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The study pace among college students before and after a student aid reform: some Swedish results*

by

Daniel Avdic[a] and Marie Gartell[b]

Abstract

In 2001, the Swedish system of student aid for college students was substantially reformed; the grant-share of the total aid was increased, students were allowed to earn more without a reduction in student aid, and the repayment schedule of the loans was significantly tightened. In this paper, we examine the effects of the reform on individual study efficiency, measured as the number of credit points achieved each semester. We use all program students with a first registration at a Swedish college between 1995 and 2001(before the reform) and estimate a linear regression model including individual fixed effects. There is a slightly positive and significant effect of the reform on the aggregate level. However, dividing the sample conditionally on the parental educational level reveals that the individual study efficiency has increased only for students from a strong academic background. In other words, the relative study efficiency has decreased for students from a weak academic background. The different results between students from different parental backgrounds appear to be related to the reallocation of time between work and studies.

Keywords: study efficiency; time-to-graduation; university education; student aid
JEL-codes: I2

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1 Introduction

Most OECD countries provide financial aid for college students, with the common goal to increase college attendance and completion (OECD 2008). The Swedish system of student aid consists of two parts: loans and grants. All students admitted to higher education are eligible for student aid independent of their parental background.

In 2001, the Swedish system of student aid went through its greatest reform since its inception.[1] In short, the government increased the share of grants, allowed students to earn considerably more income while in college without a reduction in student aid and significantly tightened the rules for repayment of the loans. However, the total amount of student aid was unchanged. The overall objective of the reform was to create a self-supporting system by requiring all students, to the greatest possible degree and according to government proposition (prop. 1999/2000:10), to repay their loans. The intention was also that the reform would reduce the individual cost of college attendance by reducing the amount of the loans.

The purpose of this paper is to evaluate the effect of the student aid reform on individual study efficiency, measured as the number of credit points achieved each semester. Hence, we study only individuals that are already enrolled at a university at the time of the reform. The number of credit points achieved each semester is closely related to the time-to-graduation, that is, the number of semesters required to reach a given number of credit points.

Increased graduation rates and the speed at which individuals obtain higher education degrees are declared social objectives in many countries. Brunello et al. (2004) study the expected time-to-graduation in ten European countries and find that the proportion of students who expect to graduate at least one year later than the specified time ranges from above 30 percent in Sweden and Italy to almost zero in the UK and Ireland.[2] Extending time-to-graduation provides private monetary costs to individuals by shortening their careers after graduation. Further, Brodaty et al. (2006) provide evidence that French individuals with longer than average time-to-graduation have significantly lower

[1] See National Board of Student Aid (CSN, 2007).
[2] Brunello et al. examine only Economics and Business students.

wages and employment rates in their early careers, and Holmlund et al. (2008) show that working experience post-college graduation is more valuable than working experience pre-college graduation. Häkkinen (2006) finds no significant effects on future employment or earnings of working while in college. Moreover, increased study duration creates considerable social costs by reducing labor supply and increasing dependency ratios.

Theoretically, the expected effects of the reform are not clear.[3] An increase in the grant level will tend to lower the relative cost of college education compared to being in the labor market and hence, *increase* the time-to-graduation. However, an increase in the grant level may *decrease* the time-to-graduation as it allows students to focus on their studies rather than on work. The tighter repayment schedule of the student loans should encourage students to minimize the amount of their loans by *decreasing* the time-to-graduation; however, it may *increase* the time-to-graduation if students choose not to take loans and work instead. Correspondingly, the increased possibilities of work during college may *increase* the time-to-graduation if students allocate more time to work and thereby less time to their studies.

There are only two (unpublished) studies that examine the effect of student aid on individual study efficiency/duration. Häkkinen and Uusitalo (2003) evaluate the time-to-graduation for Finnish college students during the 1990s. Specifically, they evaluate the effect of a student aid reform that was intended to shorten the duration of university studies.[4] Nielsen Ardent (2008) evaluates a Danish reform and examines how financial aid affects drop-outs from and completion of higher education.[5] These studies find only a modest or no effect of the studied reforms. A related, and relatively rich, literature examines the effect of tuition fees and student aid on the enrollment decision.[6]

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[3] See e.g., Becker (1993), Cameron and Taber (2000), Card (1999), Bettinger (2004), Eckstein and Woplin (1999), and Ehrenberg and Sherman (1987). See Nielsen Arendt (2008) for a summary of the theoretical framework.

[4] The Finnish reform incorporated a change from a loan-based to a grant-based financial system.

[5] The Danish reform incorporated a substantial increase in the grant-level.

[6] For studies on tuition levels and enrollment decisions, see e.g.,Manski and Wise (1983), McPherson and Schapio (1991 a,b), Kane (1994), Rouse (1994), Hoenack (1971), Ehrenberg and Sherman (1984), and Moore et al. (1991). For studies on student aid and enrollment decisions, see e.g. Schrøter Joensen (2009), Skyt Nielsen et al. (2008),Baumgartner and Steiner (2006), Linsenmeier et. al. (2006), Dynarski (2002, 2003), Van der Klaauw (2002), Reuterberg and Svensson (1999), Fredriksson (1997), Hammarström

To examine the effect of the reform on individual study efficiency, we follow students from enrollment until they achieve a degree or an equivalent number of credit points, and we examine the number of credit points earned each semester. We consider all students who enrolled in a program between 1995 and spring of 2001(i.e., before the reform). Hence, we include only students that are within a study spell at the time of the reform, and we estimate a linear regression model. We choose this approach, rather than using a duration model and estimate the time to degree, as it allows us to include individual fixed effects.[7] Furthermore, we estimate relative reform effects for individuals with different parental backgrounds. We find a slightly positive and significant effect of the reform on the aggregate level. However, dividing the sample conditionally on the parental educational level reveals that the individual study efficiency has increased only for those students from a strong academic background. Consequently, the relative study efficiency decreased for students from a weak academic background.

The different results for individuals from a strong and a weak academic background, respectively, seem to be related to differences in how they allocate their time between studying and working. Individuals from strong academic backgrounds earn substantially less after the reform compared to before, while students from weak academic backgrounds earn slightly more after the reform compared to before. One interpretation is that students want to minimize the amount of loans as a consequence of the reform, and they do so by reallocating time from work to studies so as to reach a degree sooner. However, students from weak academic backgrounds are possibly more dependent on financing their studies by work and therefore may not have this option; they rather increase their earnings.

