Tuition Fees and Student Achievement - Evidence from a Differential Raise in Fees

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Abstract

This study analyses the effect of an increase in college costs on student achievement, particularly time-to-degree and performance. I exploit a unique policy at a Swiss university to identify and estimate the causal effect of an increase in tuition. Students faced an unexpected raise in tuition. This raise varied substantially across different students. The fees were increased by 81.7% for international students and by 20.2% for Swiss students. This variation allows me to follow a difference-in-differences strategy. I formally discuss identification with multiple treatments. I find at best modest effects of the increase on student achievement. Results suggest small positive anticipation effects on the probability to graduate and the credit accumulation for students at the end of their studies. These increased effort levels do not affect the grade average of the students. After the raise, the effects on the probability to graduate and the credit accumulation disappear. There is weak evidence of negative effects on credit accumulation and grades for students further away from graduation.

JEL Codes: I20, I22, I23, C31

1 Introduction

Tuition fees and student aid are frequently used policy instrument regarding the costs of education. Among OECD countries, the highest average tuition for public institutions is charged in United States. In Europe these levels are generally much lower, but vary from no fees in the Scandinavian countries to rather high fees in the United Kingdom. Similarly, the shares of students benefiting from public student aid vary substantially (OECD, 2012).[1] Recurringly, these policy instruments are subject to extensive reforms.[2] In general, the preceding political discussion are quite controversial. Besides public finances, one major topic of the discussion is the impact of college costs on student enrollment and achievements.

The theory of human capital investments predicts clear consequences on the effects of college costs on enrollment. Students decide to enter post secondary education if accumulated utility exceeds accumulated costs (Becker, 1964). If costs for higher education increase, the decision to enroll becomes less attractive. Consequently, enrollment goes down. Several credible quasi-experimental studies confirm these predictions.[3]

In principle, the same reasoning applies also to effort levels of students and consequently for student achievement. A student stays as long in higher education as the utility from an additional semester is bigger than the costs. An increase in college costs should work as an incentive to work harder and avoid additional costs. However, the duration of studies most likely enters the utility. If the utility is reduced by studying faster, the effect of an increase in tuition fees might be attenuated. In addition, students might be credit constrained, i.e. they cannot tab future earnings to pay for tuition. In that case, students might be forced to allocate time from school work to market work. Students might fare worse, be more likely to drop out, and stay longer in the university. In conclusion, the effect of college costs on student achievement needs to be examined empirically.

In this study, I analyze the effect of an increase in tuition fees on two dimensions of student achievement: first, the time students require to finish the degree - time-to-degree - and second, the performance, i.e. grades. Research on the effects of college costs on students achievement has mainly focused on student aid. Hardly any study analyses specifically the effect of tuition fees. Furthermore, even among studies using variation

[1] While in countries such as Sweden, Netherlands, and Denmark almost all students receive some form of aid, shares of students receiving aid are below 20% in Switzerland and Belgium (OECD, 2012).

[2] For instance, UK government raised the tuition cap from around US$ 5100 to US$ 14000 in 2012. In Germany, several subnational governments introduced tuition fees after 2005. By now, most of these states have already retracted the decision and abolished the tuition fees. In Switzerland, several universities have increased tuition recently or plan to do so in future.

[3] See for example Van Der Klaauw (2002), Dynarski (2002), Dynarski (2003), Kane (2003), Long (2004),Chin and Juhn (2010), Abraham and Clark (2006) and Cornwell, Mustard, and Sridhar (2006).

in student aid, several dimensions of student achievement remain understudied. While research suggests a positive impact of students aid on completion (Bruce and Carruthers, 2011; Dynarski, 2003, 2008; Sjoquist and Winters, 2012a,b), evidence on time-to-degree and performance is limited and ambiguous.[4]

The major challenge to this literature is that students self-select into universities with different costs and into student aid. Hence, students that face different costs most likely differ in observable characteristics such as socioeconomic status, and unobservable characteristics such as ability and ambition. A simple comparison of these students would not yield the true college cost effect, even after controlling for observed attributes. For instance, early studies analyze financial support as determinant of the Ph.D. duration. Doctoral students with working requirements need more time to graduate than students with non-binding scholarships (Bowen and Rudenstine, 1992; Ehrenberg and Mavros, 1995; Siegfried and Stock, 2001; Stock and Siegfried, 2006).[5] These studies have in common that the merit- or need-based selection into the different types of funding is not controlled for.

Some of the quasi-experimental evidence on student achievement is provided by studies evaluating state student aid reforms in the US. See Cornwell, Lee, and Mustard (2005), Georgia's HOPE program, Scott-Clayton (2011) for West Virginia's PROMISE program, and Castleman (2012) for the Bright Future Scholarship (BFS) in Florida. These studies face two different problems. First, most of these student aid schemes impose performance goals. In that case, these studies do not succeed to untangle conclusively the college cost and the retention effect. For instance, Scott-Clayton (2011) and Castleman (2012) find that aid recipients complete a higher course load and graduate earlier than non-participating students. Cornwell, Lee, and Mustard (2005) suggest the opposite. The authors contribute this discrepancy to the eligibility criteria. While in the HOPE program eligibility is tied merely to the GPA, PROMISE and BFS require a minimum GPA and course load. Second, these reforms might change the composition of the students, i.e. students that would have studied outside of the state or at private universities might consider to enroll at a state institution. If these students differ from those that studied in the state before the aid was implemented, the estimates capture both the college cost and the composition effect (Castleman, 2012).

Finally, the study that is most related to mine is Garibaldi, Giavazzi, Ichino, and Rettore (2011). They are the first to provide credible evidence of a causal effect of

[4]Related to this literature is a series of field experiments. They suggest that cash incentives improve students achievements but only for female or especially gifted students (Angrist, Lang, and Oreopoulos, 2009; Leuven, Oosterbeek, and van der Klaauw, 2010; Paola, Scoppa, and Nisticó, 2012).

[5]In a similar study, Glocker (2011) does not find an effect of the German public student aid (BAfoeG) on time-to-degree of under graduate students.

tuition on time-to-degree, exploiting discontinous tuition fees at the University of Bocconi, Italy. They find that an increase in the tuition of EUR 1000 after the standard period of studies reduces the probability of late graduation by 5.2%-points without affecting grades. However, given that the university and the students have the possibility to influence the level of tuition fees, this effect cannot be generalized easily. "The effects would be different in a university that applies a hard rule, in the sense that assigned tuition must always be followed" (Garibaldi, Giavazzi, Ichino, and Rettore, 2011, P. 710).

I address the selection problem by exploiting a unique policy at a Swiss university. In the Spring Semester 2012, students faced an raise in tuition fees that effected also students that had been already enrolled. Substantial differences in the increase allow me to use a differences-in-difference (DiD) strategy. The increase differed according to the student's nationality; for foreigners tuition increased by 81.7% and 20.2% for Swiss students. Hence, this study provides further quasi-experimental evidence of an impact of tuition fees on students achievement.

The results suggest that students hardly react to the increase in fees. There seems to be a positive anticipation effects on the probability to graduate and the credit accumulation for students at the end of their studies. However, part of this this effect can be attributed to a reform of the admission criteria at the master level. These elevated effort levels do not affect the students' grades. After the increase is implemented there is no significant effect on student achievement. Furthermore, there is weak evidence of negative effects on credit accumulation and grades for students further away from graduation.

Moreover, this paper provides a formal discussion of identification with DiD in a setting with multiple treatments when the non-treatment state is not observed in the post-treatment period.[6] To the best of my knowledge, this particular case has been widely neglected.[7] In this application, all students experience a raise in tuition fees. Hence, the effect of the high versus the low increase can only be identified if the foreigners would react to the low raise in the same way as the Swiss. That is, the effect of the low increase needs to be homogeneous. If this is not fulfilled, one can interpret the DiD estimate as a lower bound in absolute values of the effect of the high increase compared to no increase under weaker assumptions. These assumptions are satisfied if Swiss students react to the low increase in the same direction as, but less than foreign students react to the high increase.

The remainder of this study is structured as follows. First, the reform and the institutional background at the University of St.Gallen are introduced. Second, empirical

[6]See Frölich (2004) for a discussion of multiple treatments in DiD when the non-treatment state is observed.

[7]For instance, in the literature on child care it is common to consider differential increases in child care facilities as treatment. See for example Havnes and Mogstad (2012).

strategy and data are discussed. Third, main results are presented. Fourth, related student behavior and alternative explanations are explored. The last part concludes.

2 Institutional Setting

The University of St. Gallen covers the fields of Business Administration, Economics, Law and International Affairs. It is one of 12 public universities in Switzerland. In the fall semester 2012, more than 7300 students were enrolled at the University of St. Gallen. This number has been constantly growing over the past decade.

The standard period of studies is three years for a bachelor's degree. In turn, the bachelor's degree is divided into the assessment phase and the bachelor phase. The assessment phase is very strictly organized, all students have the same curriculum which in general takes one year. If the students do not pass this year, they have the possibility to try the entire assessment curriculum one more time. Drop-out is particularly high during this phase. After the assessment phase is passed, students decide on their major and can organize their curriculum with more flexibility. Thus, variation in study pace for bachelor students can only be observed from the second year onwards. Students have to earn at least 120 credits to receive the bachelor's degree, i.e. 30 credits per semester.[8] In the following, I refer always to bachelor semester. For example, if students graduate on time, they finish the bachelor's degree in the fourth semester.

While tuition fees are among the highest in Switzerland, they remain comparably low on an international scale. The tuition fee is determined solely by two dimensions: the program and nationality status. Hence, tuition does not depend on household income.[9] International students pay higher fees than Swiss students. Until the spring semester 2012, this difference was moderate. The fee amounted to CHF 1020 per semester for Swiss and to CHF 1170 for foreign students.

The share of international students is fixed at 25% of the student body. That is, the number of international students is tied to the number of swiss students enrolled.[10] The selection of international students is quite rigurous. They have to take an entry exam, the GRE, or the GMAT. Students with the highest scores can enroll at the University of St. Gallen. Because of the high reputation of the University in the German speaking countries, the largest share of international students are Germans and Austrians. No

[8]Of course, there are several obligatory core courses for each program. While students receive recommendations for course selection, they ultimately decide when to take the respective exam.

[9]There are exceptions for scholarship recipients from the cantons St.Gallen and Neuenburg which can be exempt from the fee. However, the number of cases is negligibly small.

[10]For each starting cohort, the overall share of foreigners at the university is calculated and students are accepted to meet the overall quota. An exception are doctoral students. While they are considered in the calculation of the overall share of foreigners, their admission is not limited by it.

selection criteria apply to Swiss students at the bachelor level as they are granted unlimited admission. Hence, the screening of Swiss students takes place ex post during the assessment phase.

Within the framework of the "Task- and Budget Plan 2012-2014"[11], St. Gallen Cantonal Parliament passed an increase in tuition on September 27^{th} 2011. Starting with the spring semester 2012, the university moderately raised tuition for Swiss students at the bachelor and master level by CHF 206 to CHF 1226 - an increase of 20.2%. For foreign students this increase was more pronounced. After the reform the tuition was CHF 2126 which constitutes an increase of CHF 956 or 81.7%.[12][13] These changes applied also to students who were already enrolled by the time of the reform.

The university informed the students in a letter about the planned changes in tuition fees January 11^{th} 2011 - right before the spring semester 2011. On Mai 31^{st} 2011 the cantonal parliament published the draft bill on their website. On September 28^{th} 2011, the university announced in a second letter that the bill passed the cantonal parliament - right before the beginning of the fall semester. Hence, the changes in tuition fees were public knowledge from the Spring Semester 2011 onwards, two semesters before they were implemented. From this point on forward-looking students might have anticipated the increase and altered their behavior accordingly.

Table 1 summarizes the timing of this reform and the exposure of the bachelor students according to the semester in which the students started the bachelor phase. For example, students who started the bachelor phase in the fall semester 2008, learned about the raise shortly before the sixth semester. Only in the eighth semester, they had to pay the higher fees, i.e. four semesters after the standard period of studies. Those students who started in the fall semesters 2009 and 2008, learned about the raise before the fourth and second semester and and payed the higher fee for the first time in the sixth and fourth semester, respectively.

I will use this variation in the exposure to the increase in a DiD framework to estimate the effect of the increase after raise was announced and eventually after it was implemented. That is, I will compare semesters before the raise was announced (spring and fall semester 2010) to the semesters after the announcement and after the raise (spring semester 2011 to fall semester 2012). For example, in order to estimate the an-

[11]The corresponding German name is "Aufgaben- und Finanzplan 2012-2014".

[12]As a reference, in US Dollar the change was about US$ 220 and US$ 1020 for Swiss and foreign students, respectively.

[13]Furthermore, the definition of a foreign student changed in the spring semester 2012. Before, all non-Swiss students without a Swiss high school degree that did not have a Swiss address by the time of registration were considered foreign. From Spring Semester 2012 on, the definition comprises all non-Swiss students without Swiss high school degree that lived outside of Switzerland by the time of their high school graduation. The change of the definition affected only very few students.

Table 1: Semester Overview according to Bachelor Starting Semester

	First Bachelor Semester			
	Pre	Post		
	FS '08	FS '09	FS '10	FS '11
Pre-Treatment				
SS '10	4^{th}	2^{nd}		
FS '10	5^{th}	3^{rd}	1^{st}	
Information about Raise				
SS '11	6^{th}	4^{th}	2^{nd}	
FS '11	7^{th}	5^{th}	3^{rd}	1^{st}
After Raise				
SS '12	8^{th}	6^{th}	4^{th}	2^{nd}
FS '12	9^{th}	7^{th}	5^{th}	3^{rd}

ticipation effect of fourth semester students, I will compare fourth semester students in spring semester 2011 to fourth semester students in the spring semester 2010.

Note, while the bachelor starting cohorts 2008 to 2010 finished the assessment phase before they learned about the increase in tuition fees, the cohort 2011 finished the assessment one semester after they found out. This causes two problems which have to be kept in mind, when it comes to the estimation of the effects. First, since drop-out is particularly high in the assessment phase, the raise might affect the composition of students that start the bachelor level. Second, the covariates used in the estimation comprise amongst others the assessment grade and the number of attempts. Thus, for this cohort these covariates might be influenced by the treatment and are not necessarily exogenous.

