location of the university. Our analysis builds on two previous studies, which investigate this question using aggregate, state-level data, and which come to somewhat different conclusions: whereas Hübner (2012) finds that fees reduced enrollment by 2.7 pp, Bruckmeier and Wigger (2014) identify a smaller, statistically insignificant effect of 0.9 pp based on a slightly different specification. In contrast, our analysis uses individual-level data on university students and also differs from those papers in some other aspects, which we describe in detail below.

5.1 Data and descriptives

For our investigation, we merge the Student Register data, which allows us to observe in which state and year university students graduated from high school, with aggregate data on the number of graduates from high schools per state and year (Federal Statistical Office, 2012).[22] This enables us to construct our main outcome for the extensive margin analysis as the share of high school graduates who enroll at university up to one year after their graduation:

[22] Note that we only consider graduates from academic-track high schools (*Gymnasien*) who can enroll at university without obtaining any further qualifications.

$$E_{gs} = \frac{\sum_{\lambda=g}^{g+1} \sum_{h \in g,s} Enrolled_h^{t=1,y=\lambda}}{\sum_{h \in g,s} HSGraduate_h}. \quad (3)$$

Here, h denotes high school graduates, g denotes graduation cohorts, and s denotes states. The indicator for first-year, freshman enrollment of high school graduate h in year $y \in [g, g+1]$ as recorded in the Student Register is denoted by $Enrolled_h^{t=1,y=\lambda}$. We focus on enrollment up to one year after high school graduation because during our study period, most men in Germany had to do compulsory military service for about 10 months, with many serving immediately after finishing high school. Exploiting our population-level data, we measure enrollment at any German university, thus accounting for potential avoidance behavior via moving to a non-fee state. $HSGraduate_h$ is a dummy for high school graduation. While we do not have individual-level data on this measure, the denominator in Equation 3 corresponds to the aggregate data on the number of high school graduates per state and year described above. We compute E_{gs}, the share of high school graduates in cohort g and state s who enroll at university, for the graduation cohorts 2001 to 2010. This measure differs from that used by Hübner (2012) and Bruckmeier and Wigger (2014) in two ways. First, in line with our analysis of intensive margin effects, we focus on enrollment at university, whereas they also consider enrollment at technical college. Second, while we measure enrollment up to one year after high school graduation, their main analyses focus on enrollment in the year of graduation only.

Figure 6 plots E_{gs} by high school cohort separately for states which did not introduce tuition fees and for states which introduced fees for newly-enrolled students in 2006 (panel A) and 2007 (panel B). The vertical solid lines in these graphs mark the actual introduction of fees, and the vertical dashed lines denote the year before this introduction. Students in the cohort enclosed between these two lines had to pay tuition fees from their second, sophomore year onward if they enrolled immediately after graduating from high school, or from their first, freshman year onward if they enrolled in the year after high school graduation. Students in all later cohorts had to pay from their freshman year onward. Figure 6 shows that there were some level differences in enrollment rates between states that did versus did not introduce tuition fees. However, these differences stayed roughly constant until the introduction of fees. After that, enrollment rates in fee-levying states decreased visibly, suggesting that the introduction of fees lowered enrollment at university.

5.2 Empirical specification and regression results

To investigate the impact of tuition fees on enrollment more formally, we estimate versions of the following difference-in-differences model:[23]

$$E_{gs} = \beta^{ext} \cdot FreshFee_{gs} + \gamma^{ext} \cdot FreshFeeNext_{gs} + \psi_g + \phi_s + \varepsilon_{gs}. \tag{4}$$

Here, E_{gs} is defined as above, ψ_g is a vector of dummies for high school graduation cohort, ϕ_s is a vector of state dummies, and ε_{gs} is the error term. $FreshFee_{gs}$ is an indicator that takes value 1 if fees were charged from freshmen in state s in the year that cohort g graduated from high school and 0 otherwise. We also control for $FreshFeeNext_{gs}$, which is an equivalent indicator for fees being charged in the year after high school graduation. We weight regressions using the size of the high school graduation cohort and cluster standard errors at the state level. The main parameter of interest is β^{ext}, which measures the impact of tuition fees in the year of high school graduation on enrollment. Differently from the analysis of intensive margin effects, we do not allow for further cohort differences as there is no reason to believe that these exist ex ante: once enrolled, all students should expect to pay the same amount of fees, and accordingly Figure 6 suggests no differential effect on enrollment across treated cohorts.[24] Finally, note that also in this analysis, we estimate intent-to-treat effects due to exemptions for some groups of students and due to potential avoidance behavior, with students from fee states enrolling at universities in non-fee states.

[23] The use of the simple two-way fixed effects model is justified because the two treatment dates are very close to each other. As a result, potentially problematic subgroups get very little weight.

[24] An exception is the cohort graduating from high school in the year before fees were introduced: students in this cohort paid from their second year onward if they enrolled immediately after graduating from high school, and from the first year onward if they enrolled in the year thereafter. In Equation 3, we capture the impact for this cohort by γ^{ext}. Our estimates show that this cohort was about 1 pp less likely to enroll at university if exposed to fees.

Table 6 reports our results. Column 1 shows that tuition fees reduce enrollment at university by 3.9 pp, corresponding to 6.8% of the outcome mean. Columns 2-5 test the robustness of these headline results. Column 2 adds controls for a high school policy reform that changed the size of graduating cohorts in a specific year in some states.[25] Column 3 instead adds controls for state-level GDP and unemployment in the year of high school graduation. Column 4 includes separate linear trends for the two groups of states introducing fees for freshmen in 2006 and 2007. Finally, column 5 shows estimates from a regression that includes all the controls added in columns 2-4 simultaneously. Our results are robust to all of these checks. We furthermore confirmed that our estimates remain statistically significant when we compute standard errors using wild cluster bootstrap procedures (results available upon request).

The finding that tuition fees reduce enrollment is in line with results from the previous literature studying the impacts of costs of higher education. Specifically, our estimate that 1,000 EUR of fees per year lower enrollment by 3.9 pp is very similar to the conclusion by Deming and Dynarski (2010) that eligibility for 1,000 USD of financial aid increases enrollment by around 4 pp in the United States. It is also broadly similar to the estimates that 1,000 USD higher "sticker price" fees reduce enrollment by 5.1 pp at community colleges in Texas (Denning, 2017) and by 1.4 pp at four-year public universities in the United States (Kane, 1995). Finally, our estimate is somewhat larger in absolute value than the reductions found by the two previous studies of the German reform (2.7 pp by Hübner (2012) and 0.9 pp by Bruckmeier and Wigger (2014)), most likely due to the differences in data and the sample described above.

6 Policy calculation

The analysis so far has shown that tuition fees decrease enrollment at the extensive margin, but increase degree completion at the intensive margin. This implies that their overall impact on edu-

[25] In particular, a shortening of the duration of high school by one year led to two cohorts graduating at the same time. In column 2, we control for this cohort crowding by adding a dummy for such "double cohorts" to our regression. We also add a dummy for cohorts graduating in the year before such cohorts, as their study decisions were also affected by the reform. For additional details, see Huebener, Kuger, and Marcus (2017), Huebener and Marcus (2017), and Marcus and Zambre (2019).

cational attainment is ambiguous. In this section, we combine our extensive and intensive margin estimates to quantify the effect of tuition fees on the number of university graduates, a measure of educational attainment widely used in policy debates (e.g. OECD, 2018). Since discussions about tuition fees are often motivated by the high public cost of higher education, we also compute how their introduction in Germany influenced these costs. In what follows, we first derive expressions for calculating these two measures in the absence and presence of fees. In a second step, we quantify the changes due to the introduction of fees, in this accounting exercise.

6.1 Number of university graduates and costs

Starting with the case of no fees, let HS be the number of high school graduates, p the fraction of them who enroll at university, and r the fraction of university students who complete their degree. The number of university graduates can then be calculated as

$$Graduates^{NoFees} = HS \cdot p \cdot r \tag{5}$$

These students are costly for universities because they need professors who lecture them, auditoriums to host the lectures, and libraries that stock textbooks, among other things. We denote the total cost generated by a student between her initial enrollment and her graduation by M. Students who enroll but do not graduate also generate costs, but these are lower because these students drop out early and thus require the university's resources for less time. We denote the total cost generated by a student who does not graduate by $m < M$. The total public cost of higher education can then be written as

$$Cost^{NoFees} = HS \cdot p \cdot (r \cdot M + (1 - r) \cdot m) \tag{6}$$

As the empirical analyses in Sections 4 and 5 show, tuition fees affect both the enrollment of high school graduates at the extensive margin and the degree completion of university students at the intensive margin. Taking this into account, we calculate the number of graduates when tuition fees are levied as

$$Graduates^{Fees} = HS \cdot (p + \beta^{ext}) \cdot (r + \beta^{int}) \tag{7}$$

Here, β^{ext} denotes the extensive margin effect of fees as defined in Equation 4, and β^{int} denotes the intensive margin effect of fees as defined in Equation 2. Similarly, the public cost of higher education in the presence of tuition fees is

$$Cost^{Fees} = HS \cdot (p + \beta^{ext}) \cdot ((r + \beta^{int}) \cdot M + (1 - (r + \beta^{int})) \cdot m) \tag{8}$$

6.2 Impact of fees on the number of university graduates and costs

Using the expressions derived above, we can compute the effects of tuition fees on the number of university graduates and the public cost of higher education as $\Delta Graduates = Graduates^{Fees} - Graduates^{NoFees}$ and $\Delta Cost = Cost^{Fees} - Cost^{NoFees}$, respectively. To quantify these effects, we set $HS = 1$ without loss of generality. Using our sample means, we further set $p = 0.57$ (see Table 6), and we consider both completion within six years, setting $r = 0.28$, and completion within ten years, setting $r = 0.58$ (see Figure 1). The average university graduate in 2008 had studied for 5.8 years (Federal Statistical Office, 2009) and generated a total cost of 49,800 EUR (Federal Statistical Office, 2010). In contrast, drop-outs studied for an average of 3.65 years (Heublein et al., 2010). For the purpose of our calculations, we assume that costs are proportional to study time and set $M = 49,800$ and $m = 3.65/5.8 \cdot M = 31,340$. Finally, based on our findings above, we let $\beta^{ext} = -0.04$ (see Table 6) and $\beta^{int} \in \{0.03, 0.04, 0.06\}$, reflecting the fact that the intensive margin impact appears to vary with the amount of fees charged (see Table 2).