The paper is organized as follows. In the next section, we briefly describe the Swedish university system and the financial aid for college studies in Sweden. In section three, our empirical strategy is presented, and in section four, we describe the data ana-

(1996), Manski and Wise (1983), McPherson and Schapio (1991 a,b), Kane (1994), Rouse (1994), Hoenack (1971), Ehrenberg and Sherman (1984), and Moore et al. (1991).

[7]Moreover, it creates fewer problems with endogenity with time-variant covariates. See Häkkinen and Uusitalo (2003) for a discussion.

lyzed. Section five presents the empirical results, and finally, in section six, some concluding remarks are offered.

2 The Swedish University System

The number of full-time equivalent university students has increased dramatically from approximately 161,000 in 1991 to approximately 304,000 in 2009, and the number of universities and colleges has increased from 5 universities in 1965 to approximately 25 universities and colleges that provide education within most fields today.[8,9] A political goal is that 50 percent of a birth cohort should be enrolled in higher education before the age of 25.

The system of higher education is financed and regulated by the Swedish Parliament and Government.[10] Since 1977, a single administrative authority on the national level handles the admissions for all colleges.[11] Because the number of applicants is often higher than the number of educational slots, generally, grades from upper secondary schooling determine admission.[12]

The graduation requirement up to the year 2007 was, generally, a minimum of 120 acquired credit points.[13] One credit point corresponded to approximately one week of full-time study, and 120 credits corresponded to three years of full-time studies (a bachelor's degree). In general, students can be divided into two groups: the program students and the course students.[14] Program students enter a program usually lasting three or more years, whereas course students register for separate courses that typically last one semester. However, separate courses may be combined to correspond to a program.

[8]National agency for higher education (2001, 2010). There are also a number of specialized colleges.
[9] The terms university, university college and college are used interchangeably throughout the paper.
[10] See National Agency for Higher Education (2004, 2006, 2007) for details on higher education in Sweden.
[11] Initially, they handled applications to programs and all courses. Later, they handled admissions mainly to programs and to most universities. Since 2007, they again handle applications to programs and most courses.
[12] There is also an aptitude test, and previous work experience may be taken into account.
[13] However, since 2007, as a result of the Bologna process, one credit point in the old system corresponds to 1.5 points in the new system.
[14] National Agency for Higher Education (2005).

2.1 Financial Aid for College Students in Sweden

There are no tuitions fees at Swedish universities, and the government offers universal financial support for all students. The financial support consists of both loans and grants. All students admitted to higher education are eligible for financial support independent of their parental background. The overall political motive for the financial aid system in Sweden is that it should promote high participation in education, and an important political intention of the student aid is to reduce the social stratification of higher education (prop. 1999/2000:10).

Since the introduction of the student aid system in 1965, there have been several reforms of the system. The reform implemented in the fall of 2001 is considered to be the largest reform since the introduction of the system (CSN 2007). The total amount of financial aid, of approximately 700 EUR/month, was unaffected in the 2001 reform. However, after the 2001 reform *i)* the share of the grant increased to 34.5 percent of the total amount compared to 27.8 percent in the earlier system, which means that the grants increase by approximately 600 SEK (60 EUR) per month; *ii)* students are allowed to earn 91,000 SEK per year while still receiving full student support, which is 67 percent more than in the former system (one motive for this change was to make it possible for students to gain (useful) work experience and improve their standard of living while studying); and *iii)* the repayment scheme for study loans is notably less generous than that in the former system. In the earlier system, students started to repay their loans six months after they received their last payment from the system. They paid 4 percent of their annual earnings and when they turned 65 all remaining debt was written off. In the new system, the monthly repayment is calculated as an annuity such that the total debt should be repaid in 25 years.[15] The changes of the repayment should thus make it significantly less attractive to take out study loans.

Further, after the reform in 2001, the maximum number of years for student aid was unchanged with a maximum of six years for student aid; however, following the reform, the possibility of being granted an extension is considerably less. The maximum age of

[15]If the loan is not repaid by the age of 60, individuals continue to pay until the age of 67. Individuals repay a maximum of 5 percent of their annual earnings. However, from the age of 50, the maximum is 7 percent of their annual earnings. The debt is written off at the age of 67.

eligibility for a student loan after 2001 is 50 years of age compared to 45 years of age before the reform. After the reform, students with children became eligible for higher student loans than students without children.

The new system covers all students who began studying in the fall of 2001 as well as those who were already enrolled in college at the time.

3 Empirical Strategy

We include only students *i)* with a registration in a program with a theoretical length of at least 120 credit points (corresponding to a bachelors degree), *ii)* who enrolled at a program *before* the reform, and *iii)* who have not achieved 120 credit points before the reform. We follow the students until they reach 120 credit points or the maximum of 12 semesters. In general, 12 semesters is the upper bound for student aid, that is, the maximum number of semesters students can receive student aid. The rationale behind these restrictions is that we want to be reasonably sure we include only students who are within a study spell at the time of the reform.

We estimate the linear regression model such that

$$\ln CP_{is} = r_{is}\beta_1 + \sum_{t=1}^{12} s_{it}\beta_{2t} + Fall_{is}\beta_3 + cum\_p_{is}\beta_4 + cum\_p_{is}^2\beta_5 + C_{i_c}'\beta_6 + X_i'\beta_7 + X_{is}'\beta_8 + (r_{is}Parent_i)'\beta_9 + \gamma_i + \varepsilon_{is} \quad (3.1)$$

where $lnCP_{is}$ is the logarithm of credit points achieved in semester *s* for individual *i*; *r* is a dummy variable that equals 1 after the reform and 0 before; β_1 is the reform effect; γ_i is an individual time-invariant component, such as individual ability; and ε_{is} is an individual time-varying component.