3 Identification with DiD in a Multiple Treatment Setting

The differential change in tuition fees allows me to employ a DiD approach to identify and ultimately estimate the effect of tuition fees on student's achievement. Generally speaking, Swiss students will be used to estimate the impact of other time varying factors on the outcomes of interest. This application is somewhat unusual since both groups receive a treatment. Tuition for Swiss students is raised by CHF 206 and tuition for foreign students increases by CHF 956. None of the students pay the same tuition as before, in other words, none of the students is not treated. The standard DiD refers precisely to the non-treatment state. Because this state is not observed, the DiD approach in this setting requires slightly stronger assumptions as in the standard case.

Two different average treatment effects on the treated ($ATET$) are of interest in this stetting. On the one hand, the effect of the high increase compared to the low increase is straight-forward as those are the two observed states. On the other hand, the effect of paying the high increase versus experiencing no change can be partially identified under slightly weaker assumptions.

Let Y_t^d be the potential outcome in period $t \in \{1, 0\}$, where $d \in \{H, L, 0\}$ is a treatment indicator. The treatment $D = H$ corresponds to the raise in tuition of CHF 956, $D = L$ to the raise of CHF 206, and $D = 0$ to the non-treatment state. $t = 1$ indicates post-treatment and $t = 0$ the pre-treatment periods.[14] Now, the above effects can be expressed as:[15]

$$ATET_1^{H\,L} = E[Y_1^H|D = H] - E[Y_1^L|D = H], \tag{1}$$
$$ATET_1^{H\,0} = E[Y_1^H|D = H] - E[Y_1^0|D = H]. \tag{2}$$

(1) is the effect of the high versus the low increase, and (2) is the effect of the high increase versus no increase.

The major challenge to identify the $ATET$ is that the counterfactual situation is never observed. That is, what would have happened to the treated if they were not treated? Here, the counterfactual situations is either $E[Y_1^L|D = H]$ or $E[Y_1^0|D = H]$, i.e. the mean expected time-to-degree of foreign students if they had experienced either the raise of CHF 206 or no raise at all. The DiD approach solves this problem by assuming parallel trends of the counterfactual situation. Under this common trend (CT) assumption, the counterfactual can be expressed in three moments that can be identified from the data.

[14]In the following, I implicitly impose two assumptions that are needed to identify all of the aforementioned parameters. The stable unit treatment value assumption (SUTVA) implies that the potential outcomes correspond to the observed ones. Thus, interactions between students that influence each others' time-to-degree are not allowed. It would be particularly worrisome if Swiss students adjust their behavior to the reaction of the foreign students. For instance, if foreign students graduate faster, Swiss students might be afraid of disadvantages in the labor market. Thus, they might increase their speed of studying as well. My results would underestimate the true effect. Preliminary research on peer effects at the University of St.Gallen indicates that there are no attainment effects of the freshmen introduction week group composition with respect to nationality (Thiemann, 2012). A specific assumption of the DiD approach is that the treatment has no effect in the pre-treatment period (NEPT), more formally $ATET_0^{H\,L} = 0$ or $ATET_0^{H\,0} = 0$. This assumption could be violated here since the university informed the students about the possibility and later about the final decision of the raise before it was implemented. However, I solve this problem by using the spring semester and fall semester 2010 as pre-treatment period. These two semesters are before the University informed about the raise in tuition.

[15]This discussion closely follows Lechner (2011).

First, consider the CT for the effect of the high versus the low increase given in (1),

$$E[Y_1^L|D=H] - E[Y_0^0|D=H] = E[Y_1^L|D=L] - E[Y_0^0|D=L], \quad (3)$$

$$\underbrace{E[Y_1^L|D=H]}_{counterfactual} = E[Y_1^L|D=L] - E[Y_0^0|D=L] + E[Y_0^0|D=H]. \quad (4)$$

This assumption states, the mean potential outcomes under the hypothetical treatment state $D = L$ develop equally over time, i.e. foreign students would have reacted in the same way as Swiss students to an increase of CHF 206. By plugging in (4) into (1), we can see that the $ATET_1^{HL}$ is point identified:

$$\begin{aligned} ATET_1^{HL} &= E[Y^H|D=H] - E[Y_0^0|D=H] - \left[E[Y_1^L|D=L] - E[Y_0^0|D=L]\right] \\ &= \underbrace{E[Y_1|D=H]}_{identified} - \underbrace{E[Y_0|D=H]}_{identified} - \left[\underbrace{E[Y_1|D=L]}_{identified} - \underbrace{E[Y_0|D=L]}_{identified}\right] \end{aligned}$$

The CT in (3) is slightly different to the standard CT, which refers to the non-treatment states. Not only are all unobserved factors required to influence Swiss and foreign students equally, but the effect of the low increase has to be homogeneous for both groups. These conditions can be separated by expanding Equation (3):

$$\begin{aligned} &\left[E[Y_1^L|D=H] - E[Y_1^0|D=H]\right] + \left[E[Y_1^0|D=H] - E[Y_0^0|D=H]\right] \\ &= \left[E[Y_1^L|D=L] - E[Y_1^0|D=L]\right] + \left[E[Y_1^0|D=L] - E[Y_0^0|D=L]\right]. \end{aligned}$$

The first difference on each side of the equation represents the effect of the low increase for each group in the post-treatment period. The second difference represents the effect of time or in other words the impact of other time varying factors.[16] This second difference is exactly the standard CT assumption, which is thus weaker than the above assumption. In this setting, it might very well be that Swiss and foreign students have different price elasticities of education. In that case, the assumption would be violated, and (1) would not be identified.

In case homogeneous effects are not credible, one solution is to consider the $ATET_1^{H0}$ given in (2) instead of the $ATET_1^{HL}$. Even under effect homogeneity the DiD cannot point identify this effect as the CT includes two counterfactual situations. The potential

[16] In one particular case, (4) could still hold if the effects were heterogeneous and the effect of time was different for both groups. That is, if the difference in the effect of time compensated the difference in the effect of the low increase. This case, however, is very unlikely.

outcome without a raise in period $t = 1$ is not observed for either group.[17] Formally,

$$\begin{aligned} E[Y_1^0|D=H] - E[Y_0^0|D=H] =& E[Y_1^0|D=L] - E[Y_0^0|D=L] \\ \underbrace{E[Y_1^0|D=H]}_{counterfactual} =& \underbrace{E[Y_1^0|D=L]}_{counterfactual} - E[Y_0^0|D=L] + E[Y_0^0|D=H]. \end{aligned} \tag{5}$$

However, the lower bound in absolute values can be identified under an assumption about the relation between the counterfactual $E[Y_1^0|D=L]$ and the observed moment $E[Y_1^L|D=L]$:

$$\begin{aligned} E[Y_1^0|D=L] \leq& E[Y_1^L|D=L] \\ &\text{if } E[Y_1^H|D=H] - E[Y_0^0|D=H] > E[Y_1^L|D=L] - E[Y_0^0|D=L] \end{aligned} \tag{6}$$

$$\begin{aligned} E[Y_1^0|D=L] \geq& E[Y_1^L|D=L] \\ &\text{if } E[Y_1^H|D=H] - E[Y_0^0|D=H] < E[Y_1^L|D=L] - E[Y_0^0|D=L]. \end{aligned} \tag{7}$$

Together with the CT in (5), this assumption states that CHF 206 affect Swiss students less than but in the same direction as the CHF 956 affect foreign students. The first case is visualized in Figure 1. The black line and the dark gray line represent the observed expected time-to-degree of foreign and Swiss students, respectively. The light gray and the dashed light gray lines correspond the potential outcomes without a raise. The DiD estimates the difference between the observed development of the foreign and Swiss students, i.e. the difference between the black and the dark gray dashed line in the post-treatment period. In the the first case, this difference is positive. That is, the time-to-degree of foreign students increases either more (left graph) or decreases less (right graph) over time than the time-to-degree of Swiss students. If the dark gray dashed line corresponded to the expected potential outcome of foreign students in the low increase scenario, assumption (4) would be fulfilled and the $ATET_1^{H\,L}$ would be identified. If this is not the case, assumption (6) together with the CT in the non-treatment scenario, states that the expected potential outcome without the raise in the post treatment period would have been less than with the low increase for both groups. Hence, the $ATET_1^{H\,0}$, which is the difference between the black line and the light gray dashed line, will always be bigger than or equal to the DiD estimate. The above assumptions imply both the high and low increases have a positive impact, but the impact high raise on the foreigners is larger than the impact of the low raise on the Swiss.

[17] Only if the effect of the low increase is zero for both groups, the $ATET_1^{H\,0}$ can be identified. In that case $ATET_1^{H\,0} = ATET_1^{H\,L}$.

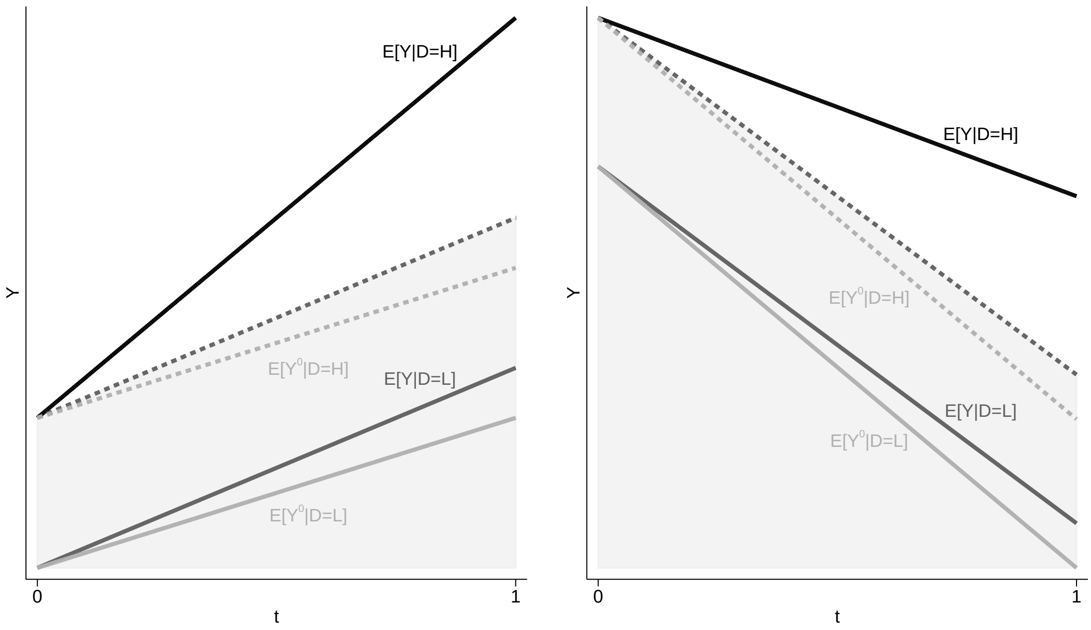

Figure 1: Positive DiD, Assumption (6)

The second case is depicted in Figure 2. The DiD is negative. That is, the time-to-degree of the foreign students either decreases more (left graph) or increases less (right graph) than that of the Swiss students. In contrast to the first case, (7) states that the expected potential time-to-degree in absence of the treatment is above the expected time-to-degree in the low increase scenario. Again, this holds for both groups because of the CT assumption on the non-treatment state. Hence, the $ATET_1^{H\,0}$ will always be smaller than or equal to the DiD estimate. Swiss students are affected less "negative" by the low increase than foreigners by the strong increase.

To show that this assumptions allows us to interpret the DiD estimate as the lower bound of $ATET_1^{H\,0}$ in absolute values, we start by plugging in the (6) in (5):

$$E[Y_1^0|D=H] \leq E[Y_1^L|D=L] - E[Y_0^0|D=L] + E[Y_0^0|D=H]. \tag{8}$$

Then, if we plug (8) into (2), we get

$$\begin{aligned} ATET_1^{H\,0} \geq & E[Y_1^H|D=H] - E[Y_0^0|D=H] - \left[E[Y_1^L|D=L] - E[Y_0^0|D=L]\right] \\ = & \underbrace{E[Y_1|D=H]}_{identified} - \underbrace{E[Y_0|D=H]}_{identified} - \left[\underbrace{E[Y_1|D=L]}_{identified} - \underbrace{E[Y_0|D=L]}_{identified}\right] \\ > & 0. \end{aligned}$$

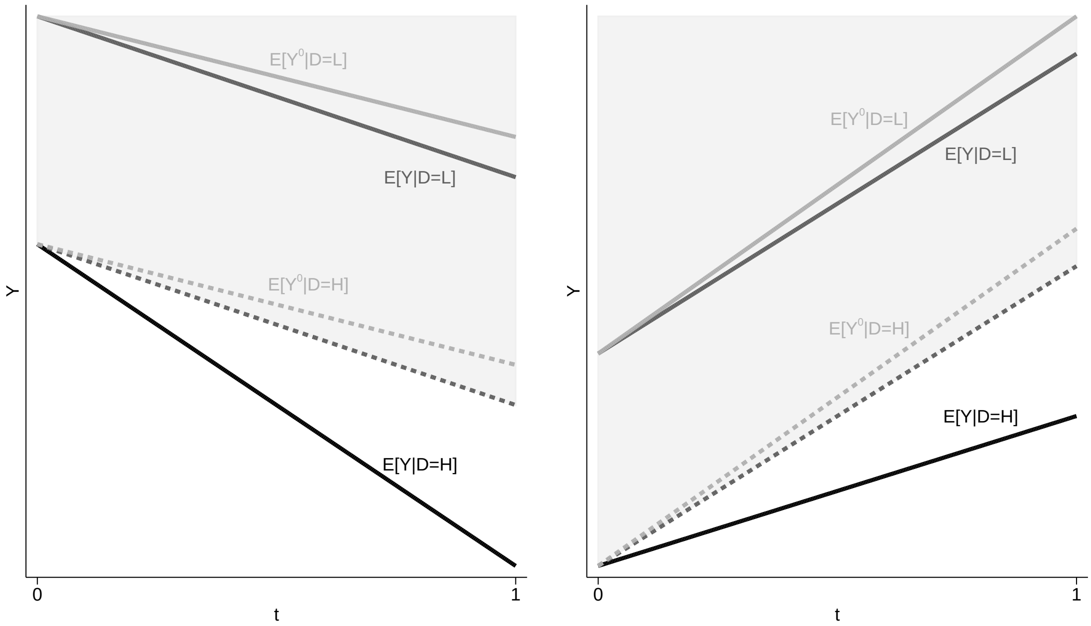

Figure 2: Negative DiD, Assumption (7)