Before turning to the results, we point out that our calculations rely on a number of assumptions. First, our intensive margin effects are estimated on the population of students who enroll in the absence of fees, and we assume here that effects are the same for the population of students who enroll when fees are levied. Symmetric intensive margin effects of the abolition of tuition fees described in Section 4.6 suggest that this is in fact a valid assumption.[26] Similar impacts of fees on study effort among incumbent students for students from different socioeconomic backgrounds as presented in Table 4 further support this assumption.

Second, we interpret the intensive margin estimates on completion within six years as reflecting a higher incidence of completion, rather than just faster completion. Our finding in Table 3 that impacts are similar when looking at completion within nine years lends support to this interpretation. Third, the cost calculation above ignores the increase in universities' income due to tuition fees. Given that improved financial resources appear to play only a limited role for explaining our intensive margin effects (see Section 4.5), public contributions to universities could potentially be reduced without affecting completion when tuition fees are levied. In this case, our calculation will underestimate any savings in the cost of higher education due to fees.

Table 7 presents the results of our policy calculation. Panel A considers the scenario where the number of graduates is measured after six years, that is, r is set to 0.28. Across the whole range of magnitudes of intensive margin effects documented in our main results, tuition fees increase the number of university graduates and decrease the public cost of higher education. The impact on graduates rises with β_{int}, as the higher completion rates of enrolled students more and more offset the reduced enrollment at the extensive margin. At the same time, costs increase because graduates are more expensive than drop-outs for universities' budgets. For our highest estimate of $\beta_{int} = 0.06$, which corresponds to the effect of paying at most 4,000 EUR, tuition fees increase the number of university graduates by 2.1 pp and decrease the cost per high school graduate by 873 EUR. Panel B shows that even when we assume that a majority of students eventually complete their degree ($r = 0.58$), tuition fees do not affect the number of graduates much, but still reduce the public cost of higher education.

These results have important implications for education policy. On the one hand, they show that countries in which public higher education is currently free of charge are able to introduce

[26] An alternative way to compute the combined extensive and intensive margin effects of fees that does not rely on this assumption would be to estimate the impact of the introduction of fees on completion for students who enrolled after the fees were introduced. However, the swift abolition of fees renders this kind of analysis unfeasible.

moderate fees, and thus shift part of the cost from the government to individuals, without decreasing educational attainment. On the other hand, they suggest that in countries which currently do charge tuition fees, completely abolishing these is not going to lead to large gains in educational attainment, despite larger public costs.[27] The reason is that in the absence of fees, students' incentive to exert study effort – their "skin in the game" – is reduced, which leads some high school graduates who enroll at university to drop out before completing their degree.

7 Conclusion

Policymakers in many countries are debating whether or not to charge tuition fees for public higher education. One important question in these debates is how tuition fees affect educational attainment. In this paper, we shed light on this issue by estimating extensive and intensive margin effects of fees in Germany, where public universities in seven out of 16 states introduced fees of 500 EUR per semester in the mid-2000s. A key advantage of this setting is that we can estimate pure intensive margin impacts on degree completion by exploiting an unusual lack of grandfathering, which meant that students who had already enrolled suddenly had to start paying fees. Moreover, by using population-level data, we are able to move beyond effects for specific subgroups of students, which were the focus of most previous research on the impacts of fees and financial aid, and to incorporate general equilibrium effects.

We find that tuition fees substantially reduce enrollment at university, thus lowering educational attainment at the extensive margin. In contrast, degree completion rises at the intensive margin, with the effect size growing with the total amount paid. Our results suggest that this increase in attainment is mainly due to a rise in study effort and potentially also increased educational quality. To gauge the overall impact of fees on attainment, we combine our estimates of extensive and intensive margin effects in a simple accounting framework. We show that the decrease in

[27] Our results are informative for policymakers who consider abolishing existing fees if the effects of introducing and abolishing fees are symmetrical. While we could in theory test this using the abolition of fees in Germany after 2007, in practice this is complicated for two reasons. First, the last state abolished fees only in 2014, which is too recent to study effects on degree completion because at the time of writing, data are not available beyond 2016. Second, unlike with the introduction of fees, the staggered nature of the abolition over the course of six years means that anticipation effects likely played a much larger role.

enrollment and the increase in degree completion roughly offset each other, such that tuition fees do not change educational attainment in the population much.

Our results have implications for both research and policy. We show that studying effects at only the extensive or only the intensive margin, as much of the previous literature has done, can lead to wrong conclusions about how costs of higher education affect attainment. Moreover, it is valuable to examine the impacts at both margins separately because they differ in their underlying mechanisms and policy implications.

At the extensive margin, the effects of costs on enrollment do not seem to be very context-specific. Indeed, the impact that we document for the introduction of fees in Germany is very similar in size to the impacts found in the international literature on college aid (Deming and Dynarski, 2010). Previous studies have identified information frictions as an important determinant of enrollment decisions in the presence of fees and have shown ways in which these frictions can be resolved (e.g. Bettinger et al., 2012; Barr and Turner, 2018; Dynarski et al., 2018). Given the similar impacts of fees on enrollment, it seems likely that such policies have large external validity and thus could effectively mitigate the reduction in enrollment also in Germany, as well as other countries at the no-fee margin.

Intensive margin effects of the cost of higher education appear to be more context-dependent. The small existing literature on this topic has focused on particular groups of students and universities, with mixed findings (Garibaldi et al., 2012; Murphy and Wyness, 2016; Barr, 2019; Denning, 2019). We complement this literature by providing estimates for an entire country and at the no-fee-to-fee and fee-to-no-fee margin and argue that one potential explanation for increased study effort and completion is that zero is a special price in higher education. This could explain why our effects differ from those of some previous studies, which examine changes in the level of existing fees far away from the zero price.

Taken together, we believe that our findings are most informative for decisions involving the zero price margin. They suggest that in countries which currently charge tuition fees, completely abolishing fees might not lead to large gains in educational attainment unless ways are found to overcome resulting intensive margin effects. In contrast, countries in which higher education is currently free of charge might actually be able to increase educational attainment when moving away from the zero price, as long as negative effects at the extensive margin are mitigated.

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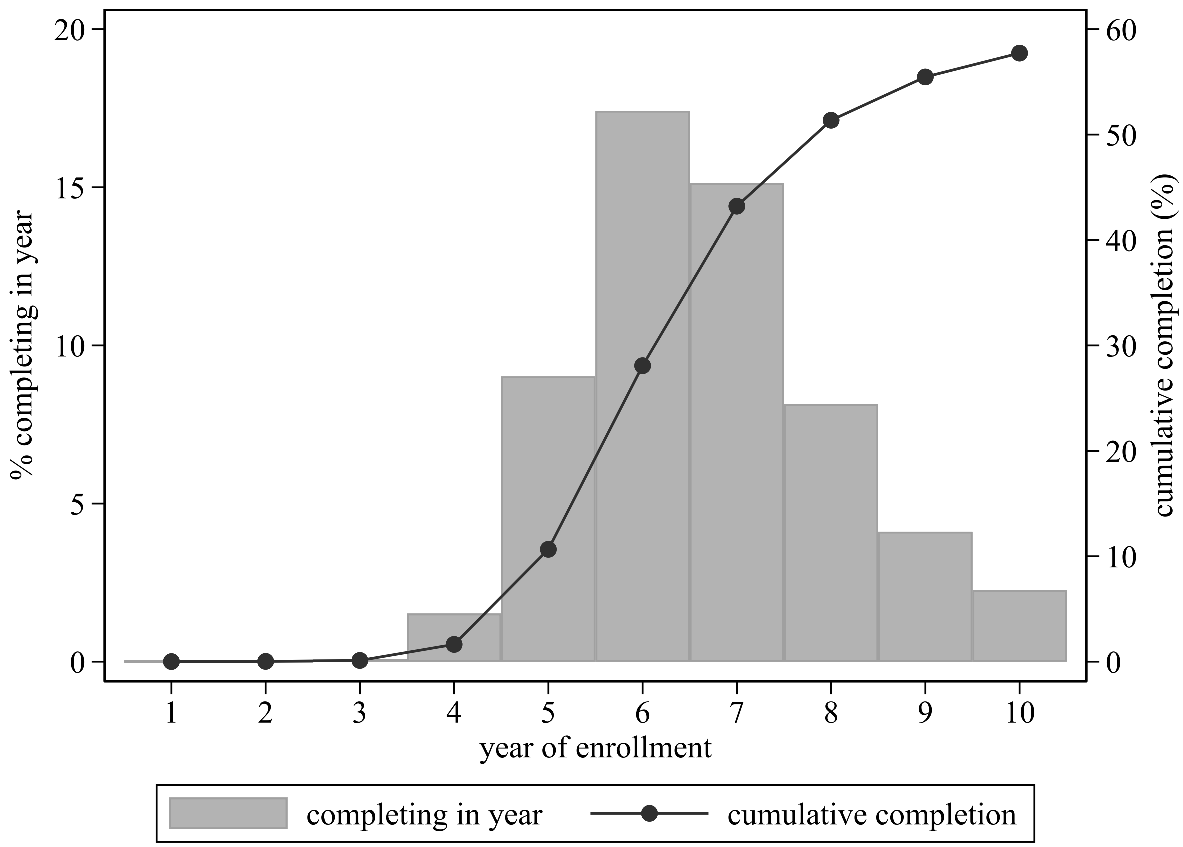
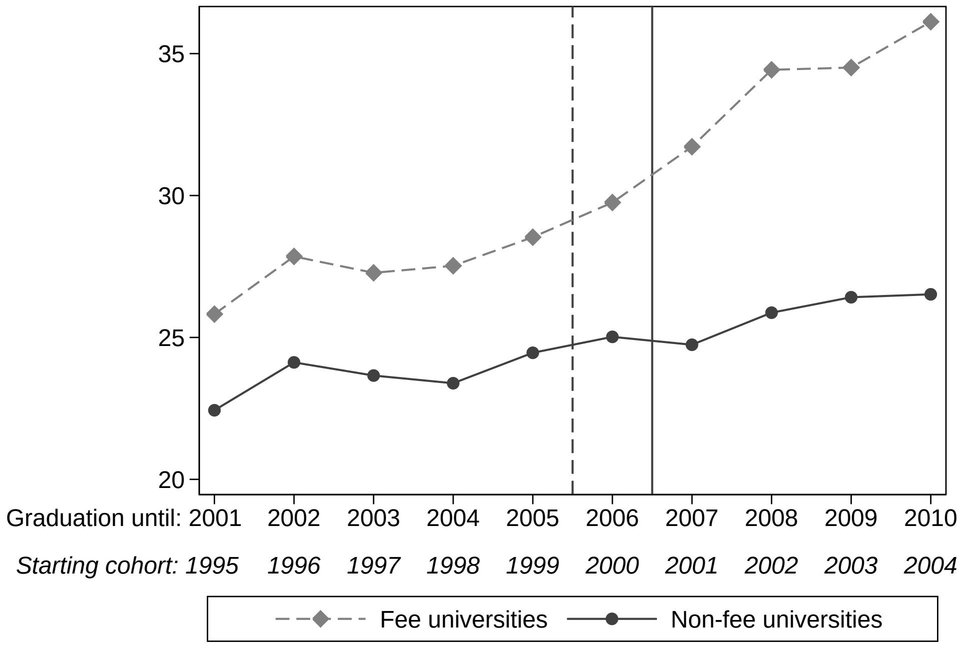
Figures and Tables