Because the reform was introduced at a particular point in time, any efficiency effect from the progression of studies due, for example, to experience, will be correlated with the reform and thus may confound the reform effect. We control for that individuals are at a later stage in their study spell after the reform compared to before (study progression) by including dummy variables for each semester since program entry (s). Moreo-

ver, we include a dummy variable indicating the fall semester (*Fall*).[16] However, individuals who have been enrolled for the same number of semesters may be at different points in their study spell in terms of the total credit points achieved. Therefore, we include a continuous variable for total credit points attained (*cum_p*) up to each *s*. To consider that the link between total credit points achieved up to semester *s* and credit points achieved in semester *s* may be non-linear, we also include a quadratic term (cum_p^2).[17]

To capture that the composition of students may change over time due to, for example, anticipation effects, we include dummy variables for each college entry cohort (*C*). This will control for all cohort specific factors that are constant across time. Moreover, this is a crucial control as our set up impose that students in earlier cohorts are different in terms of study efficiency compared to those in later cohorts. Furthermore, students are only included if they have *not* reached a degree or its equivalent before the reform. However, observing some students before the reform and some students after the reform during the same duration of study is what makes our approach viable.

Further, we include some time-constant control variables (X_i). Variables included are country of birth, gender, age at college entry, parental educational level and grades from high school (GPA). We also include a few time-varying variables (X_{is}) such as field of education, student status with respect to parent/non-parent and local youth unemployment rates (yearly).[18] We include local unemployment rates to control for the labor market opportunities at each college location (county). The reason to include these control variables is that we want to estimate the study efficiency holding these factors constant. For example, if the reform affects the student's choice of study program (field of education) and if different study programs are linked to individual study efficiency, the reform coefficient will capture this effect. Our objective is to estimate the study effi-

[16]Because the reform was introduced in the fall of 2001 and students acquire about 60 percent of their annual credit points during the spring semester, this is an important control.
[17] We have elaborated with dummy variables and found a non-linear decreasing trend that fits well with our specification. The results are robust for including a number of dummy variables as an alternative to our current specification. The results are presented in section 5.4 (the sensitivity analysis section).
[18] We use the definition used by the Swedish employment services. Youth unemployment is defined as unemployment rates for individuals 18-24 years of age. We choose this definition as most students are under the age of 24 at enrollment. See Figure A1 in the Appendix.

ciency given the study program. However, as we will see, the observable covariates do not significantly influence the estimated reform effect.

The panel dimension of our data further allows us to include individual fixed effects, that is, to control for all time-invariant individual specific factors. Hence, we estimate the following:

$$\Delta \ln CP_{is} = \Delta r_{is}\beta_1 + \sum_{t=1}^{12} \Delta s_{it}\beta_{2t} + \Delta Fall_{is}\beta_3 + \Delta cum\_p_{is}\beta_4 + \Delta cum\_p_{is}^2\beta_5 + \Delta X_{is}^{'}\beta_6 + (\Delta r_{is} Parent_i)\beta_7 + \Delta\varepsilon_{is} \quad (3.2)$$

where Δ indicates deviations from individual means. Note that in the case of dummy variables, Δ indicates a change from 0 to 1. C_i, X_i and γ_i in equation (3.1) are eliminated, as they are constant within the individual. The average treatment effect, β_1, is identified if the transformed model residual is uncorrelated with the reform dummy. If there are time trends in study efficiency for which we have not controlled in the model, the average treatment effect will be biased. Though we believe that our cohort fixed effects (included in the individual fixed effects) and local unemployment rates capture most relevant time trends, we introduce an additional source of variation by including interaction variables between the parental educational level and the reform (*rParent*). β_8 is the relative reform effect. β_8 is identified under the relatively weaker assumption that students from different parental backgrounds are affected similarly by any time trends, the parallel trends assumption.

From previous literature, we expect students from a weak academic background to be more responsive to changes in the student aid system. Hammarström (1996) has investigated why high school graduates in Sweden do not enroll in higher education. Her results suggest that individuals from a weak academic background are more dependent on financial aid when deciding whether to pursue higher education. Moreover, a survey administered to students with student aid in 2001 and 2003 reveals that approximately 60 percent of all students would "probably not" or "would not" have enrolled in higher education if there were no student aid.[19] For students who have parents with less than a

[19]CSN (2007).

post-secondary education, that number was 72 percent, and for student with parents with at most a post-secondary education, the share was 64 percent. For students with highly educated parents, the share was only 42 percent. To summarize, students from a strong academic background appear to be less dependent on the student aid system.

4 Data

The data used are provided by the Institute for Labour Market Policy Evaluation (IFAU). The original data sources are Statistics Sweden and the National Board for Higher Education. The data consist of a number of merged administrative records. The main registers used are the college registration registers that contain information such as field of study and level of education, a longitudinal income register (LOUISE) that gathers information on demographics and socioeconomic factors, and the employment register (RAMS) that contains information about earnings. The data cover the entire Swedish population aged 16 to 74 and is available up to 2007, at the time of this study.

4.1 The sample

Our original sample, including all students with a first registration at a university between the fall of 1995 and the spring of 2001, consists of approximately 368,000 students. By including only program students, we exclude approximately 110,000 individuals, and by excluding students who have attained at least 120 credit points (corresponding to a bachelors degree) before the reform we lose an additional 84,000. By imposing an age restriction and including only individuals who are under the age of 41 at the time of enrollment, we exclude another 4,000 individuals. The reason for imposing this age restriction is that the eligibility of student aid is reduced for each year after the individual has turned 41. Moreover, we exclude individuals who have missing information in any population register (5,000 observations). These are most likely to be non-Swedish citizens and/or exchange students. We then exclude 28,000 individuals because there is no available information about their high school GPA. Grades from high school are only available for individuals who graduated from high school in 1985 or later. Approximately 11,000 individuals are excluded because they have missing parental information on one or both parents. The reason for excluding these individuals is that the

causes of the missing parental data are unknown and, thus, probably differ for different individuals within the group and, as such, it is not a homogenous group. Finally, we exclude another 4,000 individuals who have missing information in the field of education.

After applying the above restrictions, we have approximately 122,000 individuals in our sample. We perform some sensitivity analyses to the above restrictions in Section 5.4.

4.2 The variables[20]

The data contain information about both registered credit points and completed credit points by semester. We observe the number of academic credit points *completed* each semester. The number of registered credit points only indicates the number of credit points an individual signed up for; it reveals no information about whether the individual actually attended or participated in the course.