Similarly, for the second case:

$$ATET_1^{H\,0} \leq E[Y_1|D = H] - E[Y_0|D = H] - [E[Y_1|D = L] - E[Y_0|D = L]] < 0.$$

Note that the assumption of parallel trends of the non-treatment in (5) is stronger than necessary. The above results hold also if

$$\begin{aligned} E[Y_1^0|D = H] - E[Y_0^0|D = H] &\leq E[Y_1^L|D = L] - E[Y_0^0|D = L] \\ \text{if } \; E[Y_1^H|D = H] - E[Y_0^0|D = H] &> E[Y_1^L|D = L] - E[Y_0^0|D = L] \end{aligned} \tag{9}$$

$$\begin{aligned} E[Y_1^0|D = H] - E[Y_0^0|D = H] &\geq E[Y_1^L|D = L] - E[Y_0^0|D = L] \\ \text{if } \; E[Y_1^H|D = H] - E[Y_0^0|D = H] &< E[Y_1^L|D = L] - E[Y_0^0|D = L]. \end{aligned} \tag{10}$$

The development of foreign students with the low increase is smaller than (first case) or equal to, or bigger than or equal to (second case) the observed development of the Swiss students. In other words, the expected potential outcome of foreign students in absence of the treatment is anywhere in the gray shaded area in Figures 1 and 2. Hence, even if unobserved factors influence both groups differently, the interpretation of DiD as a lower bound is still valid in certain cases. If the DiD is positive, unobserved factors can influence both groups differently as long as long as foreign students would have developed less without the treatment than the Swiss students with the low increase. Conversely, if the DiD estimate is negative, unobserved factors must influence foreigners in a way that the development over time without the raise is bigger for the Swiss under the low raise.

In principle, one could derive similar bounds of the $ATET_1^{HL}$ in the presence of heterogeneous effects. To this end, we would need an assumption about the nature of heterogeneity. That is, do Swiss or foreigners react stronger to CHF 206? However, in this setting one cannot be sure which scenario is true. In general, nominal and real family income is among the highest in Switzerland. Hence, the price elasticity should be lower for Swiss. However, foreign students that choose to study in Switzerland have to have the means to afford the higher costs of living. This might indicate that Swiss students have a higher price elasticity. See Appendix A.1 for a formal discussion of the necessary assumption.

In conclusion, the DiD estimate will always be same. But, depending on the assumptions imposed, it can be interpreted as a different parameter. Under the assumption of effect homogeneity for the CHF 206 increase in tuition, the effect of high versus low is point identified. In this setting, it seems more reasonable to assume that the Swiss react to the low increase in the same direction as the foreigners to the high increase. The estimated parameter can then be interpreted as a lower bound in absolute values of the effect of the high increase compared to no increase.

4 Data and Descriptive Statistics

The data are based on administrative students records of the University of St.Gallen. The records cover the entire population of entering cohorts for the years 2003 until 2012. They include enrollment, course choice and completion, grades, as well as graduation dates. Furthermore, they provide basic demographic information such as gender, year of birth, whether the student is foreign, and whether the student is a native German speaker. Information on the tuition fees allows me to identify the treated group.

The main outcomes are whether a student graduates or not, earned credits, and the weighted grade average in a given semester. The credits are simply the sum of all credits in a given semester. The grades are weighted with the credits. Credits and grades include passed courses as well as failed courses. Even if a student fails a course, he cannot repeat it and the course counts as taken. Only a student who fails too many courses has to retake all failed courses again. This second attempt starts in the following semester. Hence, the inclusion of failed courses should no be problematic for a given semester. The credits still constitute a measure of the student's effort or in other words his pace of studying.

The final sample is selected as follows. First, I restrict the sample to students in the bachelor phase. Second, I discard students that did not do the assessment phase at the University of St.Gallen. If students did not start their studies in St.Gallen, I cannot observe the semester they are in and the assessment information. Third, I only consider students that were matriculated throughout their studies. Fourth, I do not use students for which the nationality status changes. Fifth, I discard students that started the bachelor phase in a spring semester or during the assessment phase. These cases are are only possible for a few exceptions. Students can prolong the assessment phase to four semesters if their ability to study is impaired by one of the following reasons: they are non-German native speakers, they have disabilities, or have they have family duties. This selection step ensures students all start under the same conditions and are comparable in a given semester. Sixth, I consider only students that enroll before spring semester 2011, i.e. before the the raise was announced. This way, the student composition is not affected by the raise in tuition fee. Seventh, if students major in two subjects, such as business and economics, I only consider the declared main study. Finally, I only follow students until their first degree. Appendix A.2 shows the number and variable means of students that were excluded at each selection step for the four entry cohorts used in the main analysis.

Table 2: Descriptive Statistics by Bachelor Entry Cohort

	FS 2008		FS 2009		FS 2010		FS 2011	
	Swiss	Foreign	Swiss	Foreign	Swiss	Foreign	Swiss	Foreign
Female	0.326	0.208***	0.290	0.170***	0.345	0.215***	0.347	0.161***
German Speaker	0.965	0.983	0.961	0.963	0.938	0.97*	0.948	0.976
Age	21.145	20.792***	21.303	20.815***	21.174	20.785***	21.206	20.742***
Ass. Grade	4.616	4.968***	4.573	4.844***	4.600	4.803***	4.618	4.812***
2^{nd} Attempt	0.117	0.025***	0.143	0.022***	0.156	0.044***	0.151	0.048***
2^{nd} Major	0.018	0.158***	0.007	0.215***	0.024	0.111***	0.018	0.129***
Observations	488	120	558	135	545	135	504	124

*, **, *** significantly different at p<0.10, p<0.05, p<0.010

Table 2 describes the data separately for Swiss and foreign students for the bachelor entry cohorts of the fall semesters 2008 to 2011. Foreign students are on average more likely male and are younger when they enroll. Both groups contain mainly German native speakers. As for the educational achievement in the assessment phase, foreign students finish the assessment phase with better grades and less often require two attempts. The above differences are persistent over the four cohorts. Furthermore, foreign students major more often in two subjects than Swiss students. As mentioned before, foreign students experience a much more rigorous selection process before they start the assessment phase, while the selection of Swiss students takes places mainly during the assessment phase.

This difference in ex-ante and ex-post selection causes the differences in the achievement during the assessment phase.

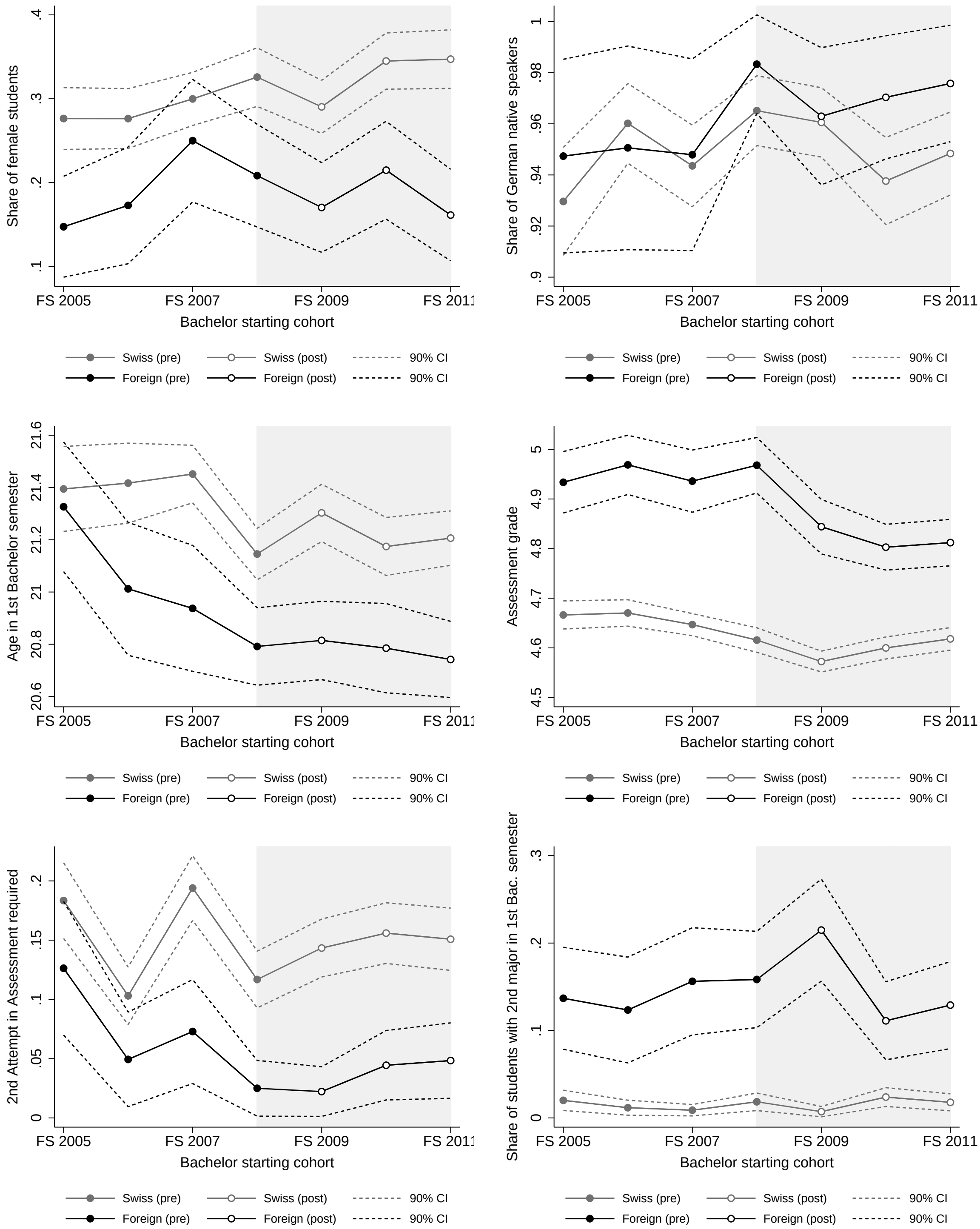

Figure 3: Characteristics of Bachelor Entry Cohorts 2005 to 2012

Figure 3 displays the development of the above characteristics over time for both groups. The gray shaded area marks the four cohorts used for the main analysis. The characteristics seem to follow a similar underlying trend. However, in some periods both groups deviate from that trend. Since these changes in the student composition might be correlated with student achievement, these characteristics should be controlled for. Hence, all estimations will be carried out unconditionally as well as conditionally on these characteristics.

Now, consider the main outcomes. Figure 4 illustrates the development of the probability to graduate in a given semester. The light gray area marks the the announcement of the raise and the dark gray area the raise itself. In the first three semesters, the probability to graduate is either zero or negligibly small. Hence, graduation before the standard period of studies is not common in either group. In the fourth semester, the probability to graduate has been decreasing from approximately 50% to around 25% in both groups. This decrease was parallel until the universitity announced the fee raise before the spring semester 2011. The probability that foreign students graduate in the fourth semester increases more than 10%, while the probablitity of Swiss students remains fairly stable. In the spring semester 2012, the probability to graduate for foreign students returns to the previous level. This hike provides evidence of a positive anticipation effect.

Figure 4 shows that late graduation has become the rule rather than the exception at the University of St. Gallen. A closer look at the time-to-degree by cohort reveals that students increasingly delay graduation by one or two semesters. See Appendix A.3. While this development is striking, it is in line with the experience of other universities. For example, Garibaldi, Giavazzi, Ichino, and Rettore (2011) report a probability of graduating on time of 20% at Bocconi University. In the US, only 39.4% of the college graduates of the high school class of 1992 recieved their bachelor's degree after four years while still 53.1% of the high school class of 1972 graduated on time (Bound, Lovenheim, and Turner, 2012).