Figure 1: Degree completion at German universities



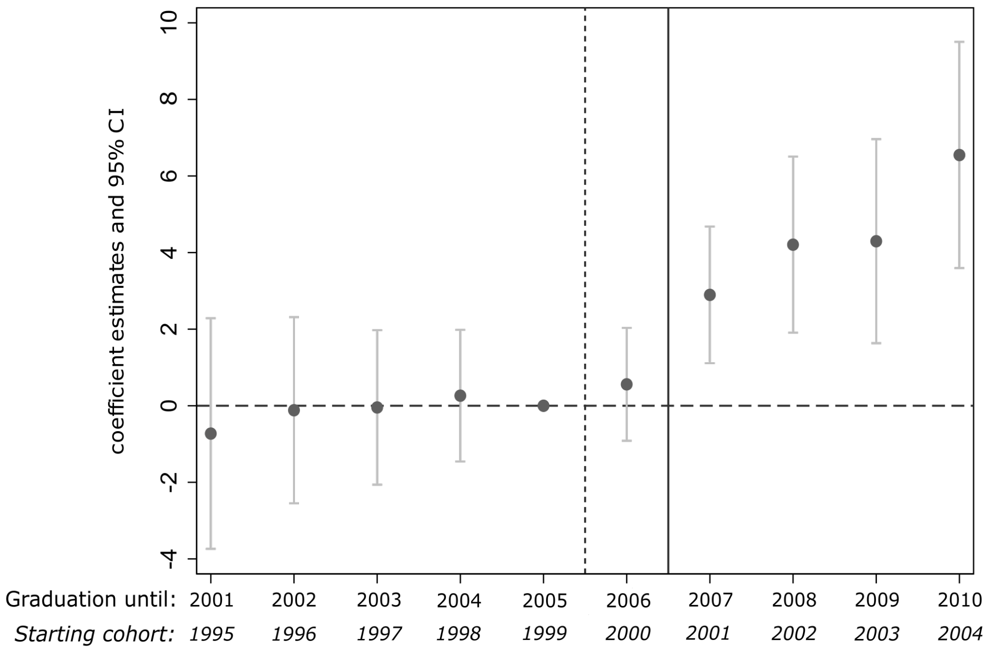
Notes: The figure shows the share of students completing in each of the first ten years after initial enrollment (left axis), as well as the cumulative completion rate over these years (right axis).

Figure 2: Share of students completing within six years



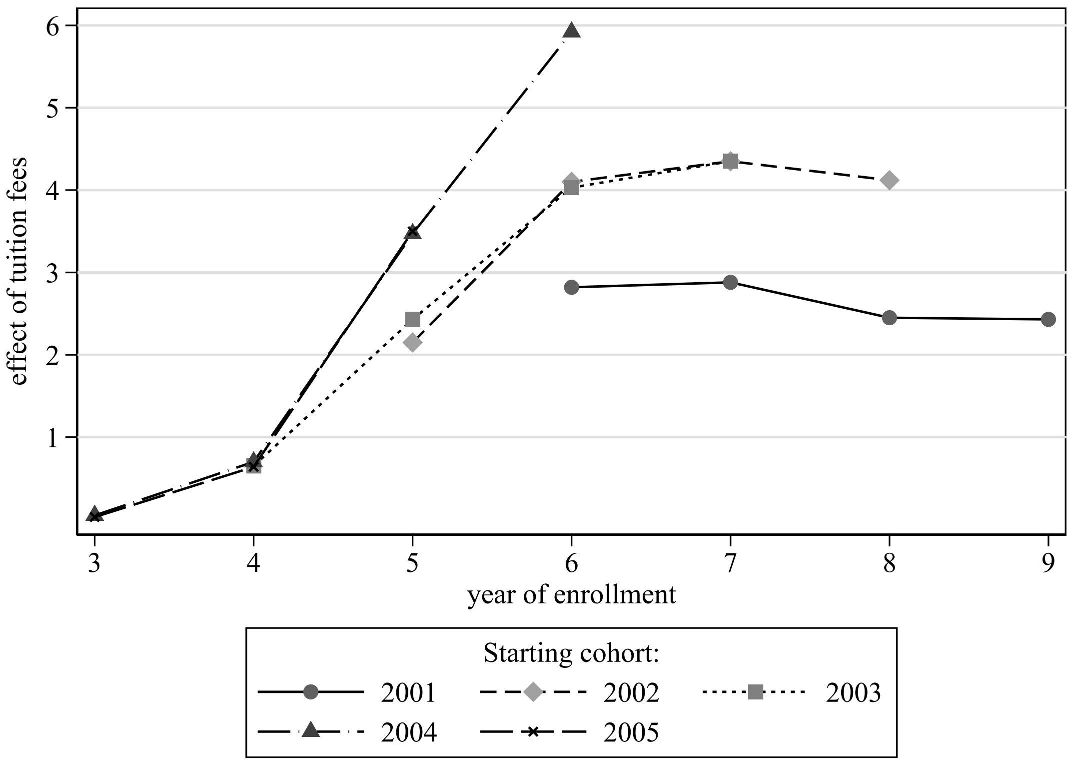
Notes: The figure plots the share of students who complete a degree within six years by cohort and treatment group. The vertical dashed line marks the passing of the first laws introducing general tuition fees in late 2005 and early 2006. The vertical solid line marks the actual introduction of tuition fees for incumbent students in 2007.

Figure 3: Difference-in-differences event study



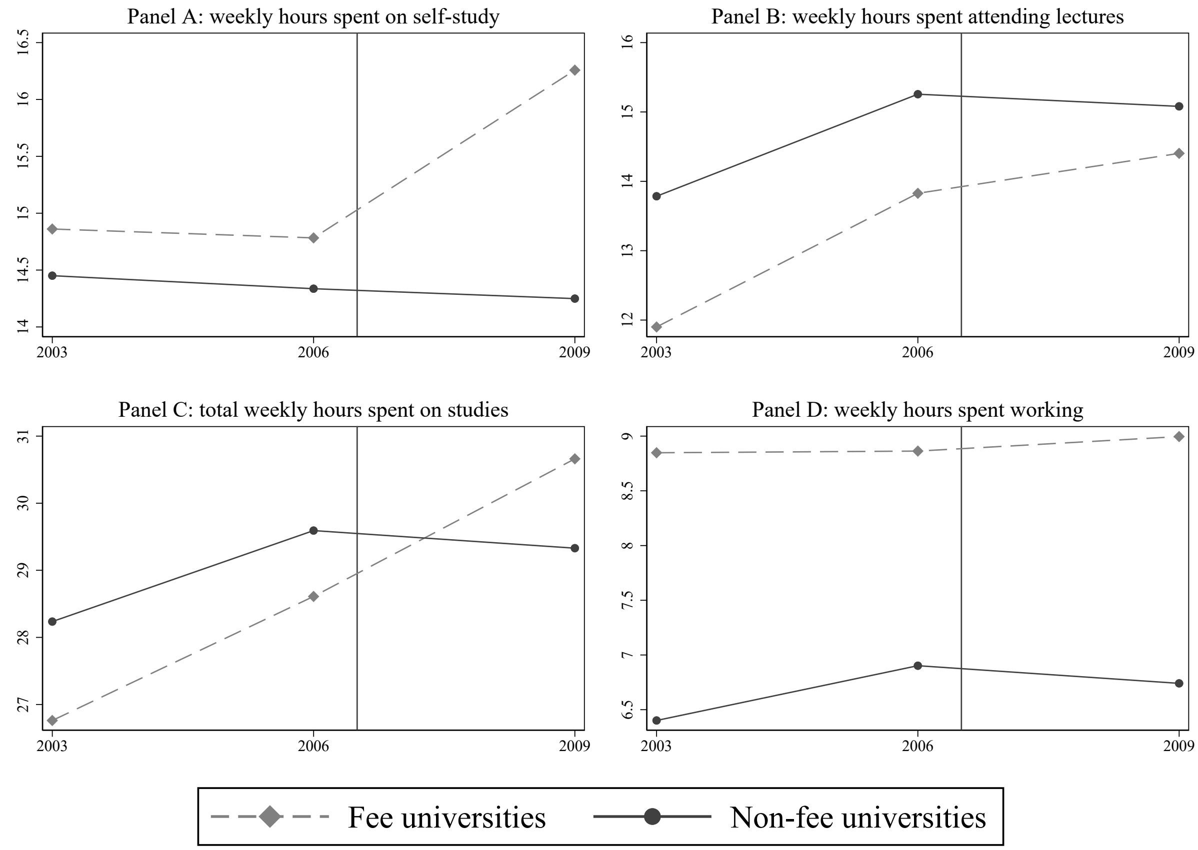
Notes: The figure plots point estimates and 95 percent confidence intervals from difference-in-differences event-study regressions (with the starting cohort 1999 being the reference category). The point estimates reflect the cohort-specific effects of tuition fees on degree completion within six years. The vertical dashed line marks the passing of the first laws introducing general tuition fees in late 2005 and early 2006. The vertical solid line marks the actual introduction of tuition fees for incumbent students in 2007. Confidence intervals are based on standard errors clustered at the level of starting university.