In 1996, the grading in high school changed. Until 1996, the grading scale ranged from 1 to 5, with 1 being the lowest and 5 the highest. From 1997 on, the grading consists of a four-degree scale of 0 to 20 (0, 10, 15, and 20). We have converted all grades into the new grading system, where 0 is the lowest grade and 20 the highest.[21]

The field of education is defined as the registered field each semester. If the individual has several registered fields of education for the same semester, we use the field in which the individual achieved the most number of credit points. If the student does not achieve any credit points in a given semester, the field of education is the same as the registered field of education the previous semester. Hence, individuals change field of education when a new field is registered. If the individual does not have a known field of education the first semester, we use the registered field the following semester. This accounts for approximately 20 percent of the sample.

Incomes are available on a yearly basis. We have information about student earnings, capital incomes, unemployment benefits, sickness insurance and student aid. We cannot distinguish between student loans and grants; we can only observe the total amount of student aid. However, if we assume that individuals who have loans normally also have

[20] See Appendix Table A1 and A2 for a detailed description of the variables used.
[21] After performing this conversion, we do not find any discontinuities in the distribution of the GPA or any other objects in our data that might be related to the different grading system.

grants, we may also assume that students who have a total amount of student aid that is equal to or less than the maximum grant, do not have any (or very small) loans. This is a reasonable assumption as students eligible for loans are also eligible for grants. Still, students may, in theory, choose not to take grants (e.g., principled reasons). In practice, this is not very likely.

Further, we have information about the parental educational level. This is used to deduce whether the student may be classified as having a weak or a strong academic background. We divide students into three categories depending on their parental educational level: *i)* both parents have a post-high school education (BOTH), *ii)* only one parent has a post-high school education (ONE), and *iii)* neither parent has a post-high school education (NONE). We will focus on the groups NONE and BOTH; however, the results for ONE are also presented throughout. If the parental educational level changes during the time period, we use the highest educational level attained.

5 Empirical Results

In this section, we present the results from our analysis. First, we present the results on an aggregate level including interaction variables between the reform and the parental educational level; that is, we estimate the relative reform effects. Second, we conduct separate analyses on students with different pre-reform amounts of student aid and labor market earnings. Finally, we perform robustness checks to confirm that our model and results are stable with respect to the choice of sampling and functional form.

5.1 Main Results

Table 1 presents our main results. We have estimated our model by *i)* successively including different covariates, *ii)* including individual fixed effects, and *iii)* including interaction variables between the parental educational level and the reform.

First, we only include the reform dummy variable, that is, a dummy variable indicating the post reform period. Including only the reform dummy variable results in a negative coefficient; thus, the number of achieved credit points each semester is, on average, lower after the reform than before the reform.

Next, we control for study progression by *i)* including dummy variables for each semester of enrollment (Specification 2) together with an indicator for whether the semester is spring or fall and *ii)* including the total credit points achieved up to each semester (Specification 3). Including control variables for study progression changes the sign of the estimated coefficient of the reform. Hence, the negative estimate in Specification 1 appears to be driven by the fact that students are at a different stage in their study spell before the reform compared to after the reform. This is expected, as we have a strong correlation between the reform dummy variable and the study progression variables; semesters later in the individual study spell will occur after the introduction of the reform. Likewise, students with more accumulated credit points are concentrated in the years after the reform. The coefficients on the semester specific indicators are more negative for later semesters; for example, the probability of being a college drop-out is higher. This tends to bias the reform effect parameter downwards. However, the positive linear term on the cumulative credit points indicates that students are more productive when they are at a later stage of their study spell, perhaps due to experience, which tends to bias the reform parameter upwards.

In Specification 4, we include cohort fixed effects. The cohort fixed effects will capture all time-invariant factors that are cohort specific. This is a crucial control because it will capture that the study efficiency, on average, by definition in our set up, is lower for earlier entry cohorts. Including the cohort fixed effects, the estimated coefficient on the reform dummy is still positive and significant, though much smaller in magnitude. Further, in Specification 5, we add a number of control variables, such as field of education, gender, country of birth, age at program entry, GPA from high school, student status as parent/non-parent, local unemployment rates and parental educational level. To include these observable covariates does not have much effect on the estimated reform effect, but all of the variables have a significant effect on credit points achieved each semester. For example, women appear to be slightly less effective in producing academic credit points compared to men, while immigrants are considerably more efficient than natives. Furthermore, students with a higher age at enrollment and students with children seem to be less efficient, while students with a higher GPA and a higher parental educational level are more efficient. The local unemployment rate is positively correlat-

ed with individual efficiency; one explanation may be that the possibility of work is more limited during high unemployment periods and thus, students focus more on their studies.

In Specification 6, we include individual fixed effects (FE) to control for additional time-invariant individual heterogeneity possibly not captured by our observable control variables. Including individual FE, the magnitude of the estimated reform effect is reduced. Hence, our FE captures some individual specific (time-invariant) factors that have not been controlled for by including observable control variables. Therefore, we include individual FE from here on.

To examine the relative reform effects for individuals with a strong and weak academic background, we include interaction variables between the parental educational level and the reform (Specification 7). The estimated reform effect is significant and slightly negative for students from a weak academic background, whereas the reform effect for individuals from a strong academic background is highly significant and has a positive magnitude of more than ten percent.[22] Thus, while students from a weak academic background seem to be rather unaffected by the reform, students from a strong academic background have a considerably higher efficiency after the reform compared to before. Hence, the time-to-degree is shorter. To put the estimated effects into context, ten percent corresponds to two credit points for full-time students; over the course of six semesters (the equivalent of a bachelor's degree for a full-time student), this corresponds to twelve credit points, which is more than half a semester.