Consider the credits earned in a given semester. Figure 5 displays the development from 2005 to 2012. As mentioned before, students should earn approximately 30 credits each semester to graduate on time. In semesters one and two, both groups earn only slightly less than 30 credits on average. Besides, more pronounced fluctuations of the Swiss students in the first semester, both groups behave fairly similar over time. The picture changes in the third semester. Foreign students earned dramatically more credits than Swiss students in the fall semester 2006. In the fall semester 2007, they earn less on average. Between 2008 and 2010, both groups show parallel trends. Once the information of the raise is released, foreign students earn less credits whereas Swiss students accumulate more. This reaction might point at a negative anticipation effect. However,

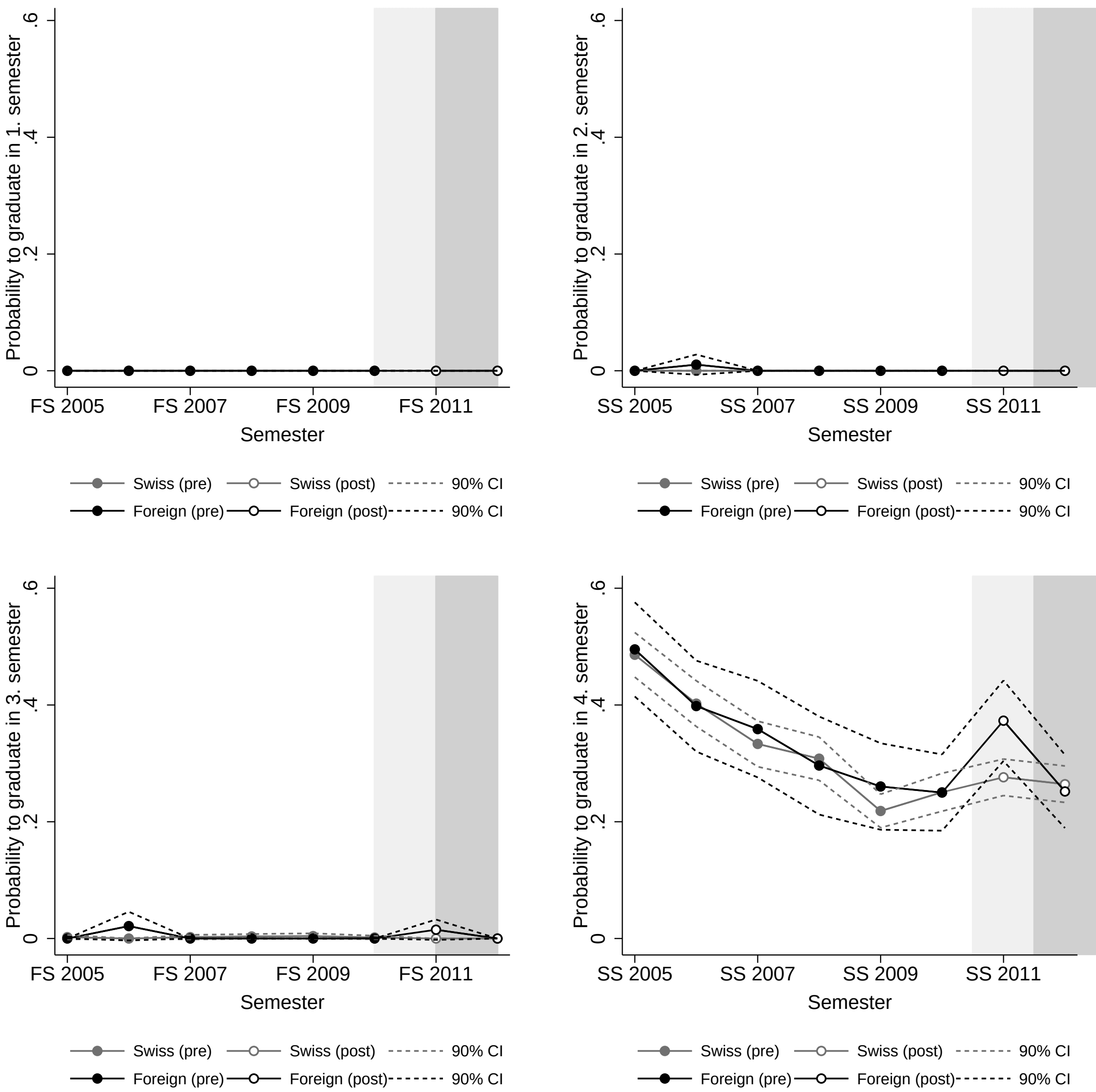

Figure 4: Probability to Graduate 2005 to 2012

in light of the severe fluctuations in 2006 and 2007, one should be concerned about the validity of the common trend assumption.

In the fourth semester, students demonstrate a similar pattern as with regards to the probability to graduate. While students earn around 32 credits in 2005, students earn only around 24 credits in 2010. This decrease is steady and fairly similar for both groups. After the raise was made public, credits show a notable increase for foreign students while they remain stable for Swiss students in the spring semester 2011. However, they return to previous levels when the raise is implemented.

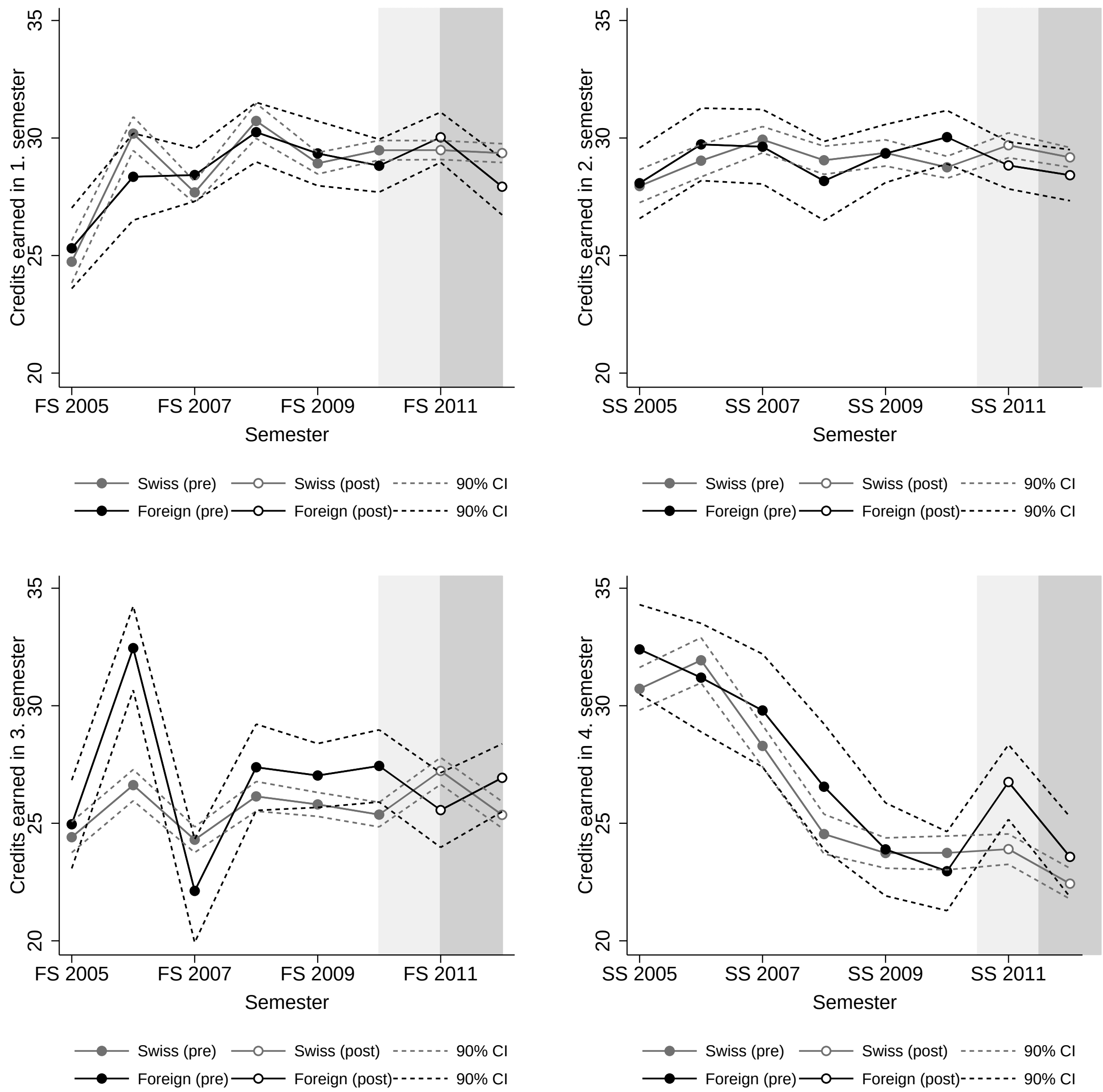

Figure 5: Credits earned 2005 to 2012

As for the weighted grades, foreign students show higher grades across all semesters. This difference remains constant in most years with two exceptions. Foreign students deviate from the trend in the first semester in 2008 and in the third semester in 2009. Even after the raise was announced and eventually implemented, both groups develop similarly. Only in the first semester, foreign students demonstrate a somewhat more pronounced decrease than before.

In conclusion, the graphical analysis suggests that foreign students close to graduation increased their effort to avoid the higher fees. These higher effort levels seem not to have

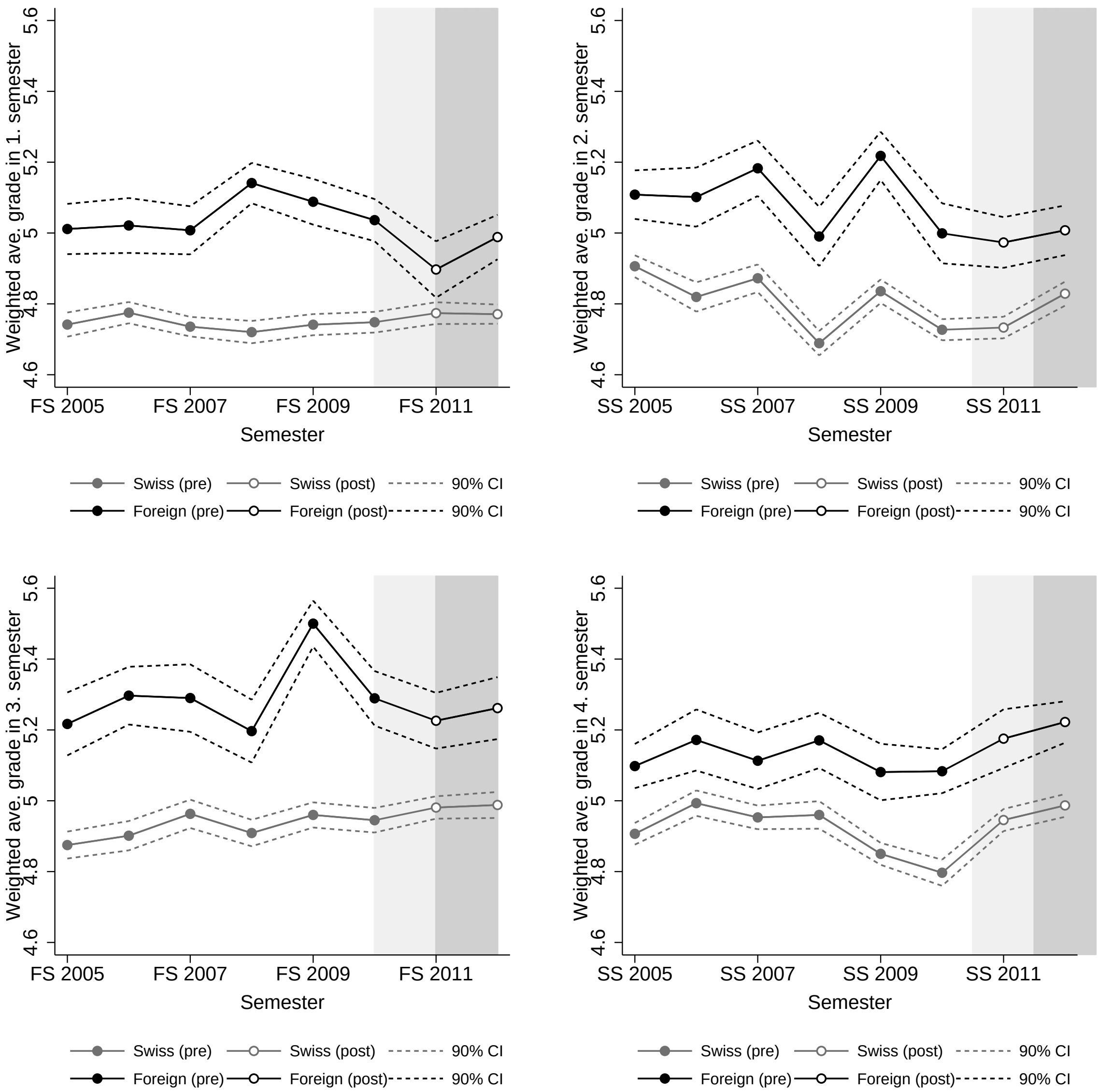

Figure 6: Weighted Grade Averages 2005 to 2012

an effect on the performance of the students. Furthermore, the main outcomes of both groups move fairly similar over time. This suggests that the common trend assumption seems to be plausible in most instances. In Section 8, I use placebo test to statistically assess the common trend assumption in pre-treatment periods.

5 Estimation

Using a standard linear model as baseline, the effect of the raise in tuition is estimated by

$$Y_i = \beta_0 + \beta_1 FOREIGN_i * POST_i + \beta_1 FOREIGN_i + \beta_3 POST_i + \delta X_i + \epsilon_i \qquad (11)$$

where $FOREIGN_i$ is an indicator for students that pay the high tuition, $POST_i$ is an indicator for the post-treatment periods and X_i is a vector of student characteristics. Y_i represents the outcomes of interest, whether a students graduates, credits and the weighted grade in a given semester. Depending on the assumptions, β_1 is the effect of the high raise in tuition fees compared to the low raise given in Equation (1), or the lower bound in absolute values of the effect of the high raise compared to no raise given Equation (2).

The student characteristics include age, gender, assessment grade, whether the students needed two attempts to finish the assessment, whether the student majors in two subjects at the beginning of the bachelor phase, and whether the student is not a native German speaker. Furthermore, I add the square of age and the assessment grade in order to allow for non-linear effects. All these covariates are measured for the most part before the treatment. Thus, they should be exogenous. As mentioned in Section 2, the only exception is the bachelor entry cohort 2011, which was finishing the assessment when the raise was announced. See Table 1, for the affected semester.

As mentioned in Section 2, I estimate Equation (11) separately for the periods: spring semester and fall semester 2011, and spring semester 2012. One has to keep in mind that fall semesters and spring semesters differ at least in two respects. First, the programs start in the fall semester, and if students are on time, programs end in the summer semester. Hence, students are in their first, third, and fifth semester in fall semesters. In spring semesters, students are in the second and fourth semester. Second, the courses that are offered change. Therefore, I use spring semester 2010 as pre-treatment period for spring semester 2011 and 2012, and the fall semester 2010 as pre-treatment period for fall semester 2011.

One immediate concern, is attrition due to drop-out and early graduation. If the raise affects this attrition, estimates for subsequent semesters might be biased. Fortunately, drop-out and early graduation are very rare in the bachelor phase. Additionally, in the weighted grades specification, I restrict the sample to students that finished at least one graded course. Students that do not complete any credits, are mainly on leave of absence. Students can take a leave of absence for an internship, motherhood or other family obligations, in case of sickness or other hardship, to finish their thesis, and because of military obligations. Nonetheless, the overall share in the sample is rather small. See Appendix A.4 for the incidence and descriptive statistics of students that terminate their studies early or do not complete any credits.