Figure 4: Effect of tuition fees over time, by starting cohort



Notes: The figure groups the point estimates from Table 3 by starting cohort and plots the impact of tuition fees on cumulative completion after different numbers of years since enrollment for each of these cohorts. See the notes to Table 3 for further details.

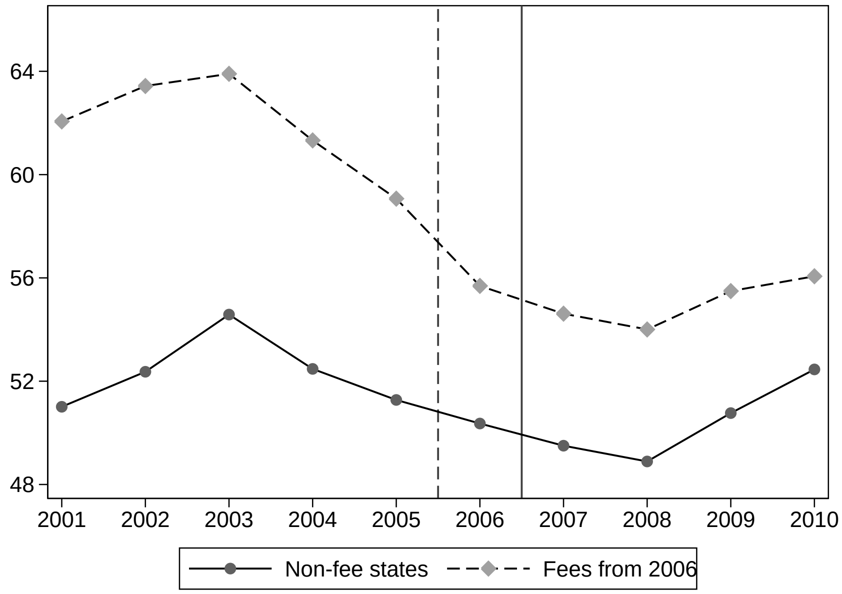
Figure 5: Students' time investment



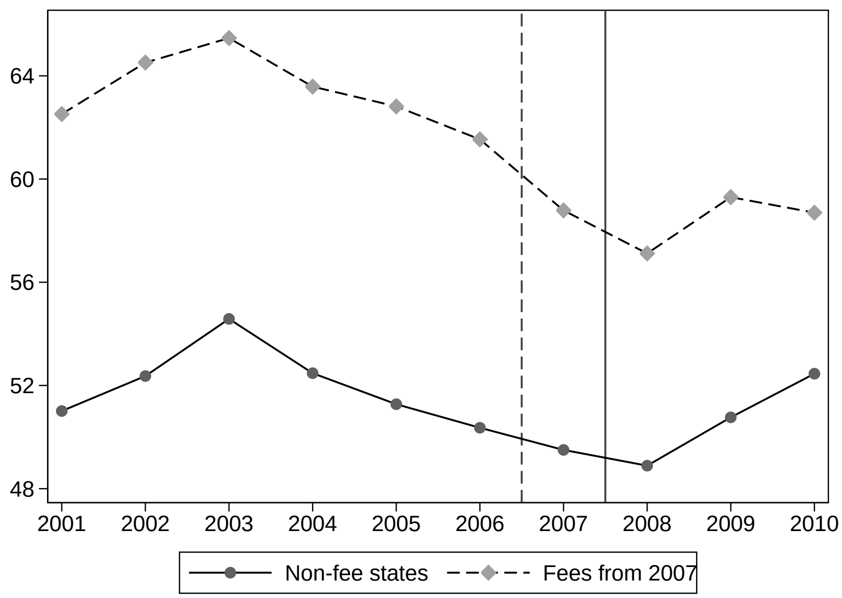
Notes: The figure plots the number of weekly hours students spent on different activities, separately for students at fee and non-fee universities. The analysis uses data from three waves of the German Student Survey conducted in the winter terms of 2003, 2006, and 2009. The sample is restricted to students who initially enrolled at university in or before 2005 and who had not yet reached their seventh year of studies, which implies that the sample includes students in the fifth and sixth year of their studies in the winter of 2009 (we select the corresponding cohorts for the previous waves). The sample is further restricted to students at universities located in the states included in our main estimation sample (nine universities). Panel A shows time spent on self-study, panel B shows time spent attending lectures, panel C shows the total time spent on studies (that is, the sum of time spent on self-study and attending lectures), and panel D shows time spent working. The vertical line marks the introduction of tuition fees for incumbent students in 2007.

Figure 6: Share of high school graduates enrolling at university

(A) Fees from 2006



(B) Fees from 2007



Notes: The figure shows the share of high school graduates enrolling at university within one year of high school graduation, split by by cohort and treatment group. Panel A compares high school graduates from states that did not introduce fees with those from states that introduced fees in 2006. Panel B compares high school graduates from states that did not introduce fees with those from states that introduced fees in 2007.

Table 1: Descriptive statistics for the intensive margin analysis

Variable	Mean	SD
Student characteristics		
Cohort	1999.69	2.84
Female	53.00	9.40
Degree type		
- *Diplom*	52.80	17.87
- *Magister*	16.57	10.76
- *Staatsexamen*	30.32	10.37
Treatment		
Initially enrolled at university that introduced fees	57.35	49.46
Treated (fees introduced within six years of enrollment)	24.56	43.05
Outcome		
Degree completion within six years	27.88	9.43

Notes: The table shows means and standard deviations of key variables for the 731,352 students included in the main estimation sample. Degree types do not sum to 100 percent because 0.29 percent of students study towards a degree other than the ones shown here.

Table 2: Effect of tuition fees on completion within six years

	Main	Robustness			
	(1)	(2)	(3)	(4)	(5)
Tuition fees					
× 1st cohort (≤ 1,000 EUR)	2.82***	2.59***	2.63***	2.13**	2.30**
	(0.87)	(0.78)	(0.89)	(0.94)	(0.88)
	[2001]	*[2001]*	*[2001]*	*[2001]*	*[2001]*
× 2nd cohort (≤ 2,000 EUR)	4.10***	3.90***	3.82***	3.21**	3.82***
	(1.13)	(0.87)	(1.19)	(1.47)	(1.32)
	[2002]	*[2002]*	*[2002]*	*[2002]*	*[2002]*
× 3rd cohort (≤ 3,000 EUR)	4.03***	3.63***	3.81***	2.93*	3.79**
	(1.34)	(1.04)	(1.45)	(1.78)	(1.72)
	[2003]	*[2003]*	*[2003]*	*[2003]*	*[2003]*
× 4th cohort (≤ 4,000 EUR)	5.92***	5.00***	5.79***	4.63***	5.31***
	(1.11)	(0.90)	(1.13)	(1.93)	(1.90)
	[2004]	*[2004]*	*[2004]*	*[2004]*	*[2004]*
Mean of dependent variable	27.88	27.88	27.88	27.88	27.88
Number of univ.-cohort cells	390	390	390	390	390
Number of students	731,352	731,352	731,352	731,352	731,352
University fixed effects	✓	✓	✓	✓	✓
Cohort fixed effects	✓	✓	✓	✓	✓
University controls		✓			✓
Regional controls			✓		✓
Group-specific trends				✓	✓

Notes: The table reports difference-in-differences estimates of the effect of tuition fees on degree completion within six years using data for cohorts that initially enrolled at university between 1995-2004. Below each coefficient estimate, the starting cohort which identifies the effect is reported in square brackets. University controls include the student gender composition and the share of students enrolled in different degree types. Regional controls include state-level GDP and unemployment. Group-specific trends include separate linear time trends for universities introducing general tuition fees. Standard errors in parentheses are clustered at the level of starting university. * p<0.1, ** p<0.05, *** p<0.01.