In Section 3, we posed the hypothesis that students from a strong academic background are less dependent on the student aid system and therefore less sensitive to changes in the system. However, our results indicate that mainly students from a strong academic background are affected by the reform. However, if students from a strong academic background have better knowledge and information about the student aid system, they could potentially be more sensitive to changes in the system. An alternative explanation for this result, based on theory, is that students may react to the reform by

[22] We also excluded the time-varying variables field of education and having children from this specification, because these variables possibly are endogenous to the reform. However, the estimated reform effects were not affected. Hence, to include these variables does not affect the results. See section 3 for a more detailed discussion.

minimizing the amount of loans by reallocating time from work to studies so as to sooner attain a degree, while students from a weak academic background may not have the option to reallocate time from work to studies, plausibly being more dependent on financing their studies by work. Yet another explanation, which fits with the hypothesis posited in Section 3, is that the estimated reform effect for students from a strong academic background captures some time trend in study efficiency, while students from a weak academic background would have had the same time trend in absence of the reform. Hence, their study efficiency has decreased relative to students from a strong academic background. Alternatively, students with different academic backgrounds simply may have different time trends. In the next section, we will further discuss and test the hypotheses presented here.

Finally, in Specification 8, we include a dummy variable that controls for semesters with zero credit points achieved (intermissions or drop-out). The estimated reform effect is now interpreted as the effect for *active* students, that is, for students who have obtained at least some credit points. For students from a weak academic background, the estimated reform effect becomes more negative, including a variable that controls for semesters with zero credit points. For students from a strong academic background, the estimated reform effect changes sign and is now slightly negative, though not as negative as for students from a weak academic background. Hence, the positive reform effect for students from a strong academic background is completely driven by their intermission and dropping-out behaviors. Moreover, for active students, the reform effect is more pronounced for students from a weak academic background than it is for students from a strong academic background.

The conclusion from the results controlling for intermissions is that students, especially those from a strong academic background, on average, tend to have fewer intermissions (semesters with zero credit points achieved) after the reform than before the reform. This result fits nicely with the interpretation that individuals tend to reallocate time from work to studies so as to graduate faster. Another interpretation, related to the stricter repayment schedule of the student loans, is that students drop-out to a lesser degree since *i)* they have to start the repayment of their loans as they do or *ii)* they have

greater incentive to attain a degree because it will increase their chances of obtaining a job and an income, thus allowing them to have the necessary means to repay their loans.

Intermission and drop-out behavior is a part of the individual study efficiency, our outcome of interest; therefore, we will not control for intermissions in our main analyses. We will, however, present the results including intermissions in the Appendix. Moreover, we will, for the remainder of the paper, present the results including the interaction variables between parental educational level and the reform. Hence, we focus on Specification 7 in Table 1 from here on.

Table 1. The estimated reform effect on credit points achieved each semester. OLS.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
r	-0.288***	0.490***	0.226***	0.097***	0.085***	0.032***	-0.013**	-0.058***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.006)	(0.004)
ONE*r							0.044***	0.008
							(0.008)	(0.005)
BOTH*r							0.126***	0.027***
							(0.008)	(0.005)
Study progression variables								
Number of semesters since enrollment								
S_2		-0.021***	-0.473***	-0.447***	-0.446***	-0.666***	-0.666***	-0.265***
		(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
S_3		-0.193***	-0.992***	-0.937***	-0.928***	-1.276***	-1.275***	-0.524***
		(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.005)
S_4		-0.320***	-1.432***	-1.358***	-1.344***	-1.774***	-1.772***	-0.779***
		(0.005)	(0.005)	(0.005)	(0.005)	(0.008)	(0.008)	(0.007)
S_5		-0.500***	-1.864***	-1.765***	-1.745***	-2.197***	-2.195***	-0.963***
		(0.005)	(0.004)	(0.005)	(0.005)	(0.010)	(0.010)	(0.008)
S_6		-0.663***	-2.189***	-2.077***	-2.053***	-2.453***	-2.450***	-1.112***
		(0.006)	(0.005)	(0.005)	(0.005)	(0.011)	(0.011)	(0.009)
S_7		-0.937***	-2.532***	-2.404***	-2.375***	-2.720***	-2.718***	-1.032***
		(0.005)	(0.005)	(0.005)	(0.005)	(0.012)	(0.012)	(0.010)
S_8		-1.541***	-3.008***	-2.864***	-2.833***	-3.101***	-3.099***	-1.319***
		(0.006)	(0.006)	(0.006)	(0.006)	(0.013)	(0.013)	(0.010)
S_9		-1.638***	-3.091***	-2.928***	-2.888***	-3.116***	-3.113***	-1.209***
		(0.006)	(0.006)	(0.006)	(0.007)	(0.013)	(0.013)	(0.011)
S_10		-2.146***	-3.489***	-3.312***	-3.267***	-3.446***	-3.442***	-1.464***
		(0.007)	(0.006)	(0.007)	(0.007)	(0.014)	(0.014)	(0.011)
S_11		-2.075***	-3.431***	-3.244***	-3.189***	-3.335***	-3.330***	-1.278***
		(0.006)	(0.006)	(0.007)	(0.007)	(0.014)	(0.014)	(0.011)
S_12		-2.520***	-3.781***	-3.580***	-3.521***	-3.628***	-3.622***	-1.542***
		(0.007)	(0.007)	(0.007)	(0.008)	(0.014)	(0.014)	(0.012)
Fall		-0.555***	-0.434***	-0.422***	-0.425***	-0.414***	-0.414***	-0.440***
		(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Cumulative credit points								
cum_p			0.036***	0.035***	0.035***	0.056***	0.056***	0.032***
			(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
cum_p^2			-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
			(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Cohorts								
C_1996				0.099***	0.111***			
				(0.006)	(0.006)			
C_1997				0.155***	0.174***			
				(0.005)	(0.005)			
C_1998				0.303***	0.325***			
				(0.005)	(0.005)			