Recently, inference in a DiD setup has received increasing attention. Using only one pre- and one post-treatment period, I do not run into problems of serially correlated outcomes as pointed out in Bertrand, Duflo, and Mullainathan (2004). However, since only two groups are considered, the standard errors might be underestimated because of common group error components (Donald and Lang, 2007). Additionally, if the estimate is interpreted as a lower bound, considerations about inference of partially identified parameters are necessary. Imbens and Manski (2004) show that if the identification area is sufficiently large, one-sided confidence intervals can be used outside the bounds. Since the parameter of interest can only be close to one border of the identification area, the probability that the estimate of opposite border exceeds the true parameter value can be neglected.

In the set-up considered here, one complication arises. The estimate can be interpreted as a lower bound in magnitude. That is, if the estimate is negative, it is an upper bound and if it is positive, it is a lower bound. Thus, for estimates that are not significantly different from zero, it is not clear whether its a lower or an upper bound. In these cases, the approach of Imbens and Manski (2004) can not be applied. As I do not know on which side of the estimate the true values lies, I cannot use a one-sided interval. Therefore, I will use a two step procedure. First, I decide whether the estimate is positive or negative with a regular two-sided test. Second, for those estimates that are significantly positive or negative, I construct the corresponding one-sided interval.

6 Time-to-Degree and Student Performance

This section presents the regression results. I use Equation (11) to estimate the effect of the increase in tuition fees on the probability to graduate on time, i.e. in the fourth semester, and the credits earned as measures of time-to-degree, and the weighted average grade as a measure of performance. The results are shown in Table 3. Remember, the coefficient of $FOREIGN * POST$ is the effect of the high increase compared to the low increase under Assumption (4) or the lower bound in magnitude of the effect of high increase compared to no increase under Assumptions (6) and (7).

Panel 1 shows the results on the probability to graduate in the fourth semester. In line with the graphical analysis, there is a notable positive increase, 9.7% point, in the probability to graduate on time in the Spring Semester 2011, i.e. two semesters before the raise. Unconditionally, it is only significant at the 15% significance level (p-value=0.127).

However, after controlling for changes in the composition of the students, it is significant at the 5% level. This increase translates into 38.8% more students graduating on time given the initial level of 25%. Once the raise is implemented, the effect disappears. In the spring semester 2012, the estimate is close to zero and insignificant.

Consider the credits earned in a given semester. See Panel 2. Similar to the above results, the estimated effect on credits for forth semester students in the spring semester 2011 is positive and significant at the 5% level. Foreign students earn on average around 3.5 credits more after the raise is announced, which correspond to a little less than an average course . In the spring semester 2012, the effect on credit accumulation is smaller and only significant at the 10% level after controlling for covariates. Students in earlier semesters, show a negative impact on the credits earned in the semesters before the raise. Second semester students in the spring semester 2011 and 2012 complete around two credits less on average. In the spring semester 2012 this effect is only significant unconditionally. Third semester students in the fall semester 2011 earn more than three credits less. Once the students have to pay the higher fees, the effect disappears.

Panel 3 depicts the results for average grades. In most semesters, the coefficients are close to zero and insignificant. Note, no effects are found for fourth semester students in the spring semester 2011. Thus, students accomplish to earn more credits and graduate earlier without jeopardizing their grades. Only in the fall semester 2011, the coefficients are negative for first semester students. This negative effect is surprising as students in their first bachelor semester do not significantly alter the course load. However, while statistically significant, an effect of -0.165 grade points from an initial value of 5.04 merely represents a decrease of 3.3%.

Note, in the above discussion, I treated the estimated coefficients as point identified effects. This interpretation relies the assumption of homogeneous effects of the low fee raise. If only the assumption that both groups react in the same direction holds, the estimated effects can still be interpreted as lower bounds in absolute values. That is, the true effects on the probability to graduate and on the credit accumulation for fourth semester students in the spring 2011 could even be bigger. Similarly, the negative effects for second and third semester students might even be smaller ("more negative"). In the those cases in which the estimate is not significantly different from zero, nothing can be said about the true effect. It could either be bigger or smaller.

Table 3: Effects on Student Achievement

	SS 2011				FS 2011				SS 2012				FS 2012	
	2^{nd} Sem.		4^{th} Sem.		1^{st} Sem.		3^{rd} Sem.		2^{nd} Sem.		4^{th} Sem.		3^{rd} Sem.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel 1														
PROB. TO GRADUATE														
FOREIGN*POST			0.097	0.131**							-0.012	0.020		
			(0.064)	(0.064)							(0.061)	(0.061)		
				[0.026]										
FOREIGN			-0.001	-0.071							-0.001	-0.059		
			(0.044)	(0.047)							(0.044)	(0.047)		
POST			0.026	0.037							0.014	0.016		
			(0.027)	(0.027)							(0.027)	(0.027)		
CONSTANT			0.251***	-0.269							0.251***	1.868		
			(0.020)	(2.325)							(0.020)	(2.207)		
Adjusted R^2			0.004	0.035							-0.002	0.024		
Observations			1291	1291							1275	1275		
Panel 2														
CREDITS														
FOREIGN*POST	-2.139**	-1.891*	3.634**	3.446**	1.204	1.185	-3.736***	-3.259**	-2.040**	-1.600	1.918	2.747*	-0.494	0.230
	(1.014)	(1.008)	(1.521)	(1.543)	(1.004)	(0.979)	(1.419)	(1.427)	(1.027)	(1.015)	(1.567)	(1.567)	(1.361)	(1.346)
	[-0.471]	[-0.234]	[1.131]	[0.907]			[-1.402]	[-0.912]	[-0.35]			[0.17]		
FOREIGN	1.282*	0.468	-0.778	-1.938*	-0.658	-1.547**	2.072**	1.410	1.282*	0.497	-0.778	-1.891	2.072**	0.639
	(0.746)	(0.793)	(1.110)	(1.170)	(0.727)	(0.741)	(0.984)	(1.024)	(0.746)	(0.817)	(1.110)	(1.189)	(0.984)	(1.016)
POST	0.937**	0.797*	0.160	0.228	0.006	-0.030	1.858***	1.738***	0.422	0.271	-1.308**	-1.371**	-0.011	-0.186
	(0.429)	(0.428)	(0.588)	(0.581)	(0.353)	(0.342)	(0.471)	(0.474)	(0.381)	(0.380)	(0.590)	(0.583)	(0.468)	(0.472)
CONSTANT	28.755***	36.781	23.741***	98.405*	29.480***	9.338	25.369***	-3.195	28.755***	48.620	23.741***	104.655**	25.369***	22.953
	(0.284)	(43.687)	(0.437)	(55.597)	(0.254)	(31.764)	(0.318)	(49.970)	(0.284)	(43.025)	(0.437)	(50.291)	(0.318)	(44.775)
Adjusted R^2	0.003	0.016	0.007	0.023	-0.000	0.052	0.010	0.016	0.002	0.018	0.001	0.035	0.006	0.026
Observations	1368	1368	1291	1291	1308	1308	1364	1364	1318	1318	1275	1275	1318	1318
Panel 3														
GRADES														
FOREIGN*POST	-0.032	0.033	-0.057	-0.002	-0.165**	-0.157***	-0.099	-0.025	-0.093	-0.013	-0.051	0.053	-0.071	0.007
	(0.072)	(0.066)	(0.069)	(0.059)	(0.066)	(0.051)	(0.073)	(0.061)	(0.072)	(0.060)	(0.060)	(0.049)	(0.077)	(0.067)
					[-0.057]	[-0.073]								
FOREIGN	0.272***	0.017	0.287***	0.015	0.288***	0.099***	0.344***	0.092**	0.272***	-0.002	0.287***	0.025	0.344***	0.095**
	(0.054)	(0.052)	(0.044)	(0.037)	(0.040)	(0.029)	(0.051)	(0.046)	(0.054)	(0.051)	(0.044)	(0.037)	(0.051)	(0.048)
POST	0.006	-0.018	0.149***	0.188***	0.026	0.012	0.036	0.014	0.102***	0.064***	0.190***	0.209***	0.043	0.001
	(0.026)	(0.020)	(0.029)	(0.025)	(0.026)	(0.019)	(0.029)	(0.023)	(0.027)	(0.022)	(0.030)	(0.025)	(0.031)	(0.025)
CONSTANT	4.727***	5.390***	4.797***	0.827	4.748***	4.938***	4.945***	1.509	4.727***	6.993***	4.797***	2.209	4.945***	2.999
	(0.018)	(2.014)	(0.022)	(1.846)	(0.018)	(1.524)	(0.021)	(1.993)	(0.018)	(1.774)	(0.022)	(1.645)	(0.021)	(2.100)
Adjusted R^2	0.047	0.396	0.061	0.331	0.040	0.475	0.055	0.372	0.044	0.412	0.080	0.350	0.055	0.354
Observations	1348	1348	1276	1276	1301	1301	1330	1330	1298	1298	1264	1264	1289	1289
X		✓		✓		✓		✓		✓		✓		✓

Robust standard errors in round parentheses. One-sided 95% confidence bound as suggested in Imbens and Manski (2004) in squared parentheses.
* p<0.10, ** p<0.05, *** p<0.010
PROB. TO GRADUATE: binary indicator if student graduates in fourth semester, CREDITS: earned credits in a given semester, GRADES: weighted average grade in a given semester. Controls include age, age squared, gender, assessment grade, assessment grades squared, whether the students needed two attempts to finish the assessment, whether the student majors in two subjects at the beginning of the bachelor phase, and whether the student is not a native German speaker. Spring and Fall Semesters 2010 control semesters, respectively.

The linear model given in Equation (11) is rather inflexible when it comes to controlling for covariates. It does not allow interactions among the covariates nor between covariates and treatment. Furthermore, it does not enforce common support. To check how sensitive the results are to the linear specification, I estimate the effects also using a semi-parametric matching approach as suggested in Lechner (2011). Additionally, in case of the probability to graduate, the non-linearity in the expected outcomes leads to a violation of the common trend assumptions. Therefore, I estimate a non-linear DiD following Blundell and Dias (2009) as well. The estimates for the probability to graduate and the grades do not differ much across the estimation procedures. The semi-parametric estimates for credits have the same sign but are slightly bigger than the those of the linear model. In general, the semi-parametric and the non-linear DiD are less precise. See Appendix A.5 for results and estimation details.

In conclusion, students that are on the margin to graduate, i.e. fourth semester students, increase their effort after the raise is announced. A tempting explanation is that these students try avoid the higher fees. However, it is puzzling that students anticipate the raise two semesters before it is implemented. That is, students still have the fifth semester without paying more. So why should students that otherwise would have finished in the fifth or sixth semester graduate in the fourth? It turns out that part of this increase can most likely be attributed to a reform of the admission criteria on the master level. See Section 8.

Students that increase their effort do not jeopardize their grades. Furthermore, students in earlier semester, i.e. those are not yet close to graduation, seem to show negative anticipation effects. This would be in line with the explanation that students shift time from school to market work. Or in other words, students neglect their studies in order to finance them. However, due to the above mentioned inference problems these negative effects should not be overemphasized. Hence, the take away should be that positive effects on students in earlier semester can most likely be ruled out.

7 Related Student Behavior

This section tries to shed more light on the students reaction to the fees. To that end, I estimated Equation (11) for several additional outcomes. These are the number of courses, whether the student finishes the bachelor thesis, the number of complementary courses, whether the student dropped or started a a second major, whether the student is on exchange, or on leave of absence. See Table 4 for the results.

Consider the results on the number of courses and whether the student finished the bachelor thesis in the respective semester. Fourth semester students in the spring semester

2011 do not show an effect on the number of courses they complete. However, there is an increase of 15.3%-points of the students that complete the bachelor thesis. Hence, those students that show an increase in the probability to graduate seem to be missing only the thesis and not regular coursework. As for the other semesters, the results on course completion are in line with the results on credit completion. Second semester students in the spring 2011 and third semester students in the fall 2011, show a negative effect on the number of courses which roughly translates into the negative effect on the number of credits. These effects are, however, only significant at the 10% significance level. After controlling for student's characteristics the effects are significant at the 10.1% and 12.8% significance levels, respectively. Moreover, the positive effect found for first semester students is striking. Since, students in that period are not affected in their credits accumulation. This dicrepancy points at a change in the composition of the completed courses.

Consider complementary courses and second majors. Complementary courses are required for complementary certificates, such as business education or book and publishing studies. Students that opt for these certificates or a second degree are potentially longer enrolled than students with only one major. Hence, students might decide not pursue these additional degrees in oder to avoid the tuition fee raise. If that was the case, we would observe an increase in time-to-degree not because students study faster, but rather because students study less. The results indicate that the decision to receive a complementary certificate or a second degree is not affected by the increase. In most periods the estimates are close to zero and insignificant. Only first semester students in the fall 2011 show a weakly significant negative effect. However, the effect is negligibly small.

Finally, the decision to do an exchange semester or to be on leave of absence might also be affected by the raise. Even on exchange, students have to pay the St. Gallen tuition fee. However, on average students complete more credits abroad. Hence, students could potentially go an additional semester on exchange to finish faster. This seems not to be the case as all of the estimates are close to zero and insignificant.