Table 3: Completion within different periods of time

	Completion within								
	3 years (1)	4 years (2)	5 years (3)	6 years (4)	7 years (5)	8 years (6)	9 years (7)	10 years (8)	11 years (9)
Tuition fees									
× 1st cohort (≤ 1,000 EUR)	0.05	0.65*	2.15***	2.82***	1.06	0.34	-0.53	-1.14	-0.16
	(0.06)	(0.36)	(0.75)	(0.87)	(1.10)	(1.38)	(1.15)	(1.11)	(1.40)
	[2004]	*[2003]*	*[2002]*	*[2001]*	*[2000]*	*[1999]*	*[1998]*	*[1997]*	*[1996]*
× 2nd cohort (≤ 2,000 EUR)	0.03	0.70**	2.43***	4.10***	2.88**	0.75	-0.24	-1.33	-1.28
	(0.05)	(0.34)	(0.93)	(1.13)	(1.24)	(1.44)	(1.61)	(1.38)	(1.67)
	[2005]	*[2004]*	*[2003]*	*[2002]*	*[2001]*	*[2000]*	*[1999]*	*[1998]*	*[1997]*
× 3rd cohort (≤ 3,000 EUR)		0.64	3.47***	4.03***	4.35***	2.45	0.55	-0.84	-1.48
		(0.42)	(0.84)	(1.34)	(1.48)	(1.57)	(1.64)	(1.86)	(1.82)
		[2005]	*[2004]*	*[2003]*	*[2002]*	*[2001]*	*[2000]*	*[1999]*	*[1998]*
× 4th cohort (≤ 4,000 EUR)			3.50***	5.92***	4.35***	4.12***	2.43	-0.01	-0.97
			(0.89)	(1.11)	(1.50)	(1.63)	(1.73)	(1.87)	(2.32)
			[2005]	*[2004]*	*[2003]*	*[2002]*	*[2001]*	*[2000]*	*[1999]*
Mean of dependent variable	0.13	1.68	10.90	27.88	42.45	50.32	54.23	56.45	57.76
Number of univ.-cohort cells	312	351	390	390	351	312	273	234	195
Number of students	594,894	662,993	730,841	731,352	656,882	575,221	497,134	416,149	340,136

Notes: The table reports estimates of the effect of tuition fees on degree completion within different time frames, indicated in the column headers. The treatment in each regression is an indicator for whether tuition fees were introduced at the university of initial enrollment within this time frame. Below each coefficient estimate, the starting cohort which identifies the effect is reported in square brackets. The coefficient estimates shaded in gray identify those starting cohorts which had reached their seventh or later year of studies in 2007; students in these cohorts would have had to pay long-term tuition fees of usually 500 EUR per semester even if general tuition fees had not been introduced. Standard errors in parentheses are clustered at the level of starting university. * p<0.1, ** p<0.05, *** p<0.01.

Table 4: Effects of tuition fees on students' time investment

	Time use (weekly hours)			
	self-study	lectures	total study time	work
	(1)	(2)	(3)	(4)
Panel A: without controls				
Tuition fees	1.769	1.431***	3.200**	–0.200
	(1.389)	(0.480)	(1.500)	(0.706)
	[2004/05]	*[2004/05]*	*[2004/05]*	*[2004/05]*
Panel B: with individual-level controls				
Tuition fees	1.695	1.523***	3.218**	–0.270
	(1.359)	(0.524)	(1.487)	(0.754)
	[2004/05]	*[2004/05]*	*[2004/05]*	*[2004/05]*
Panel C: interaction with parental education, with individual-level controls				
Tuition fees	1.619	1.964***	3.582***	–0.163
	(1.064)	(0.587)	(1.219)	(1.031)
× parents college	0.136	–0.784	–0.649	–0.191
	(1.091)	(0.610)	(0.723)	(1.267)
	[2004/05]	*[2004/05]*	*[2004/05]*	*[2004/05]*
Panels A + B + C:				
Mean of dependent var.	14.74	14.03	28.77	7.65
Number of students	2,011	2,011	2,011	2,011
University fixed effects	✓	✓	✓	✓
Cohort fixed effects	✓	✓	✓	✓

Notes: The table reports estimates of the effect of tuition fees on students' time use, based on the sample of Figure 5. Column 1 shows results for time spent on self-study, column 2 shows results for time spent attending lectures, column 3 shows results for total time spent studying (that is, the sum of time spent on self-study and attending lectures), and column 4 shows results for time spent working. All specifications include cohort and university fixed effects, and specifications in panels B and C additionally control for students' high school GPA, gender, age, degree type, and a dummy for whether any of their parents completed higher education. Panel C also includes an interaction between tuition fees and this parental-education dummy (61% of students in the sample have college-educated parents). The numbers in square brackets below the coefficient estimates indicate the starting cohorts identifying the effect of tuition fees. Standard errors in parentheses are clustered at the level of university. * p<0.1, ** p<0.05, *** p<0.01.

Table 5: Effect of tuition fee abolition on Bachelor's degree completion

	Main			Robustness			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Abolition	-2.76**	-2.26**	-2.26**	-2.70**	-2.29**	-2.91**	-3.18**
	(1.12)	(0.97)	(0.97)	(1.21)	(1.08)	(1.33)	(1.46)
	[*TWFE*]	[DID_M]	[DID_M]	[DID_M]	[DID_M]	[DID_M]	[DID_M]
Number of students	1,079,544	342,770	342,770	342,770	342,770	342,770	342,770
Number of switchers	285,689	88,108	88,108	88,108	88,108	88,108	88,108
University fixed effects	✓	✓	✓	✓	✓	✓	✓
Cohort fixed effects	✓	✓	✓	✓	✓	✓	✓
Fee Starter			✓				✓
University controls				✓			✓
State-level controls					✓		✓
Group-specific trends						✓	✓

Notes: The table reports DID_M estimates and a standard *TWFE* estimate of the effect of tuition fee abolition on bachelor students degree completion in three years, using data for cohorts that initially enrolled at university between 2007-2014. Number of students is the number of first differences of the outcome and of the treatment used in the estimation. Number of switchers is the number of switchers or first-time switchers the effect applies to. Fee starter includes a dummy signalling enrollment when fees where active. University controls include the student gender composition and the share of students enrolled in different degree types. Regional controls include state-level unemployment. Group-specific trends include separate linear time trends for universities that abolished general tuition fees between 2010 and 2014. Bootstrapped standard errors (1,000 replications) for DID_M estimates and adjusted for clustering at the university level for standard *TWFE* estimates in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 6: Effect of tuition fees on enrollment of high school graduates

	Main	Robustness			
	(1)	(2)	(3)	(4)	(5)
Tuition fees	-3.87***	-3.95***	-3.55***	-3.10***	-3.92***
	(1.08)	(1.07)	(0.99)	(0.97)	(1.02)
Mean of dependent variable	56.75	56.75	56.75	56.75	56.75
Number of state-cohort cells	150	150	150	150	150
Number of high school graduates	2,613,738	2,613,738	2,613,738	2,613,738	2,613,738
State fixed effects	✓	✓	✓	✓	✓
High school cohort fixed effects	✓	✓	✓	✓	✓
High school policy change control		✓			✓
State-level controls			✓		✓
Group-specific trends				✓	✓

Notes: The table shows estimates of the effect of tuition fees on high school graduates' enrollment at university. The sample spans the years from 2001-2010 and excludes the state of Hesse. HS policy change control is a dummy for a double cohort of high school leavers graduating; see text for details. State-level controls include GDP and unemployment. Group-specific trends include separate linear trends for states introducing general tuition fees in 2006 and 2007. Standard errors in parentheses are clustered at the level of state. * p<0.1, ** p<0.05, *** p<0.01.

Table 7: Overall impact of fees on the number of university graduates and the public cost of higher education

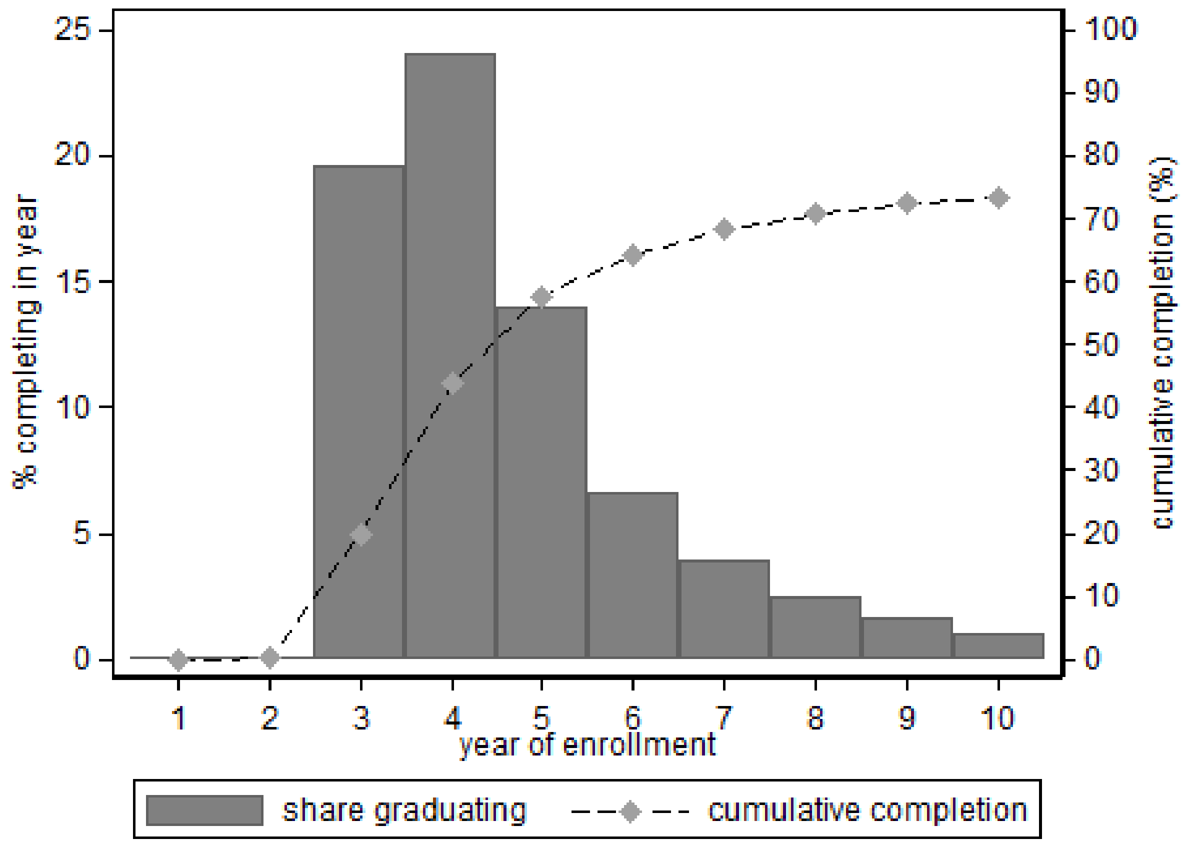
β^{ext}	β^{int}	$Graduates^{NoFees}$	$Graduates^{Fees}$	$Cost^{NoFees}$	$Cost^{Fees}$	$\Delta Graduates$	$\Delta Cost$
Panel A: graduation within six years ($r = 0.28$)							
-0.04	0.03	0.160	0.164	20810	19643	0.005	-1167
-0.04	0.04	0.160	0.170	20810	19741	0.010	-1069
-0.04	0.06	0.160	0.180	20810	19937	0.021	-873
Panel B: graduation within ten years ($r = 0.58$)							
-0.04	0.03	0.331	0.323	23967	22578	-0.007	-1388
-0.04	0.04	0.331	0.329	23967	22676	-0.002	-1291
-0.04	0.06	0.331	0.339	23967	22872	0.009	-1095

Notes: The table shows estimates of the overall (combined extensive and intensive margin) impact of tuition fees on the number of university graduates and the public cost of higher education. The columns to the left of the vertical line show the parameter values used in the accounting exercise, and the columns to the right of the vertical line show the resulting changes in the number of university graduates and the public cost of higher education. See Section 6 for details.