C_1999				0.340***	0.377***			
				(0.005)	(0.005)			
C_2000				0.312***	0.359***			
				(0.005)	(0.006)			
C_2001				0.204***	0.318***			
				(0.007)	(0.007)			
Field of education								
Pedagogic					-0.027***	0.036***	0.038***	0.055***
					(0.005)	(0.011)	(0.011)	(0.007)
Humanities					-0.022***	-0.079***	-0.079***	-0.047***
					(0.004)	(0.008)	(0.008)	(0.005)
Science					0.064***	-0.115***	-0.114***	-0.031***
					(0.003)	(0.007)	(0.007)	(0.004)
Technology					0.020***	-0.105***	-0.105***	-0.063***
					(0.003)	(0.008)	(0.008)	(0.005)
Other					0.029***	-0.019	-0.018	-0.001
					(0.006)	(0.012)	(0.012)	(0.008)
Healthcare					-0.030***	0.109***	0.109***	0.051***
					(0.003)	(0.012)	(0.012)	(0.007)
Woman					-0.017***			
					(0.002)			
Immigrant					0.094***			
					(0.005)			
Age at first registration					-0.021***			
					(0.000)			
GPA					0.001***			
					(0.000)			
Have children					-0.087***	-0.259***	-0.253***	-0.071***
					(0.004)	(0.011)	(0.011)	(0.007)
Local unempl. rates					0.004***	0.034***	0.034***	0.007***
					(0.000)	(0.001)	(0.001)	(0.001)
Parental educational level (university educ.)								
ONE					0.020***			
					(0.002)			
BOTH					0.054***			
					(0.002)			
Individual FE	No	No	No	No	No	Yes	Yes	Yes
Intermission								-1.720***
								(0.004)
Constant	2.004***	2.698***	2.219***	1.948***	2.287***	1.631***	1.631***	2.170***
	(0.002)	(0.004)	(0.003)	(0.006)	(0.014)	(0.010)	(0.010)	(0.007)
Observations	1,135,649	1,135,649	1,135,649	1,135,649	1,135,649	1,135,649	1,135,649	1,135,649
R-squared	0.011	0.255	0.482	0.486	0.491	0.299	0.299	0.542
ll	-1.965e+06	-1.804e+06	-1.598e+06	-1.594e+06	-1.588e+06	-1.474e+06	-1.474e+06	-1.233e+06
Number of id.	122,372	122,372	122,372	122,372	122,372	122,372	122,372	122,372

Note: Robust standard deviations are in parenthesis. *p<0.1, **p<0.05 ***p<0.01.

5.2 Students with Different Amounts of Student Aid

In this section, we divide students into two groups depending on their average amount of student aid during the first two years of studies (before the reform for most cohorts): *i)* students with student aid corresponding to the maximum amount of student aid for full-year students (loans and grants)[23] and *ii)* students with student aid corresponding to

[23] That is, full-time students who are enrolled for the full-academic year.

the amount of student aid for full-year students with *only* grants.[24] Hence, our intention is to identify full-year students and to divide them into two groups – one group of students with grants and maximum loans and one group with only grants. Students with loans are affected by the changes in the repayment rules, whereas students with only grants are primarily affected by the change in the grant amount and in the allowed amount of earnings. However, we can only observe the total amount of student aid; that is, we cannot distinguish between grants and loans in the data. Therefore, we need to assume that students with a total amount of student aid corresponding to full-year grants do not have any loans. In theory, it is possible that these students have loans that correspond to full-year grants. However, as mentioned in Section 4, this is not very likely.

From Table 2, we can see that there is a significant and positive reform effect for students with only grants, whereas there is a significant and negative reform effect for students who also have the maximum amount of loans.[25] Note that this is the result for individuals from a weak academic background. Theoretically, an increase in the grant level may increase study efficiency by allowing students to focus on their studies rather than on working, and the tighter repayment schedule of the loans may decrease study efficiency if students choose not to take loans and, instead, finance their studies by working.

[24] See Appendix Figure A2 for the distribution of the amount of student aid. We use only the groups *between* the vertical lines in our estimation.
[25] For full estimates, see Appendix Table A3.The results, including control for intermissions, are displayed in Appendix Table A4.

Table 2. The estimated reform effect on credit points achieved each semester for students with only grants and students with grants *and* a maximum amount of loans before the reform. OLS.

	Only Grants	Grants and max. loans
r	0.063***	-0.112***
	(0.019)	(0.008)
ONE*r	0.064***	0.070***
	(0.024)	(0.009)
BOTH*r	0.164***	0.152***
	(0.025)	(0.010)
Constant	1.448***	1.819***
	(0.036)	(0.016)
Controls	Yes	Yes
Individual FE	Yes	Yes
Observations	105,162	418,018
R-squared	0.317	0.449
Number of id.	11,521	49,484
Ll	-134241	-476610

Note: Controls include study progression variables, field of education, have children and local unemployment rates. Robust standard deviations are in parenthesis. *p<0.1, **p<0.05 ***p<0.01. See Appendix Figure A2 for the distribution of the student aid and the definition of the groups "only grants" and "grants and loans".

For individuals with a strong academic background, the estimated reform effect is significant and substantially positive for students with only grants and is only slightly positive for students with both grants and loans. In the previous section, we suggested that students may react to the reform by minimizing the amount of loans by allocating less time to work and more time to studying so as to sooner attain a degree. One explanation for the different results for students with only grants compared to students with grants and loans could be that students with a maximum amount of loans already before the reform focused on their studies. Hence, they could not reallocate much more time to studying.

To test the time reallocation hypotheses, we estimate the effect of the reform on individual earnings for the two groups: students with only grants and students with grants and maximum loans. Note that earnings are only observed on yearly basis; hence, we adjust the model and estimate credit points achieved each year. We define the reform dummy as the post-2000 period (rather than post the spring semester 2001).[26] The results in Table 3 confirm our hypotheses that students from a weak academic background

[26] To exclude the year 2001 from the analysis and define the reform dummy as the post-2001 period produces similar results though more positive estimates.

who have only grants earn less after the reform compared to before the reform, whereas students with both grants and loans significantly increase their earnings after the reform.[27] Students from a strong academic background decrease their earnings after the reform compared to before, and for those students who have only grants, this decrease is even more substantial.

An alternative hypothesis posited in Section 5.1 is that the estimated reform effect captures some time trend in study efficiency that we have not controlled for. Controlling for individual FE, that is, factors such as individual preferences for loans, etc., there is no obvious reason that students with only grants and grants plus maximum loans should have different time trends. For students from a strong academic background, the estimated reform effect for the sub-groups has the same sign but is very different in magnitude, whereas for students from a weak academic background, the estimated reform effect has opposite signs. This supports the interpretation that the reform has had a causal effect on individual study efficiency.

Table 3. The estimated reform effect on annual earnings for students with only grants and students with grants and a maximum amount of loans before the reform. OLS.