Table 4: Effects on Related Student Behavior

	SS 2011				FS 2011				SS 2012				FS 2012	
	2^{nd} Sem.		4^{th} Sem.		1^{st} Sem.		3^{rd} Sem.		2^{nd} Sem.		4^{th} Sem.		3^{rd} Sem.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
COURSES	-0.51* (0.275) [-0.057]	-0.451 (0.275)	0.406 (0.358)	0.267 (0.361)	0.553** (0.252) [0.139]	0.553** (0.247) [0.147]	-0.73* (0.38) [-0.105]	-0.576 (0.378)	-0.427 (0.271)	-0.322 (0.268)	0.550 (0.363)	0.656* (0.359) [0.065]	0.358 (0.398)	0.615 (0.39)
THESIS			0.153** (0.064) [0.049]	0.179*** (0.064) [0.074]			0.002 (0.015)	0.000 (0.016)			0.034 (0.061)	0.056 (0.061)	-0.010 (0.011)	-0.010 (0.012)
COMPL. COURSES	0.001 (0.012)	-0.001 (0.013)	0.009 (0.008)	0.010 (0.008)	-0.025* (0.015) [-0.001]	-0.023 (0.014)	-0.017 (0.016)	-0.019 (0.017)	-0.014 (0.014)	-0.012 (0.013)	0.013 (0.019)	0.015 (0.018)	0.013 (0.008)	0.013 (0.009)
DROPPED 2^{nd} MAJOR	-0.021 (0.013)	-0.013 (0.01)	-0.002 (0.002)	-0.003 (0.003)			-0.011 (0.008)	-0.004 (0.006)	-0.021 (0.013)	-0.014 (0.011)	0.000 (0)	0.000 (0)	-0.007 (0.007)	-0.007 (0.006)
STARTED 2^{nd} MAJOR	-0.008 (0.013)	-0.008 (0.014)	0.006 (0.008)	0.003 (0.008)			-0.006 (0.008)	-0.005 (0.008)	-0.013 (0.011)	-0.014 (0.011)	-0.002 (0.002)	0.002 (0.002)	-0.008 (0.008)	-0.007 (0.007)
ON EXCHANGE	-0.023 (0.019)	-0.018 (0.017)	0.053 (0.036)	0.055 (0.037)			0.018 (0.065)	0.046 (0.063)	-0.024 (0.019)	-0.022 (0.017)	0.055 (0.037)	0.053 (0.037)	-0.023 (0.067)	0.008 (0.065)
ON LEAVE	0.005 (0.007)	0.005 (0.007)	0.031** (0.016) [0.005]	0.036** (0.017) [0.008]	-0.002 (0.003)	-0.002 (0.003)	0.056*** (0.021) [0.022]	0.06*** (0.022) [0.023]	0.011 (0.011)	0.009 (0.011)	0.014 (0.012)	0.018 (0.012)	0.004 (0.012)	0.004 (0.012)
Observations	1368	1368	1291	1291	1308	1308	1364	1364	1318	1318	1275	1275	1318	1318
X		✓		✓		✓		✓		✓		✓		✓

Robust standard errors in round parentheses. One-sided 95% confidence bound as suggested in Imbens and Manski (2004) in squared parentheses.
* $p<0.10$, ** $p<0.05$, *** $p<0.010$
COURSES: number of courses in semester; THESIS: binary indicator whether students finished Bachelor thesis; COMPL. COURSES: number of complementary courses visited in semester; DROPPED/STARTED 2^{nd} MAJOR: binary indicator whether student dropped or started a second major in semester; ON EXCHANGE: binary indicator whether student is on exchange; ON LEAVE: binary indicator whether students is on leave of absence. Controls include age, age squared, gender, assessment grade, assessment grades squared, whether the students needed two attempts to finish the assessment, whether the student majors in two subjects at the beginning of the bachelor phase, and whether the student is not a native German speaker. Spring and Fall Semesters 2010 control semesters, respectively. Blanc cells are due to missing variation in the dependent variable.

As for the leave of absence, students can take a leave of absence for an internship, motherhood or other family obligations, in case of sickness or other hardship, to finish their thesis, and because of military obligations. On leave, the fee is reduced significantly to CHF 69.5. However, students cannot earn credits. The only exception is the thesis, which can be written while on leave. For instance, the raise could cause students to postpone an internship until after graduating. The behavior would lead to an increase in the probability to graduate on time and in the credits. However, the effect would be driven by a change in the student's priorities and not by an increase of the students effort.

Surprisingly, the only significant effects are positive. The probability to be on leave is increased for fourth semester students in the spring 2011 and third semester students in the fall 2011. For the fourth semester students the increase works against the results on the probability to graduate on time an credits, i.e. there are positive effects despite the increase in students that are on leave. Roughly two credits of the negative effect for third semester students, however, can be attributed to the increase in students on leave. It is impossible to speed up studies by being on leave. However, one possible explanation of this development might be that students choose to do a paid internship in order to finance tuition and thus take a semester on leave. As the latter explanation seems a bit far fetched, it is more likely that an unrelated shock is responsible. Thus, the negative effect for third semester students should be interpreted with caution.

8 Alternative Explanations

Reforms that were implemented at the same time constitute a threat to the identification strategy. Unfortunately, both bachelor programs as well as master programs were subject to reforms during the considered periods. While none of these reforms distinguished between Swiss and foreign students, they might have had an differential impact on each group. For instance, these differences could arise because of distinct preferences. In the following, I try to assess how much of the observed behavior can be attributed to these reforms.

One particular reform on the master level is a concern. The prestigious Master in Banking an Finance (MBF) introduced stricter admission criteria in the fall semester 2011 for external students and in the fall semester 2012 for internal students. That is, until the fall semester 2011 internal students were granted unlimited admission. Students that wanted to be sure to enroll in the program, had to graduate until the spring semester 2011. Again the reform targeted Swiss and Foreign students equally. However, foreign students might have reacted differently if they were for instance more ambitious. In order to find out how much of the positive anticipation effect could be attributed to the change in admission criteria, I estimate Equation (11) on a master program indicator for fourth semester students in the spring 2011. This indicator takes one if the student graduated in the fourth semester and started that particular master in the fifth semester and zero otherwise.

Table 5: Master Choice of Fourth Semester Graduates in Spring 2011

	(1)			(2)		
Banking and Finance	0.063*	(0.034)	[0.008]	0.07**	(0.034)	[0.013]
International Affairs and Governance	-0.003	(0.019)		0.001	(0.019)	
Quantitative Economics and Finance	-0.026**	(0.012)	[-0.005]	-0.021*	(0.011)	[-0.003]
Business Innovation	0.020	(0.017)		0.020	(0.017)	
Accounting and Finance	-0.041*	(0.023)	[-0.003]	-0.038	(0.023)	
Marketing, Services and Communication Management	0.004	(0.016)		0.005	(0.016)	
Strategy and International Management	0.015	(0.01)		0.017	(0.012)	
Economics	0.010	(0.012)		0.010	(0.013)	
Law	-0.010	(0.01)		-0.007	(0.011)	
Law and Economics	0.029*	(0.015)	[0.004]	0.034**	(0.015)	[0.009]
Observations	1291			1291		
X				✓		

Robust standard errors in round parentheses. One-sided 95% confidence bound as suggested in Imbens and Manski (2004) in squared parentheses.
* p<0.10, ** p<0.05, *** p<0.010
Fourth semester students in the spring semester 2011 are considered. Dependent variable: indicator that student graduated in fourth semester and started respective master program in fifth semester. Controls include age, age squared, gender, assessment grade, assessment grades squared, whether the students needed two attempts to finish the assessment, whether the student majors in two subjects at the beginning of the bachelor phase, and whether the student is not a native German speaker. Spring 2010 control semester.

Table 5 shows the results for each master program of the University of St. Gallen. Column (1) contains the results without covariates and Column (2) contains the results conditional on students characteristics. There is a notable and significant effect on the probability to begin a MBF in the fifth semester. The other programs do not show similar changes in the enrollment rates. Hence, most of the additional graduates chose to continue in the MBF program. In fact, up to 6.3%-points of the 9.7%-points increase in the probability to graduate can be attributed to MBF students. Therefore, it is possible that the change in the admission criteria rather than the anouncement of the raise in fees drives the positive effect for fourth semester students in the spring 2011.

In an attempt to disentangle the fee and the admissions effect, I reestimate Equation (11) for fourth semester students in the spring 2011 without those students that graduated and went on to the MBF. This approach relies on the assumption that none of the students graduated because of the raise in tuition fees and then chose to enroll in the MBF. If that is not the case, the coefficients will be downward biased. Less students who would have graduated because of the raise will be in sample. The downward bias will be even more severe if students alter their master choice because of the raise. For example, students might opt for a program with a higher return (e.g. the MBF) as tuition fees are higher.

Table 6 shows the results for fourth semester students who did not graduate and started a MBF in spring 2011. Now, the effect on the probability to graduate on time is smaller than in the main specification, 5.6% without covarites and 8.9% with covariates. In the main specification, the corresponding estimates are 9.7% and 13.1%, respectively. The coefficients are not siginificant at any conventional level. However, conditional on covariates the estimate would be significant at the 15.9% level. The effects on credits earned are significant at the 5% level and almost identical as in the unrestricted sample, i.e. around 3.5 credits. Similarly, the estimated effect on the grades remains close to zero and insignificant as in the main specification. Thus, while the reform of the admission criteria might explains some of the effect on the probability to graduate, it cannot exlpain the whole story. The annoucement of the raise still seems to have an effect even if MBF students are not considered.

Table 6: Effects for Fourth Semester Students in Spring 2011 without MBF Starters

	PROB. TO GRADUATE		CREDITS		GRADES	
	(1)	(2)	(3)	(4)	(5)	(6)
FOREIGN*POST	0.056	0.089	3.693**	3.492**	-0.079	-0.020
	(0.063)	(0.063)	(1.605)	(1.632)	(0.074)	(0.064)
			[1.053]	[0.807]		
Observations	1216	1216	1216	1216	1201	1201
X		✓		✓		✓

Robust standard errors in round parentheses. One-sided 95% confidence bound as suggested in Imbens and Manski (2004) in squared parentheses.
* $p<0.10$, ** $p<0.05$, *** $p<0.010$
Fourth semester students in the spring semester 2011 are considered. Students that graduate in the fourth semester and start a MBF in the fifth are discarded. PROB. TO GRADUATE: binary indicator if student graduates in fourth semester, CREDITS: earned credits in a given semester, GRADES: weighted average grade in a given semester. Controls include age, age squared, gender, assessment grade, assessment grades squared, whether the students needed two attempts to finish the assessment, whether the student majors in two subjects at the beginning of the bachelor phase, and whether the student is not a native German speaker. Spring 2010 control semester.

First, consider the reforms to the bachelor programs. Four out of five programs extensively changed the core curriculum. If students did not finish the core curriculum until the spring semester 2011, all of their completed courses would be transfered to the new regime. Courses that were changed or replaced would be accredited to the new courses. If the number of credits differed between the new an the old, courses were mostly upgraded but also in a fewer cases downgraded. In the most extreme scenarios, students could loose 2 or earn 6 credits according to the course choice. Students could have anticipated this change by either accelerating or delaying their studies depending on whether they would profit or not. Note, these rules applied similarly to Swiss and foreign students. Hence, the only way this would bias the estimates would be if the groups would react differently. That could be the case, if groups differ in their course choice.

Consider the fourth semester students. The core curriculum does not contain the bachelor thesis. Hence, the only way that students can either avoid the reform or be deliberately affected by the reform is by adjusting their course load. However, the reduction in time-to-degree and the increase in credits is due to an increase in students that finish their bachelor thesis and not due to an increase in courses. See Section 7. Thus, it is unlikely that differences in strategic behavior drive the results in the spring semester 2011. Furthermore, the biggest bachelor program, business administration, was not reformed. That is, business students should not show any strategic behavior. Thus, if I reestimate the main specifications for business students and find similar effects, the effects are most likely not caused by an anticipation of the curriculum reform. On the downside, I loose around half of the observations by restricting the sample to students that are exclusively enrolled into business administration. The results are qualitatively similar, but lack precision. See Appendix A.6.

At the University of St. Gallen, early enrollment into master programs was possible until recently. That is, if students were short at most five credits from finishing the bachelor's degree, the students could simultaneously enroll into a master program. Hence, students could to a certain extent strategically postpone their graduation without jeopardizing their total time-to-master's-degree. This possibility raises one question. Is the observed effect for fourth semester students driven by students that decide not enroll into a master program in St. Gallen who would have otherwise done so? Such a behavior might be caused by the higher fee or reforms on the master level in 2011 that appealed differently to Swiss and foreign bachelor students. In both cases, students that would have opted for a master's degree in St. Gallen but chose not to would not have any reason to postpone graduation. In that case, the observed effect on time-to-degree, would merely reflect a decrease in the popularity of the master programs at the University of St. Gallen.

Figure 7 depicts the shares of students that opt for an internal master program within seven semesters and of students that finished their bachelor after starting the master program. In order to observe the affected bachelor cohorts 2009 and 2010, I restrict the sample to students that graduated within six semesters. Throughout the bachelor cohorts the share of students that enrolled into both programs at the University of St. Gallen is high and stable, approximately 80% of Swiss and 60% of foreign students. There is no notable deviation from that trend for the cohort that shows the increase in the probability to graduate on time. Hence, neither the raise in tuition fees nor the reforms

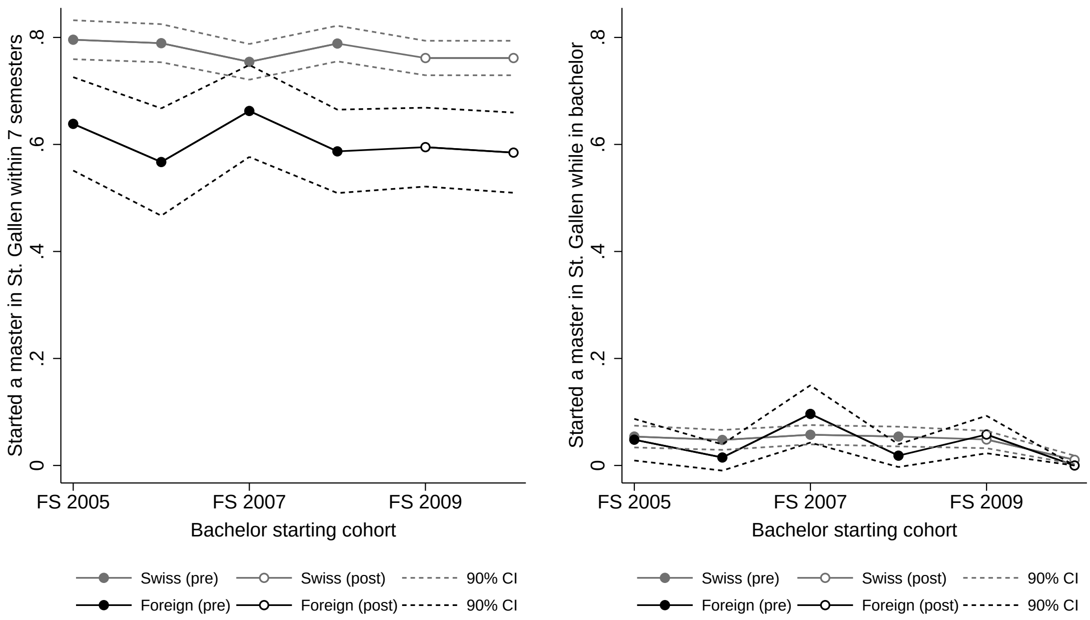

Figure 7: Internal Bachelor-Master Transition

on the master level impact the decision to stay in St. Gallen on average. Moreover, the incidence of students that start the master program while finishing the bachelor is relatively low, below 10%. Again, there is no notable deviation of the affected cohorts. Thus, a decrease in the attractiveness of the master programs and a resultung reduction in strategic delays is unlikely behind he main results.