Appendix

Additional figures and tables

Figure A.1: Bachelor's degree completion at German universities



Notes: The figure shows the share of students in starting cohort 2007 completing their Bachelor's degree in each of the first ten years after initial enrollment (left axis), as well as the cumulative completion rate over these years (right axis).

Table A.1: Timing of the introduction and abolition of tuition fees in German states

	General tuition fees					Long-term fees		
	Introduction			Abolition				
		First term with fees for						
State	Legislation passed	newly-enrolled students	incumbent students	Legislation passed	First term w/o general fees	First term with fees	Fees first charged after	First term w/o long-term fees
Baden-Wuerttemberg	Dec 2005	Apr 2007	Apr 2007	Dec 2011	Apr 2012	Oct 1998	NSD + 2 years	Apr 2012
Bavaria	May 2006	Apr 2007	Apr 2007	Apr 2013	Oct 2013	Oct 2005	NSD + 2 years	Oct 2013
Berlin	-	-	-	-	-	-	-	-
Brandenburg	-	-	-	-	-	-	-	-
Bremen	-	-	-	-	-	Oct 2006	7 years	Apr 2020
Hamburg	Jun 2006	Apr 2007	Apr 2007	Sep 2011	Oct 2012	Apr 2004	NSD + 1 year	Oct 2012
Hesse	Oct 2006	Oct 2007	Oct 2007	Jun 2008	Oct 2008	Apr 2004	NSD + 2 years	Oct 2008
Mecklenburg-Vorpommern	-	-	-	-	-	-	-	-
Lower Saxony	Dec 2005	Oct 2006	Apr 2007	Dec 2013	Oct 2014	Apr 2003	NSD + 3 years	active
North Rhine-Westphalia	Mar 2006	Oct 2006	Apr 2007	Sep 2010	Oct 2011	Apr 2004	1.5 $\times$ NSD	Oct 2011
Rhineland-Palatinate	-	-	-	-	-	Oct 2004	1.75 $\times$ NSD	active
Saarland	Jul 2006	Oct 2007	Oct 2007	Dec 2009	Apr 2010	Oct 2003	NSD + 2 years	active
Saxony	-	-	-	-	-	Oct 2012	NSD + 2 years	active
Saxony-Anhalt	-	-	-	-	-	Oct 2004	NSD + 2.5 years	Oct 2020
Schleswig-Holstein	-	-	-	-	-	-	-	-
Thuringia	-	-	-	-	-	Oct 2005	NSD + 2 years	active

Notes: The table shows the dates at which different German states introduced and abolished general tuition fees and regulations concerning long-term tuition fees. Term dates in April denote the start of summer term, and term dates in October denote the start of winter term. NSD denotes the nominal study duration, that is, the number of years that a degree program is supposed to take to complete. States where long-term fess are still active (as of winter term 2021) are indicated as such.

Table A.2: Universities in final estimation samples

	N_I	N_A		N_I	N_A
Baden-Württemberg (Treated)					
Freiburg, U	-	13,876	Lüneburg	-	9,658
Freiburg, PH	-	2,134	Oldenburg	-	13,077
Heidelberg, U	-	13,961	Osnabrück	-	12,949
Heidelberg, PH	-	728	Vechta	-	6,690
Hohenheim	-	11,093			
Karlsruhe, TU	-	23,420	**Mecklenburg-Vorpommern** (Control)		
Karlsruhe, PH	-	922	Greifswald	-	5,220
Koblenz-Landau	-	11,370	Rostock	-	10,601
Konstanz	-	10,143			
Ludwigsburg, PH	-	1,658	**North-Rhine Westphalia** (Treated)		
Mannheim	-	14,084	Aachen	28,548	33,948
Schwäbisch Gmünd, PH	-	1,105	Bielefeld	16,865	16,345
Stuttgart	-	22,560	Bochum	-	28,300
Tübingen	-	15,055	Bonn	30,471	21,478
Ulm	-	7,712	Dortmund	24,839	30,894
Weingarten, PH	-	1,328	Duisburg - Essen	35,285	30,946
			Düsseldorf	17,677	19,599
Bavaria (Treated)			Köln, DSHS	3,753	3,549
Augsburg	17,750	12,469	Köln, U	43,486	29,394
Bamberg	10,935	8,715	Münster	38,772	27,515
Bayreuth	12,046	9,950	Paderborn	18,307	29,767
Eichstätt-Ingolstadt	5,958	4,532	Siegen	12,705	16,500
Erlangen-Nürnberg	28,851	28,601	Wuppertal	13,826	19,177
München, TU	-	31,890			
München, U	-	24,936	**Rhineland-Palatinate** (Control)		
Passau	10,869	7,992	Mainz	24,623	20,393
Regensburg	21,869	12,159	Kaiserslautern	8,732	8,514
Würzburg	20,771	13,866	Trier	14,201	13,353
Berlin (Control)			**Saarland** (Treated)		
Berlin, FU	25,467	20,240	Saarbrücken	-	9,777
Berlin, HU	26,328	18,220			
Berlin, TU	26,551	21,444	**Saxony** (Control)		
			Chemnitz	11,874	10,814
Brandenburg (Control)			Dresden	41,256	18,234
Cottbus	7,102	8,869	Freiberg	5,415	4,473
Frankfurt (Oder)	4,788	4,116	Leipzig	35,936	17,003
Potsdam	16,758	17,398			
			Saxony-Anhalt (Control)		
Bremen (Control)			Halle	20,820	13,535
Bremen	-	18,789	Magdeburg	13,320	13,819
Hamburg (Treated)			**Schleswig-Holstein** (Control)		
Hamburg, TU	5,855	7,950	Lübeck	2,234	2,473
Hamburg, U	-	29,854	Flensburg	4,731	5,237
			Kiel	21,778	20,383
Lower Saxony (Treated)					
Braunschweig	-	15,913	**Thuringia** (Control)		
Clausthal	-	2,765	Erfurt	-	8,047
Hannover	-	23,585	Ilmenau	-	7,505
Hildesheim	-	7,765	Jena	-	13,587
Göttingen	-	20,393	Weimar	-	3,230
Total:				731,352	1,079,544

Notes: The table lists the universities included in the final estimation samples of the empirical analyses of introduction N_I and abolition N_A of tuition fees, cohorts 1995-2004 and 2007-2014, respectively. The sampling frame consists of all public universities in Germany, including free-standing schools of education (PH), but excluding the two universities of the military and the University of Hagen, which focuses on distance teaching. The final estimation sample for introduction effects excludes universities with high fractions of missing information on the university and year of initial enrollment in the Final Examinations Register, see Section 3.2 for details.

Table A.3: Effect heterogeneity

	Gender		Income of HS county		Subject			
	Males (1)	Females (2)	< median (3)	> median (4)	Humanities (5)	Social Sciences (6)	STEM (7)	Other (8)
Tuition fees								
× 1st cohort (≤ 1,000 EUR)	2.43**	2.91***	1.01	3.29***	2.68**	4.75***	1.84	1.31
	(0.95)	(0.99)	(1.10)	(0.90)	(1.26)	(1.61)	(1.55)	(3.81)
	[2001]	*[2001]*	*[2001]*	*[2001]*	*[2001]*	*[2001]*	*[2001]*	*[2001]*
× 2nd cohort (≤ 2,000 EUR)	4.80***	3.56***	3.48**	4.17***	3.68**	7.57***	2.46	-0.03
	(1.33)	(1.07)	(1.49)	(1.17)	(1.45)	(2.18)	(2.09)	(2.97)
	[2002]	*[2002]*	*[2002]*	*[2002]*	*[2002]*	*[2002]*	*[2002]*	*[2001]*
× 3rd cohort (≤ 3,000 EUR)	4.70***	3.44**	4.41***	3.72***	2.03	11.59***	1.82	-2.62
	(1.30)	(1.51)	(1.52)	(1.38)	(2.24)	(2.82)	(2.36)	(4.62)
	[2003]	*[2003]*	*[2003]*	*[2003]*	*[2003]*	*[2003]*	*[2003]*	*[2001]*
× 4th cohort (≤ 4,000 EUR)	5.55***	5.61***	7.02***	5.52***	4.61**	11.07***	5.40**	-1.86
	(0.99)	(1.39)	(1.34)	(1.24)	(1.95)	(2.89)	(2.19)	(3.58)
	[2004]	*[2004]*	*[2004]*	*[2004]*	*[2004]*	*[2004]*	*[2004]*	*[2001]*
Mean of dependent variable	24.26	31.09	32.67	25.03	29.05	30.27	24.29	32.88
Number of univ.-cohort cells	390	390	390	390	335	363	370	341
Number of students	343,742	387,588	269,759	459,929	195,824	216,244	274,864	44,278

Notes: The table reports estimates from regressions in which the sample is divided by gender (columns 1 and 2), an indicator for above- vs below-median GDP of the county in which the student graduated from high school (columns 3 and 4), and broad subject groups (columns 5-8). The total number of observations in each sample split is slightly lower than in the overall sample due to missing information. Below each coefficient estimate, the starting cohort which identifies the effect is reported in square brackets. Standard errors in parentheses are clustered at the level of starting university. * p<0.1, ** p<0.05, *** p<0.01.