	Only Grants	Grants and max. loans
r	-0.101***	0.102***
	(0.035)	(0.015)
ONE*r	-0.151***	-0.177***
	(0.045)	(0.019)
BOTH*r	-0.423***	-0.401***
	(0.046)	(0.019)
Constant	2.564***	2.166***
	(0.058)	(0.026)
Controls	Yes	Yes
Individual FE	Yes	Yes
Observations	57,885	236,400
R-squared	0.400	0.425
Number of id.	11,521	49,484
Ll	-92390	-355496

Note: Controls include study progression variables, field of education, having children and local unemployment rates. Robust standard deviations are in parenthesis. *p<0.1, **p<0.05, ***p<0.01. See Appendix Figure A2 for the distribution of the student aid and the definition of the groups "only grants" and "grants and loans".

[27] For full estimates, see Appendix Table A5.The results, including control for intermissions, are displayed in Appendix Table A6.

5.3 Students with Different Earnings

In this section, we examine the reform effect for students with different earnings before the reform. As with the student aid, we use the average earnings over the first two years of the study spell.[28] In particular, we are interested in determining if the reform effect differs between students who earn less than and more than the allowed amount before the reform. Students who earn more than the allowed amount before the reform are probably not as affected by the changes imposed by the reform as students who earned less than the allowed amount, which was possibly restricted by the rules in the former system. Overall, this analysis corresponds to the analysis in the previous section; students with only grants may earn more than the allowed amount, whereas students with grants and maximum student loans, by definition, earn less. The allowed amount before the reform was approximately 55,000 SEK (approximately 5,500 EUR).[29]

The results in Table 4 reveal that, for students from a weak academic background, the study efficiency has decreased for students who, before the reform, earned less than 55,000 SEK, whereas the study efficiency seems to have increased slightly for students who earned more than the allowed amount. This result is consistent with the results in the previous section.[30] The corresponding result holds for individuals from a strong academic background. That is, individuals who earned more than the allowed amount increased their study efficiency more compared to students who earned less than the allowed amount.

[28] See Appendix Figure A3 for the distribution of earnings.
[29] This corresponds to 1.5 basic amounts. In 2001, one basic amount corresponded to 36,900 SEK.
[30] For full estimates, see Appendix Table A7. The results, including control for intermissions, are displayed in Appendix Table A8.

Table 4. The estimated reform effect on credit points achieved each semester for students with different earnings (E) before the reform (in basic amounts). OLS.

	E=0	0<E<37’	37’<E<55’	55’<E<92’	E>92’
r	-0.009	-0.049***	-0.038***	0.029*	0.026
	(0.030)	(0.008)	(0.013)	(0.015)	(0.019)
ONE*r	0.071*	0.071***	0.052***	0.039*	0.039
	(0.040)	(0.010)	(0.017)	(0.020)	(0.028)
BOTH*r	0.180***	0.161***	0.173***	0.133***	0.167***
	(0.041)	(0.010)	(0.018)	(0.023)	(0.035)
Constant	1.628***	1.687***	1.714***	1.695***	1.453***
	(0.056)	(0.014)	(0.024)	(0.028)	(0.039)
Controls	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Observations	39,547	603,467	204,092	161,069	88,768
R-squared	0.291	0.313	0.333	0.310	0.319
Number of id.	4,032	66,323	22,443	16,766	8,126
Ll	-51381	-773975	-261776	-211841	-114028

Note: Controls include study progression variables, field of education, having children and local unemployment rates. Robust standard deviations are in parenthesis. *p<0.1, **p<0.05 ***p<0.01. See the Appendix Figure A3 for the distribution of earnings and the definition of the thresholds. The thresholds are based on basic amounts (1, 1.5 and 2.5).

As in the previous section, we investigate whether the heterogeneous results may be linked to the reallocation of time between working and studying; hence, we specifically investigate how individual earnings are affected by the reform for students who earned less than and more than the allowed amount. Again, as in the previous section, we adjust the model to yearly data. From Table 5, it is obvious that individuals from a weak academic background who earned less than 55,000 SEK before the reform have increased their earnings, whereas there is a negative and significant effect for individuals who earned more than 55,000 SEK. Individuals from a strong academic background have decreased their earnings, and more so for those who earned less than 55,000 SEK before the reform.[31] The results confirm the findings from the previous section and, hence, strengthen the hypothesis that individuals tend to reallocate time from studying to working as a consequence of the reform.

[31] For full estimates, see Appendix Table A9.The results including control for intermissions are displayed in Appendix Table A10.

Table 5. The estimated reform effect on annual earnings for students with different earnings (E) before the reform (in basic amounts). OLS.

	E=0	0<E<37’	37’<E<55’	55’<E<92’	E>92’
r	-0.079	0.046***	0.087***	-0.066**	0.043
	(0.051)	(0.014)	(0.024)	(0.029)	(0.046)
ONE*r	-0.061	-0.195***	-0.226***	-0.126***	0.067
	(0.066)	(0.018)	(0.031)	(0.039)	(0.067)
BOTH*r	-0.372***	-0.440***	-0.497***	-0.289***	-0.229***
	(0.058)	(0.017)	(0.033)	(0.043)	(0.084)
Constant	1.137***	1.721***	2.798***	3.563***	4.679***
	(0.091)	(0.021)	(0.040)	(0.049)	(0.086)
Controls	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Observations	21,296	333,650	112,714	87,450	46,038
R-squared	0.358	0.406	0.369	0.306	0.240
Number of id.	4,032	66,323	22,443	16,766	8,126
Ll	-31038	-501468	-184793	-155197	-93881

Note: Controls include study progression variables, field of education, having children and local unemployment rates. Robust standard deviations are in parenthesis. *p<0.1, **p<0.05 ***p<0.01. See the Appendix Figure A3 for the distribution of earnings and the definition of the thresholds. The thresholds are based on basic amounts (1, 1.5 and 2.5).

5.4 Sensitivity Analysis

To test the robustness of our results, we have preformed a number of sensitivity analyses. The results are presented in Table A11 of the Appendix. We have, in our baseline specification (Specification 7 in Table 1), *i)* included students with missing information on their parental education in the NONE category (students for whom neither parent has a university education),*ii)* imposed a tighter age-restriction so as to focus on a more homogenous sample, and *iii)* exchanged the continuous variables for total credit points achieved (*cum_p* and cum_p^2) with dummy variables to test the functional form of our model. The results are robust to these sensitivity checks.