Finally, Table 7 shows the results of the placebo tests for the main outcomes. Placebo-tests are one option to verify whether other factors impact Swiss and foreign students equally in the periods before the treatment. Equation (11) is separately estimated for spring and fall semesters 2009 and 2010 as post-treatment periods. Periods in which tuition remained constant. Since there is no actual treatment, the true treatment effect is zero. Under the assumption of common trends, the DiD estimate should also be zero. Only if unobserved factors influence Swiss and foreign students differently, the estimated effect will be different from zero as these factors will be picked up in the estimate. The respective pre-treatment periods are the prior spring and fall semesters, respectively. The results without covariates are reported in the rows "Unconditional" and with covariates in the rows "Conditional".

Consider the probability to graduate on time and credits. In all periods considered, the effects are small and not significantly different from zero. The inclusion of covariates does not change the estimates much. In line with the graphical analysis, these results

Table 7: Placebo Tests

	SS 2009		FS 2009		SS 2010		FS 2010	
	2^{nd} Sem.	4^{th} Sem.	1^{st} Sem.	3^{rd} Sem.	2^{nd} Sem.	4^{th} Sem.	1^{st} Sem.	3^{rd} Sem.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel 1 - PROB. TO GRADUATE								
Unconditional								
FOREIGN*POST		0.054				-0.042		
		(0.074)				(0.065)		
Observations		1162				1262		
Conditional								
FOREIGN*POST		0.052				-0.059		
		(0.071)				(0.064)		
Observations		1162				1262		
Panel 2 - CREDITS								
Unconditional								
FOREIGN*POST	0.854	-1.857	0.897	-0.007	1.307	-0.934	-1.077	0.841
	(1.357)	(2.113)	(1.250)	(1.468)	(1.110)	(1.679)	(1.140)	(1.319)
Observations	1270	1162	1301	1267	1297	1262	1373	1296
Conditional								
FOREIGN*POST	0.746	-2.075	0.773	0.087	1.294	-0.863	-0.523	0.533
	(1.330)	(2.133)	(1.208)	(1.414)	(1.122)	(1.703)	(1.116)	(1.301)
Observations	1270	1162	1301	1267	1297	1262	1373	1296
Panel 3 - GRADES								
Unconditional								
FOREIGN*POST	0.081	0.020	-0.074	0.252***	-0.110	0.056	-0.059	-0.196***
	(0.071)	(0.074)	(0.058)	(0.074)	(0.071)	(0.068)	(0.059)	(0.068)
Observations	1252	1143	1285	1243	1278	1245	1354	1270
Conditional								
FOREIGN*POST	0.050	0.014	-0.016	0.213***	-0.055	0.017	0.010	-0.151***
	(0.055)	(0.060)	(0.039)	(0.057)	(0.056)	(0.055)	(0.040)	(0.056)
Observations	1252	1143	1285	1243	1278	1245	1354	1270

Robust standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.010
PROB. TO GRADUATE: binary indicator if student graduates in fourth semester, CREDITS: earned credits in a given semester, GRADES: weighted average grade in a given semester. Controls include age, age squared, gender, assessment grade, assessment grades squared, whether the students needed two attempts to finish the assessment, whether the student majors in two subjects at the beginning of the bachelor phase, and whether the student is not a native German speaker. The prior spring or fall semester is the control period, respectively.

suggest that unobserved factors influence the behavior of both groups equally. As for the grades, the DiD estimate is significant for third semester students in the fall semester 2009 and 2010. The coefficients are smaller but remain significant after controlling for changes in the composition. These findings are in line with the graphical analysis, as they capture the spike in the weighted grades of foreigners in fall semester 2009. In the remaining periods the estimates are close to zero and insignificant. Overall, these results suggest that in the four prior semesters, both groups followed a similar trend.

9 Discussion

This study analyses the effects of an unexpected increase in tuition fees on student's time-to-degree and performance. Students experienced the raise in different semesters. Some students were close to graduation while others were affected in the middle or at the beginning of there studies. As a result this study provides an insight into two policy questions. First, how do students react to an increase in fees at the end of the standard period of studies. And second, how do students behave if tuition fees are high during their studies.

Garibaldi, Giavazzi, Ichino, and Rettore (2011) provide evidence that an increase in tuition fees after the standard period of studies is an effective measure to reduce time-to-degree. The reasoning behind this argument is that students have an incentive to finish on time to avoid these additional costs. Recently, such policies have found more attention. After abolishing general tuition fees, several German states introduced tuition fees for long term students. Similarly, eight of 21 Austrian universities introduced tuition fees for students past the standard period of studies. Even the University of St.Gallen will further increase fees for these students in 2014. This study cannot support the finding by Garibaldi, Giavazzi, Ichino, and Rettore (2011). The positive effect after the announcement seems to be at least partially driven by the reform of admission criteria of the MBF. After the raise is implemented, students do not change their behavior.

Even though many students study longer than intended by the university,[18] it is not necessarily clear whether the policy maker should try to alter time-to-degree, since not much is known about the optimal learning time (Garibaldi, Giavazzi, Ichino, and Rettore, 2011). For instance, a reduction in time-to-degree could lead to an increase in the information to be learned in the available time, in other words, an increased study

[18]In the US, only 39.4% of the college graduates of the high school class of 1992 recieved their bachelor's degree after four years while still 53.1% of the high school class of 1972 graduated on time (Bound, Lovenheim, and Turner, 2012). Brunello and Winter-Ebmer (2003) report lower but still notable shares for 26 economics and business departments in ten European countries. For details about the survey see Brunello, Lucifora, and Winter-Ebmer (2004).

intensity. Research focusing mainly on secondary education suggests that a higher study intensity affects student's achievement negatively (Agüero and Beleche, 2013; Lavy, 2012; Pischke, 2007).[19] If these findings apply also to higher education, a reduction in time-to-degree might not be desirable.

Garibaldi, Giavazzi, Ichino, and Rettore (2011) propose three reasons, why policy action might be positive: publicly subsidized higher education, congestion externalities, and peer effects. However, the first two reasons only apply if students actually study more in total or make use of university facilities and subsidies. From a macro perspective one reasons seems to be pressing as well. The upcoming retirement of the baby boom cohorts as well as low fertility rates pose an enormous challenge to the labor supply in the developed world (Lutz, O'Neill, and Scherbov, 2003). A reduction in time that students spend in the educational system could be one way to counteract this decline (Lutz and Skirbekk, 2005).[20]

Now consider the second policy question, how do fees affect students during their studies. Again the study suggests that students do not react pronouncedly. While in some semesters, there are some significant negative effects on credit accumulation and grades for students that experience the raise during their studies, they are not economically significant. Furthermore, inference problems suggest that the significance levels should not be over-emphasized. One lesson is however, that if effects exist tuition fees before the standard period of studies are rather harmful than beneficial to student's achievement. Second and third semester students earn significantly less credits and first semester students seem to suffer a decrease in their performance.

One limitation of this study is the external validity. If the assumptions stated in Section 3 hold, the estimated effect represents the average treatment effect for those students that experienced the high increase in tuition. These students are foreigners. Hence the question is: do foreigners represent an interesting population or do we learn something that can be extrapolated to more interesting groups?

As for the first part of that question, foreigners in St.Gallen are pre-selected with the admissions test and have to be able to afford studying abroad. Hence, they are possibly more determined and better informed. Nonetheless, this is most likely also true for foreigners at other universities. As the focus of university politics shifts more towards foreign students, they become an more interesting population. One the one hand, universities increasingly try to attract international students. On reason for this behavior is that the share of international students is one criteria in many university rankings, e.g. the Times Higher Education Ranking. On the other hand, international policies to foster

[19] Oosterbeek and Webbink (2007) and Skirbekk (2006) find no effects.

[20] Besides the immediate inflow to the labor force, a reduction in education time might lead to earlier first birth (Black, Devereux, and Salvanes, 2008; Cygan-Rehm and Maeder, 2013; Silles, 2011)

integration of education and labor markets has increased student and labor mobility in the past decades. Thus, if these migration flows are unbalanced, considerations about free-riding behavior and optimal financing of higher education arise.[21] Hence, universities as well as public policy makers should be interested in the effects of financing policies on international students.

As for the second part of the question, it is not clear to which extent the results can be extrapolated to other students. The only comparison results that exist are those of Garibaldi, Giavazzi, Ichino, and Rettore (2011). They find that an 1000 Euro increase in fees raises the probability of students in their last semester to graduate on time by 5.2% points, which represents an increase of 26% with respect to the initial level. I do not find a significant effect. One reason for this difference could be that this study provides an ATET for foreigners, while Garibaldi, Giavazzi, Ichino, and Rettore (2011) identify only the intention-to-treat effect. Furthermore, this discripancy might be the result of differences in the students' perception of higher education. On the one hand, if a university eductaion is an investment, an increase in tution fees should not result in a large effect on the probablilty to graduate, as the opportunity cost of forgone earnings in the labor marked are much larger. On the other hand, if university education provides direct utlity, the increase in tuition fees is simply a price increase of a consumption good. And as the price of a consuption good goes up, the demand decreases.

10 Conclusion

In this study, I provide new evidence of the college cost effect on time-to-degree and students performance. More precisely, I estimate the effect of a raise in tuition fees on the probability to graduate on time, credit accumulation, and the weighted grade average. I identify the effect using an unique policy at the University of St. Gallen, Switzerland. Tuition was raised differently for Swiss and foreign students. This setting allows me to employ an DiD strategy. That is, I estimate the impact of time varying unobserved factors using Swiss students as control group.

Furthermore, I formally discuss DiD in a setting with multiple treatments when the non-treatment state is not observed in the post-treatment period. I can only identify the effect of the high increase in comparison to the low raise under the assumptions of parallel trends and, additionally, homogeneous effect of the low increase. However, I propose an alternative interpretation in case of heterogeneous effects: the lower bound in magnitude of the effect of the high increase compared to no increase. The assumptions for the second interpretation are fulfilled if the both both treatments have the same effect

[21] See for example Demange, Fenge, and Uebelmesser (2012).

direction but the high increase affects foreign students more than the low raise affects the Swiss students. This condition more reasonable that homogeneous effects.

I find at best modest effects of the increase on student achievement. Results suggest a positive anticipation effects on the probability to graduate and the credit accumulation for students at the end of their studies. However, part of the effect can be explained by a simultaneous reform of the admission criteria on the master level. After the higher fees are implemented the effect on the probability to graduate disappears. These increased effort levels do not affect the grade average of the students. There is weak evidence of negative effects on credit accumulation and grades for students further away from graduation.

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A Appendix

A.1 Bound for the Effect High vs. Low Increase

It is possible to derive a lower bound of $ATET_1^{HL}$ in absolute value under the following assumption:

$$\begin{aligned} & E[Y_1^L|D=H] - E[Y_0^0|D=H] \leq E[Y_1^L|D=L] - E[Y_0^0|D=L] \\ \text{if } \; & E[Y_1^H|D=H] - E[Y_0^0|D=H] > E[Y_1^L|D=L] - E[Y_0^0|D=L], \end{aligned} \tag{12}$$

$$\begin{aligned} & E[Y_1^L|D=H] - E[Y_0^0|D=H] \geq E[Y_1^L|D=L] - E[Y_0^0|D=L] \\ \text{if } \; & E[Y_1^H|D=H] - E[Y_0^0|D=H] < E[Y_1^L|D=L] - E[Y_0^0|D=L]. \end{aligned} \tag{13}$$

Assumption (12) states that if foreigners the DiD estimate is positive, the change between pre- and post-treatment period for foreigners would have been smaller than or equal to for Swiss. Similarly, if the DiD estimate is negative, (13) states that the development of foreigners must be bigger than or equal to the development of the Swiss. These assumptions are fulfilled if the Swiss react in the same direction but stronger to the low increase than the foreigners. However, (12) and (13) are broader.