Table A.4: Effects on final grade and university switching

	Final grade		At different university	
	Overall grade	Has top grade	Enrolled after 6 years	Completion w/in 6 years
	(1)	(2)	(3)	(4)
Tuition fees				
× 1st cohort (≤ 1,000 EUR)	0.03	0.01	0.61	0.16
	(0.04)	(0.02)	(0.46)	(0.26)
	[2001]	*[2001]*	*[2001]*	*[2001]*
× 2nd cohort (≤ 2,000 EUR)	0.04	0.01	0.82*	0.09
	(0.04)	(0.02)	(0.48)	(0.28)
	[2002]	*[2002]*	*[2002]*	*[2002]*
× 3rd cohort (≤ 3,000 EUR)	0.03	0.01	0.66	0.12
	(0.04)	(0.02)	(0.81)	(0.27)
	[2003]	*[2003]*	*[2003]*	*[2003]*
× 4th cohort (≤ 4,000 EUR)	0.08**	-0.01	1.70*	0.65**
	(0.04)	(0.02)	(0.91)	(0.33)
	[2004]	*[2004]*	*[2004]*	*[2004]*
Mean of dependent variable	1.93	0.27	12.27	3.61
Number of univ.-cohort cells	390	390	390	390
Number of students	201,048	201,048	731,352	731,352

Notes: The table displays the effect of tuition fees on final grades and university switching. In columns 1 and 2, the sample consists of all students who initially enrolled at one of the 39 universities in our main analysis sample between 1995 and 2004 and who successfully graduated within six years. The outcome in column 1 is the final grade point average, which ranges from 1 (very good) to 4 (sufficient). The outcome in column 2 is an indicator for achieving the top grade (very good). The outcome in column 3 is an indicator for whether the student is enrolled at a university other than the starting university after 6 years. The outcome in column 4 is an indicator for whether the student graduated from a university other than the the starting university within six years. The regressions in columns 3 and 4 are based on our main analysis sample. Below each coefficient estimate, the starting cohort which identifies the effect is reported in square brackets. Standard errors in parentheses are clustered at the level of university. * p<0.1, ** p<0.05, *** p<0.01.

Table A.5: Further robustness checks

		Long-term fees		Other policies		Sample restrictions		
	Main (1)	at enrollment (2)	first six years (3)	BA intro (4)	w/o excel. initiat. (5)	with Hesse (6)	w/o imputed (7)	incl. BA (8)
Tuition fees								
× 1st cohort (≤ 1,000 EUR)	2.82***	2.82***	2.84***	2.93***	2.60***	2.59***	2.98***	2.86***
	(0.87)	(0.87)	(0.87)	(0.79)	(0.88)	(0.80)	(0.89)	(0.95)
	[2001]	*[2001]*	*[2001]*	*[2001]*	*[2001]*	*[2001]*	*[2001]*	*[2001]*
× 2nd cohort (≤ 2,000 EUR)	4.10***	4.10***	4.13***	3.68***	4.00***	3.43***	4.55***	5.06***
	(1.13)	(1.13)	(1.13)	(1.01)	(1.15)	(1.04)	(1.12)	(1.40)
	[2002]	*[2002]*	*[2002]*	*[2002]*	*[2002]*	*[2002]*	*[2002]*	*[2002]*
× 3rd cohort (≤ 3,000 EUR)	4.03***	4.03***	4.05***	3.43***	4.17***	3.46***	4.38***	4.78***
	(1.34)	(1.34)	(1.34)	(1.17)	(1.36)	(1.19)	(1.34)	(1.68)
	[2003]	*[2003]*	*[2003]*	*[2003]*	*[2003]*	*[2003]*	*[2003]*	*[2003]*
× 4th cohort (≤ 4,000 EUR)	5.92***	6.04***	5.95***	5.39***	6.35***	5.73***	6.20***	4.91***
	(1.11)	(0.99)	(1.11)	(0.87)	(1.03)	(1.03)	(1.15)	(1.72)
	[2004]	*[2004]*	*[2004]*	*[2004]*	*[2004]*	*[2004]*	*[2004]*	*[2004]*
Number of univ.-cohort cells	390	390	390	390	380	430	350	390
Number of students	731,352	731,352	731,352	731,352	705,885	820,858	696,754	758,748

Notes: The table reports estimates from regressions that probe the robustness of the headline results; see text for details. Below each coefficient estimate, the starting cohort which identifies the effect is reported in square brackets. Standard errors in parentheses are clustered at the level of starting university. * p<0.1, ** p<0.05, *** p<0.01.

Table A.6: Alternative ways of statistical inference

	Cluster			Wild cluster bootstrap					
				Rademacher		Mammen		Webb	
	Uni	Uni, Year	State	H_0	H_1	H_0	H_1	H_0	H_1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Tuition fees									
× 1st cohort (≤ 1,000 EUR)	2.82***	2.82***	2.82***	2.82**	2.82***	2.82	2.82***	2.82**	2.82***
p-value	0.002	0.002	0.002	0.023	0.000	0.106	0.000	0.027	0.002
	[2001]	*[2001]*	*[2001]*	*[2001]*	*[2001]*	*[2001]*	*[2001]*	*[2001]*	*[2001]*
× 2nd cohort (≤ 2,000 EUR)	4.10***	4.10***	4.10***	4.10**	4.10***	4.10*	4.10***	4.10***	4.10***
p-value	0.001	0.001	0.000	0.016	0.000	0.052	0.000	0.004	0.000
	[2002]	*[2002]*	*[2002]*	*[2002]*	*[2002]*	*[2002]*	*[2002]*	*[2002]*	*[2002]*
× 3rd cohort (≤ 3,000 EUR)	4.03***	4.03***	4.03***	4.03**	4.03**	4.03*	4.03***	4.03**	4.03**
p-value	0.005	0.005	0.004	0.027	0.016	0.071	0.000	0.026	0.012
	[2003]	*[2003]*	*[2003]*	*[2003]*	*[2003]*	*[2003]*	*[2003]*	*[2003]*	*[2003]*
× 4th cohort (≤ 4,000 EUR)	5.92***	5.92***	5.92***	5.92***	5.92***	5.92***	5.92***	5.92***	5.92***
p-value	0.000	0.000	0.000	0.004	0.000	0.000	0.000	0.001	0.000
	[2004]	*[2004]*	*[2004]*	*[2004]*	*[2004]*	*[2004]*	*[2004]*	*[2004]*	*[2004]*

Notes: The table displays p-values based on alternative procedures for inference for the main specification. Columns (1)-(3) implement clustered standard errors, where the level of clustering is the starting university (default) in column (1), the university and the starting cohort (two-way clustering) in column (2), and the state in column (3). The p-values in columns (4)-(9) are based on wild cluster bootstrap procedures with states as clusters, where columns (4) and (5) apply Rademacher weights, while columns (6) and (7) use Mammen weights and columns (8) and (9) use Webb weights. Testing is under the null hypothesis in columns (4), (6), and (8) and under the alternative hypothesis in columns (5), (7), and (9). All estimations are performed using the user-written Stata-program BOOTTEST (Roodman et al., 2019). Below each coefficient estimate, the starting cohort which identifies the effect is reported in square brackets. * p<0.1, ** p<0.05, *** p<0.01.

Table A.7: Completion within different periods of time, sample period extended until 2012

	Completion within								
	3 years (1)	4 years (2)	5 years (3)	6 years (4)	7 years (5)	8 years (6)	9 years (7)	10 years (8)	11 years (9)
Tuition fees									
× 1st cohort (≤ 1,000 EUR)	0.05	0.65*	2.15***	2.81***	1.05	0.38	-0.49	-1.11	-0.15
	(0.06)	(0.36)	(0.75)	(0.87)	(1.11)	(1.39)	(1.16)	(1.10)	(1.40)
	[2004]	*[2003]*	*[2002]*	*[2001]*	*[2000]*	*[1999]*	*[1998]*	*[1997]*	*[1996]*
× 2nd cohort (≤ 2,000 EUR)	0.03	0.70**	2.43***	4.11***	2.86**	0.80	-0.17	-1.29	-1.23
	(0.05)	(0.34)	(0.93)	(1.13)	(1.25)	(1.45)	(1.61)	(1.37)	(1.66)
	[2005]	*[2004]*	*[2003]*	*[2002]*	*[2001]*	*[2000]*	*[1999]*	*[1998]*	*[1997]*
× 3rd cohort (≤ 3,000 EUR)		0.64	3.47***	4.03***	4.34***	2.49	0.63	-0.78	-1.44
		(0.42)	(0.84)	(1.34)	(1.48)	(1.58)	(1.64)	(1.86)	(1.82)
		[2005]	*[2004]*	*[2003]*	*[2002]*	*[2001]*	*[2000]*	*[1999]*	*[1998]*
× 4th cohort (≤ 4,000 EUR)			3.50***	5.92***	4.36***	4.14***	2.51	0.06	-0.93
			(0.89)	(1.12)	(1.51)	(1.64)	(1.73)	(1.86)	(2.33)
			[2005]	*[2004]*	*[2003]*	*[2002]*	*[2001]*	*[2000]*	*[1999]*
× 5th cohort (≤ 5,000 EUR)[a]				5.55***	6.72***	4.39***	4.00**	1.85	0.18
				(1.34)	(1.50)	(1.63)	(1.75)	(1.90)	(2.29)
				[2005]	*[2004]*	*[2003]*	*[2002]*	*[2001]*	*[2000]*
× 6th cohort (≤ 6,000 EUR)[a]					6.80***	7.18***	4.23**	3.66*	1.99
					(1.93)	(1.70)	(1.86)	(1.91)	(2.28)
					[2005]	*[2004]*	*[2003]*	*[2002]*	*[2001]*
Number of univ.-cohort cells	312	351	390	429	429	390	351	312	273
Number of students	594,894	662,993	730,841	795,987	795,987	731,352	656,882	575,221	497,134

Notes: The table reports estimates of the effect of tuition fees on degree completion within different time frames, indicated in the column headers. The regressions follow the ones in Table 3, with the difference that the sample period is extended until 2012 and that as a consequence we can estimate effects for two additional cohorts. The coefficient estimates shaded in gray identify those starting cohorts which had reached their seventh or later year of studies in 2007; students in these cohorts would have had to pay long-term tuition fees of usually 500 EUR per semester even if general tuition fees had not been introduced. Standard errors in parentheses are clustered at the level of starting university. * p<0.1, ** p<0.05, *** p<0.01.