The proof is similar to the one given in 3. Consider the first case. By solving (12) for the missing counterfactual and plugging it into (1), we get

$$\begin{aligned} ATET_1^{HL} \geq & E[Y_1^H|D=H] - E[Y_0^0|D=H] - \left[E[Y_1^L|D=L] - E[Y_0^0|D=L]\right] \\ = & \underbrace{E[Y_1|D=H]}_{identified} - \underbrace{E[Y_0|D=H]}_{identified} - \left[\underbrace{E[Y_1|D=L]}_{identified} - \underbrace{E[Y_0|D=L]}_{identified}\right] \\ > & 0. \end{aligned}$$

Similarly, for the second case:

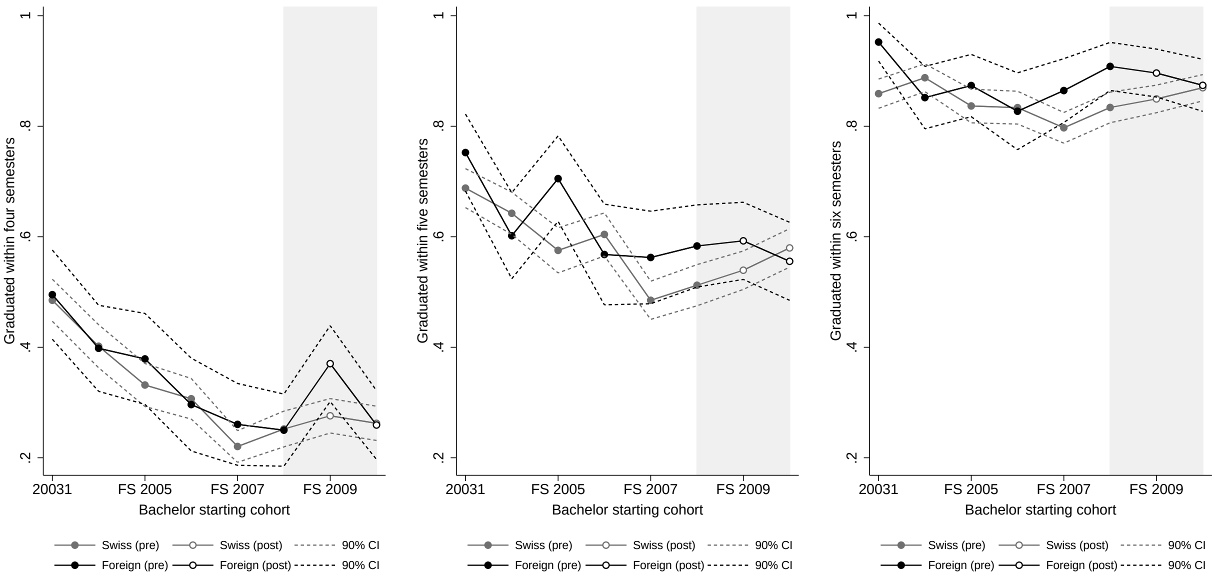
$$ATET_1^{HL} \leq E[Y_1|D=H] - E[Y_0|D=H] - [E[Y_1|D=L] - E[Y_0|D=L]] < 0.$$

A.2 Number and Variable Means of Excluded Students

	N		Female		German Speaker		Age	
	Swiss	Foreign	Swiss	Foreign	Swiss	Foreign	Swiss	Foreign
No Assessment in St.Gallen								
2008	20	17	0.35	0.235	0.95	0.882	28.9	23
2009	27	15	0.444	0.333	0.778	0.6	25.185	22.733
2010	19	12	0.474	0.167	0.842	0.75	25.105	22.667
2011	21	15	0.381	0.333	0.762	0.733	27.81	22.867
Interrupted Enrollment								
2008	2	0	0.5	.	1	.	20.5	.
2009	4	0	0.5	.	1	.	22.5	.
2010	2	0	0	.	1	.	22.5	.
2011	0	0	.	.	.	.	.	.
Change in Nationality Status								
2008	0	0	.	.	.	.	.	.
2009	0	1	.	0	.	1	.	21
2010	1	0	0	.	0	.	21	.
2011	2	1	0	0	1	1	19.5	21
Started Bac. during Ass.								
2008	64	10	0.375	0.4	0.516	0.8	21.484	20.4
2009	80	27	0.362	0.111	0.588	0.889	21.45	21.296
2010	92	35	0.326	0.086	0.478	0.771	21.152	20.829
2011	90	27	0.4	0	0.533	0.852	21.078	21.259
Started Bac. in Spring								
2008	6	0	0.5	.	0.833	.	21.5	.
2009	8	2	0.125	0	0.75	1	22.5	23
2010	4	2	0.75	0	1	1	22.5	21
2011	8	2	0.75	0	0.75	1	21.75	22.5
Total								
2008	92	27	0.38	0.296	0.641	0.852**	23.076	22.037
2009	119	45	0.37	0.178***	0.655	0.8*	22.403	21.844
2010	118	49	0.356	0.102***	0.559	0.776***	21.856	21.286**
2011	121	45	0.413	0.111***	0.595	0.822***	22.264	21.844

*, **, *** significantly different at p<0.10, p<0.05, p<0.0100
The foreigner nationality status refers to first bachelor semester.

A.3 Probabilities to Graduate within a Given Semester by Cohort



A.4 Attrition Due to Drop-Out, Early Graduation, and Zero Credits

Reason for Attrition		N		Share of Bac. Cohort		Female		Age	
Bac. Cohort	Semester	Swiss	Foreign	Swiss	Foreign	Swiss	Foreign	Swiss	Foreign
Dropout									
FS 2008	1	1	0	0.002	0	0	.	21	.
	2	1	0	0.002	0	0	.	22	.
	3	1	0	0.002	0	0	.	20	.
	4	4	0	0.008	0	0.25	.	21.5	.
FS 2009	1	2	1	0.004	0.007	1	1	19.5	19
	2	1	0	0.002	0	1	.	28	.
	3	1	0	0.002	0	0	.	23	.
	4	1	0	0.002	0	0	.	24	.
FS 2010	1	2	0	0.004	0	0	.	23.5	.
	2	2	2	0.004	0.015	1	0	22.5	21.5
	3	0	0	0	0	.	.	.	.
	4	0	0	0	0	.	.	.	.
FS 2011	1	0	0	0	0	.	.	.	.
	2	0	0	0	0	.	.	.	.
	3	0	0	0	0	.	.	.	.
Early Graduation									
FS 2008	2	0	0	0	0	.	.	.	.
	3	2	0	0.004	0	1	.	21.5	.
FS 2009	2	0	0	0	0	.	.	.	.
	3	1	0	0.002	0	0	.	21	.
FS 2010	2	0	0	0	0	.	.	.	.
	3	0	2	0	0.015	.	0	.	20.5
FS 2011	2	0	0	0	0	.	.	.	.
	3	0	0	0	0	.	.	.	.
0 Credits									
FS 2008	1	2	0	0.004	0	0.5	.	21	.
	2	7	1	0.014	0.008	0.143	0	21	21
	3	8	2	0.016	0.017	0.5	0	20.5	22
	4	5	1	0.01	0.008	0.2	0	21.2	22
FS 2009	1	11	4	0.02	0.03	0.273	0.25	21.545	20.5
	2	10	0	0.018	0	0.1	.	21.7	.
	3	13	2	0.023	0.015	0.538	0	20.308	20.5
	4	6	2	0.011	0.015	0.167	0	21.833	21.5
FS 2010	1	5	0	0.009	0	0.2	.	22	.
	2	6	0	0.011	0	0.5	.	21.167	.
	3	10	8	0.018	0.059	0.3	0.125	20.8	21
	4	4	1	0.007	0.007	0.25	0	21	21
FS 2011	1	2	0	0.004	0	0	.	21	.
	2	6	2	0.012	0.016	0	0	20.833	20
	3	12	2	0.024	0.016	0.083	0.5	20.75	21.5

A.5 Comparison of OLS to alternative Estimation Methods

For the semi-parametric DiD, I use a propensity score matching approach. That is, I match the treated observations in the post-treatment period separately to the treated in the pre-treatment periods, the controls in the pre-treatment periods, and the controls in the post-treatment periods. For each of these three comparisons, I calculate a separate propensity score. I use the distance-weighted radius matching with bias adjustment as proposed in Lechner, Miquel, and Wunsch (2011). The radius is determined as three times the 90% quantile of the distance distribution between pairs of treated and control observations. Furthermore, I apply a trimming rule that drops each control obsevation if its share of all weights exceeds 4%. See Huber, Lechner, and Steinmayr (2012) for an assessment of the above parameters.

I determine the support of all three comparisons simultaneously. That is, in each comparison all the observations are considered. Only when the support in all three comparisons is determined those observations that are off support are discarded. The support is determined in three steps. First, I exclude all observations with values in any covariate either below the minimum or above the maximum of the other group. Practically, this step only applies to age and the assessment grade. Second, I compute the three propensity scores and exclude those treated observations with larger scores than the maximum of the control observations. Third, I apply trimming rule mentioned above. In order to be able to compare the the semi-parametric DiD to the linear and non-linar DiD, I also estimate the linear and non-linear DiD with only those observation that are on support.

The standard errors of the semi-parametric and the non-linear DiD are calculated by bootstrapping the DiD estimate. For each method, I use 800 replications. I do not include the indicator of whether the student is a native German speaker, as it leads to failure of convergence in some of the bootstrap samples. For comparison I estimate also the linear model without that indicator.

Table 8: Comparison of OLS to Semi-Parametric and Non-Linear DiD

	SS 2011			SS 2012	
SEMI-PARAMETRIC	0.134	(0.115)		0.074	(0.111)
Observations	1184			1200	
OLS	0.131**	(0.064)	[0.026]	0.021	(0.061)
Observations	1291			1275	
OLS ON SUPPORT	0.129*	(0.067)	[0.019]	0.027	(0.063)
Observations	1184			1200	
NON-LINEAR	0.123*	(0.074)	[0.001]	0.024	(0.065)
Observations	1291			1275	
NON-LINEAR ON SUPPORT	0.146*	(0.078)	[0.018]	0.066	(0.066)
Observations	1184			1200	
X	✓			✓	

OLS: Robust standard errors in round parentheses; SEMI-PARAMETRIC, NON-LINEAR: bootstrapped standard errors (800 replications) in round parentheses. One-sided 95% confidence bound as suggested in Imbens and Manski (2004) in squared parentheses.
* p<0.10, ** p<0.05, *** p<0.010
Dependent variable is binary indicator if student graduates in fourth semester. Controls include age, age squared, gender, assessment grade, assessment grades squared, whether the students needed two attempts to finish the assessment, and whether the student majors in two subjects at the beginning of the bachelor phase. Spring and Fall Semesters 2010 control semesters, respectively.

Table 9: Comparison of OLS to Semi-parametric DiD

	SS 2011				FS 2011				SS 2012				FS 2012	
	2^{nd} Sem.		4^{th} Sem.		1^{st} Sem.		3^{rd} Sem.		2^{nd} Sem.		4^{th} Sem.		3^{rd} Sem.	
	CREDITS	GRADES	CREDITS	GRADES	CREDITS	GRADES	CREDITS	GRADES	CREDITS	GRADES	CREDITS	GRADES	CREDITS	GRADES
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
SEMI-PARAMETRIC	-4.282**	0.182	2.070	-0.041	1.868	-0.135	-7.234***	-0.122	-2.554	-0.077	7.59**	0.058	-3.404	0.086
	(1.924)	(0.112)	(2.88)	(0.11)	(1.546)	(0.087)	(2.393)	(0.115)	(2.079)	(0.119)	(3.233)	(0.095)	(2.354)	(0.126)
	[-1.117]						[-3.297]				[2.272]			
Observations	1264	1242	1184	1173	1249	1244	1261	1212	1171	1144	1200	1195	1171	1131
OLS	-1.883*	0.037	3.466**	-0.003	1.185	-0.157***	-3.288**	-0.024	-1.591	-0.012	2.732*	0.053	0.206	0.008
	(1.006)	(0.066)	(1.542)	(0.059)	(0.978)	(0.051)	(1.426)	(0.061)	(1.014)	(0.06)	(1.566)	(0.049)	(1.345)	(0.067)
	[-0.227]		[0.929]			[-0.073]	[-0.942]				[0.157]			
Observations	1368	1348	1291	1276	1308	1301	1364	1330	1318	1298	1275	1264	1318	1289
OLS ON SUPPORT	-2.062**	0.037	3.291**	0.011	1.289	-0.16***	-3.159**	-0.013	-1.470	-0.022	2.471	0.060	0.005	-0.002
	(1.017)	(0.069)	(1.526)	(0.062)	(0.975)	(0.053)	(1.465)	(0.068)	(1.062)	(0.062)	(1.635)	(0.051)	(1.382)	(0.072)
	[-0.388]		[0.78]			[-0.073]	[-0.749]							
Observations	1264	1242	1184	1173	1249	1244	1261	1212	1171	1144	1200	1195	1171	1131
X	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

OLS: Robust standard errors in round parentheses; SEMI-PARAMETRIC: bootstrapped standard errors (800 replications) in round parentheses. One-sided 95% confidence bound as suggested in Imbens and Manski (2004) in squared parentheses.
* p<0.10, ** p<0.05, *** p<0.010
CREDITS: earned credits in a given semester, GRADES: weighted average grade in a given semester. Controls include age, age squared, gender, assessment grade, assessment grades squared, whether the students needed two attempts to finish the assessment, and whether the student majors in two subjects at the beginning of the bachelor phase. Spring and Fall Semesters 2010 control semesters, respectively.

A.6 DiD Estimates for Main Outcomes for Business Students

	SS 2011				FS 2011				SS 2012				FS 2012	
	2^{nd} Sem.		4^{th} Sem.		1^{st} Sem.		3^{rd} Sem.		2^{nd} Sem.		4^{th} Sem.		3^{rd} Sem.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
PROB. TO GRADUATE			0.164**	0.195**							0.075	0.094		
			(0.081)	(0.082)							(0.074)	(0.074)		
			[0.03]	[0.06]										
Observations			787	787							785	785		
CREDITS	-1.354	-1.384	2.645	2.691	1.912	2.199*	-0.293	-0.469	-0.988	-0.786	-1.432	-1.232	1.184	1.028
	(1.132)	(1.142)	(1.715)	(1.716)	(1.167)	(1.153)	(1.589)	(1.583)	(1.265)	(1.271)	(1.665)	(1.675)	(1.616)	(1.646)
						[0.302]								
Observations	843	843	787	787	802	802	846	846	808	808	785	785	814	814
GRADE	0.047	0.025	0.031	0.096	-0.238***	-0.196***	0.009	-0.005	-0.063	-0.066	0.010	0.036	0.048	0.040
	(0.086)	(0.076)	(0.076)	(0.061)	(0.088)	(0.073)	(0.094)	(0.082)	(0.097)	(0.085)	(0.073)	(0.063)	(0.094)	(0.08)
					[-0.093]	[-0.076]								
Observations	833	833	777	777	797	797	828	828	795	795	779	779	795	795
X		✓		✓		✓		✓		✓		✓		✓

Robust standard errors in round parentheses. One-sided 95% confidence bound as suggested in Imbens and Manski (2004) in squared parentheses.
* $p<0.10$, ** $p<0.05$, *** $p<0.010$
PROB. TO GRADUATE: binary indicator if student graduates in fourth semester, CREDITS: earned credits in a given semester, GRADES: weighted average grade in a given semester. Controls include age, age squared, gender, assessment grade, assessment grades squared, whether the students needed two attempts to finish the assessment, whether the student majors in two subjects at the beginning of the bachelor phase, and whether the student is not a native German speaker. Spring and Fall Semesters 2010 control semesters, respectively. Only students that major exclusively in business administration are considered.