[a] These estimates could be affected by the abolishment of fees after 2010, see text for details.

Table A.8: Effects on employment and hours worked

	Employed (1)	Hours worked (2)
Tuition fees		
× 1st cohort (≤ 1,000 EUR)	-0.015	-0.256
	(0.028)	(0.361)
	[2001]	*[2001]*
× 2nd cohort (≤ 2,000 EUR)	0.007	0.096
	(0.020)	(0.399)
	[2002]	*[2002]*
× 3rd cohort (≤ 3,000 EUR)	-0.016	-0.103
	(0.016)	(0.309)
	[2003]	*[2003]*
× 4th cohort (≤ 4,000 EUR)	-0.038*	-0.513
	(0.020)	(0.355)
	[2004]	*[2004]*
Mean of dependent variable	0.292	3.952
Number of students	36,747	36,747

Notes: The table shows estimates of the effect of tuition fees on students' labor supply. In order to closely match our main analysis sample, the regressions in this table are based on Microcensus waves 2001-2010 and include university students who are between 20 and 25 years of age, live in the states that are included in our main sample, and do not have a prior higher education degree. All regressions include year and state fixed effects. Standard errors are clustered at level of state. * p<0.1, ** p<0.05, *** p<0.01.

For Online Publication

Dealing with missing information on university and year of initial enrollment in the Final Examinations Register

As noted in Section 3.2, information about the university and year of initial enrollment is missing for about 15 percent of observations in the Final Examinations Register due to data loss generated by a failed software update in some universities' administrative computer systems. In our main analysis, we address this issue by dropping from our sample all universities with more than 20 percent of missing cases in any year, or with more than ten percent of missing cases across all years. In this Appendix, we examine the nature of this problem in more detail and we demonstrate the robustness of our results to using alternative ways of dealing with the missing information.

We start by examining the relationship between tuition fees and the occurrence of missing information in the Final Examinations Register. This is critical because any systematic relationship could bias our results. For example, if universities which charge tuition fees had lower rates of missing information, we would likely overestimate the impact of fees on degree completion. The reason is that in calculating our outcome variable, we cannot match completed degrees with missing information to the corresponding starting-university-by-cohort cells and thus undercount the number of degrees obtained in these cells.

Table O.1 shows results from regressions in which the dependent variable is an indicator which takes value 1 if the information on university and year of initial enrollment is missing and 0 otherwise. The main independent variables are indicators for whether the university at which the the final exam was taken (the "graduation university") charged tuition fees in the years 2007 to 2010, and the specifications further include examination-year and graduation-university fixed effects. Note that in contrast to our main analysis, these difference-in-differences regressions are run using individual-level data on graduating students and rely exclusively on the Final Examinations Register. Column 1 shows that in the unrestricted sample which includes all universities, there is a significant imbalance in the years from 2008 to 2010, with a *higher* rate of missing information among students at fee-charging universities. Column 2 shows that this imbalance largely disappears in our main sample: once we exclude universities with a high incidence of missing information, the coefficients on the fee indicators are all close to zero and mostly statistically insignificant. This suggests that our headline results are not driven by any systematic occurrence of missing information in the outcome variable.

We next show that our estimates are robust to using alternative cut-offs for the percentage of observations with missing information when dropping universities from the sample. The results of this exercise are shown in Table O.2. Column 1 repeats our headline results. Column 2 shows smaller estimated effects for the unrestricted sample that includes all universities. The reduced effect in this regression is in line with the finding in column 1 of Table O.1 that students at treated universities have higher rates of missing information in this sample, which likely biases the coefficients downwards. Columns 3-5 show results from samples which exclude universities with more than 25 percent, 15 percent, and ten percent of observations with missing information in a single year, and column 6 shows results from a sample that excludes universities in which this problem affects more than five percent of observations across all years. The results based on these reasonable alternatives to our main sample restriction are qualitatively and quantitatively similar to our headline results.

Finally, as an alternative to restricting the sample to universities with lower incidences of missing information as in Table O.2, we impute the missing data directly. In particular, we impute the university of initial enrollment as the university at which the final examination was taken (that is, we assume that individuals did not switch university). We further calculate the year of initial enrollment as the graduation year minus the number of semesters the student had been enrolled, a measure that is available for almost every observation in the Final Examinations Register.[28] We then construct our main outcome as in Equation 1. Table O.3 presents results from regressions which use this imputed outcome variable. Column 1 repeats our headline estimates for comparison. Column 2 uses our main sample, but imputes missing values in the dependent variable using

[28] Note that this imputation can be incorrect, for example, if the student took a break during her studies, in which case the imputed year of initial enrollment is later than the actual one.

the procedure described above. The estimates are qualitatively and quantitatively similar to those in column 1. Column 3 focuses on the wider sample of all universities, including those with high fractions of missing information, and again imputes outcomes using the procedure outlined above. Also in this population-level data, we find that tuition fees increase completion within six years, although the effects do not differ as much by the amount of fees paid. These results suggest that our headline estimates are not driven by the measurement problem due to missing information on university and year of initial enrollment in the Final Examinations Register.

Table O.1: Impact of tuition fees on the incidence of missing information

	All universities (1)	Main sample (2)
Tuition fees		
× graduation in 2007	-0.04	0.02**
	(0.06)	(0.01)
× graduation in 2008	0.09***	0.00
	(0.03)	(0.01)
× graduation in 2009	0.10**	0.01
	(0.04)	(0.01)
× graduation in 2010	0.15***	-0.00
	(0.04)	(0.01)
Number of students	1,078,398	532,101

Notes: The table shows estimates from regressions of an indicator for missing information on university and year of initial enrollment on dummies for whether the graduation university charged tuition fees in the years 2007-2010. All regressions are based on student-level data from the Final Examinations Register and include fixed effects for graduation university and year. The sample includes data from the graduation years 2001-2010. Column 1 reports results for the unrestricted sample including all universities, and column 2 reports results for our main estimation sample. Standard errors in parentheses are clustered at the level of graduation university. * p<0.1, ** p<0.05, *** p<0.01.

Table O.2: Robustness to using alternative sample restrictions

			Sample restriction regarding missing information			
	Main sample	All universities	max. 25%	max. 15%	max. 10%	mean <5%
	(1)	(2)	(3)	(4)	(5)	(6)
Tuiton fees						
× 1st cohort (≤ 1,000 EUR)	2.82***	2.25**	2.97***	2.56***	2.39*	2.60**
	(0.87)	(0.97)	(0.85)	(0.88)	(1.24)	(1.08)
	[2001]	*[2001]*	*[2001]*	*[2001]*	*[2001]*	*[2001]*
× 2nd cohort (≤ 2,000 EUR)	4.10***	2.38**	3.98***	3.29***	2.82*	3.22**
	(1.13)	(0.99)	(1.07)	(1.10)	(1.60)	(1.42)
	[2002]	*[2002]*	*[2002]*	*[2002]*	*[2002]*	*[2002]*
× 3rd cohort (≤ 3,000 EUR)	4.03***	1.37	3.84***	3.30**	3.28*	3.56**
	(1.34)	(1.18)	(1.28)	(1.35)	(1.93)	(1.68)
	[2003]	*[2003]*	*[2003]*	*[2003]*	*[2003]*	*[2003]*
× 4th cohort (≤ 4,000 EUR)	5.92***	-0.73	5.66***	5.68***	5.60***	5.56***
	(1.11)	(1.33)	(1.10)	(1.02)	(1.35)	(1.20)
	[2004]	*[2004]*	*[2004]*	*[2004]*	*[2004]*	*[2004]*
Number of univ.-cohort cells	390	780	400	350	280	300
Number of students	731,352	1,272,465	765,105	654,102	456,228	500,855

Notes: The table reports estimates of the effect of tuition fees on degree completion within six years using different subsamples. Sample restrictions are indicated in the column headers and described in detail in the text. Below each coefficient estimate, the starting cohort which identifies the effect is reported in square brackets. Standard errors in parentheses are clustered at the level of starting university. * p<0.1, ** p<0.05, *** p<0.01.

Table O.3: Robustness to imputation of missing values

		With imputed outcomes	
	Main	Main sample	All universities
	(1)	(2)	(3)
Tuition fees			
× 1st cohort (≤ 1,000 EUR)	2.82***	3.19***	3.18***
	(0.87)	(0.89)	(0.75)
	[2001]	*[2001]*	*[2001]*
× 2nd cohort (≤ 2,000 EUR)	4.10***	5.04***	3.90***
	(1.13)	(1.19)	(0.92)
	[2002]	*[2002]*	*[2002]*
× 3rd cohort (≤ 3,000 EUR)	4.03***	4.73***	3.02***
	(1.34)	(1.48)	(1.08)
	[2003]	*[2003]*	*[2003]*
× 4th cohort (≤ 4,000 EUR)	5.92***	6.04***	3.75***
	(1.11)	(1.21)	(1.24)
	[2004]	*[2004]*	*[2004]*
Number of univ.-cohort cells	390	390	780
Number of students	731,352	731,352	1,272,465

Notes: The table reports estimates from samples in which missing information on university and year of initial enrollment is imputed using available information on university of graduation and the number of semesters a student had been enrolled at the time of graduation. See text for further details on this imputation procedure. Column 1 reproduces the results from column 1 of Table 2. Column 2 applies the imputation procedure to the main estimation sample. Column 3 includes all universities in the sample, and again applies the imputation procedure. Below each coefficient estimate, the starting cohort which identifies the effect is reported in square brackets. Standard errors in parentheses are clustered at the level of starting university. * p<0.1, ** p<0.05, *** p<0.01.

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