Throughout, we have presented all results, including a dummy variable for semesters with zero credit points achieved, that is, the results for *active* students, in the Appendix. However, until now, we have not commented on these results. Overall, the pattern is very similar to the results presented in the paper, including intermissions in the outcome, but as we also noted in Section 5.1, the parameter estimates are, overall, less positive/more negative. In general, we find no positive results on study efficiency for students from a strong academic background. Hence, the positive reform effect for students from a strong academic background appears to be completely driven by their intermission and drop-out behavior.

6 Conclusions

This paper evaluates the effect of a significant reform in student aid on individual study efficiency measured by the number of credit points obtained each semester. Though the total amount of student aid was unaffected, the reform incorporated three major changes: *i)* the grant-share of the total aid increased, *ii)* students were allowed to earn more without a reduction in their student aid, and *iii)* the rules of the repayment of the loans were significantly tightened. Moreover, the reform incorporated a few minor changes including the following: the maximum number of years with student aid is, as before the reform, 6 years, but the possibility for an extension is considerably reduced; the maximum age of eligibility is 50 years, compared to 45 before the reform; and students with children are allowed higher student loans than students without children. Theoretically, the expected effect of the reform is unclear.

We find a slightly positive and significant reform effect on the aggregate level, but when dividing the sample conditionally on the student's parental educational level, we find that the study efficiency has increased only for students from a strong academic background. Hence, the time-to-graduation has decreased for this group of students, and consequently, the relative time-to-graduation has increased for students from a weak academic background.

Long study durations, as mentioned, not only create considerable social costs but also create costs at the individual level. One of the overall objectives of the student aid system is to reduce the cost of higher education for individuals with a weak academic background. However, relative to individuals from a strong academic background, the reform appears to have increased the cost in terms of relatively longer study duration of higher education for individuals from a weak academic background, even though the system of student aid after the reform may, to a higher degree, bear its own costs.

We pose two main hypotheses to explain our result. First, we suggest that students want to minimize the amount of loans as a consequence of the reform and possibly do so by reallocating time from working to studying so as to sooner attain their degree. However, students from a weak academic background may not have the option to reallocate time from working to studying. Second, we pose the hypothesis that students from a strong academic background are less dependent on the student aid system and

therefore are less sensitive to changes in the system. Hence, the estimated reform effect may capture some time trend, and students from a weak academic background would have had the same time trend in absence of the reform. However, students with different academic backgrounds may simply have different time trends. To test these hypotheses and to further examine the effect of different aspects of the reform, we perform analyses for some sub-samples.

We divide the students into sub-groups based on the amount of student aid and earnings before the reform. We posit that if our estimated reform effect captures some time trend, the effect should be similar between sub-groups of students with the same parental background. Note that we control for individual FE and, i.e., factors such as individual preferences for loans among other factors. For students from a strong academic background, the estimated reform effect has the same sign but is very different in magnitude for students who have only grants before the reform compared to students with both grants and loans. For students from a weak academic background the estimated reform effect has opposite signs for the two groups. This strengthens the interpretation that the reform has had a causal effect on individual study efficiency.

The sub-sample analysis further reveals that for individuals from a weak academic background, students who have only grants (and who may earn more than the allowed amount) increase their study efficiency, whereas students who have both grants and loans (and who earn less than the allowed amount) decrease their study efficiency. The corresponding pattern holds for students from a strong academic background; individuals with only grants increase their study efficiency substantially more compared to students with both grants and loans. Theoretically, an increase in the grant level may increase study efficiency by allowing students to focus on their studies instead of e.g. working, and the tighter repayment schedule of the loans may decrease study efficiency if students choose not to take loans and to finance their studies by working instead. For students from a strong academic background, however, students who have both grants and loans increased their study efficiency, though not as much as students with only grants; students with both grants and loans may already, before the reform, focus on their studies and thus do not reallocate more time to studies. Moreover, the different results for students from strong and weak academic backgrounds seem to be related to the

reallocation of time between working and studying. That is, students from a strong academic background earn less after the reform compared to before, whereas students from a weak academic background, on average, earn slightly more. Students from a weak academic background are plausibly more dependent on financing their studies by work and accordingly, may not have the option to reallocate time from work to studies; rather, they seem to allocate more time to work.

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Appendix

Tables

Table A1. Variable list.

Variables	Description
r	= 1 after the reform, i.e., fall 2001 - .
S_1 – S_12	Dummy variables for each semester from registration.
Fall	=1 if fall semester.
Cum_p	The total number of credit points acquired.
Cum_p^2	‘’ (squared).
C_1995	= 1 if the student has a first registration in a program in 1995.
C_1996	‘’ 1996.
C_1997	‘’ 1997.
C_1998	‘’ 1998.
C_1999	‘’ 1999.
C_2000	‘’ 2000.
C_2001	‘’ 2001.
Unemployment	Youth unemployment rate at the university location.
Social science	=1 if the dominating field of education is social science.
Pedagogics	=1 ” pedagogics.
Humanities	=1 ” humanities.
Science	=1 ” science.
Technology	=1 ” technology.
Health Care	=1 ” health care
Other	= 1 “ agriculture, services or ”other”
Woman	=1 woman
Parent	=1 if the student has children.
Immigrant	= 1 if born outside Sweden
Age at first reg.	The student’s age at first registration
NONE	= 1 if neither parent has a university education
ONE	= 1 if one parent has a university education
BOTH	= 1 if both parents have a university education.
MISSING	= 1 if parental information is missing from one or both parents.
GPA	Grade point average (GPA) from high school.
WORK	Work related earnings.
STUD	Student aid income.
Intermissions	Indicates semesters with zero credit points achieved.

Table A2. Means and standard deviations of the variables used. Different samples.

Variable	Full	BOTH	ONE	NONE	OG	FT	E=0	0<E<37’	37’<E<55’	55’<E<92’	92’<E
C_1995	0,05	0,04	0,05	0,06	0,08	0,04	0,08	0,06	0,05	0,04	0,04
C_1996	0,07	0,05	0,06	0,07	0,07	0,05	0,10	0,07	0,06	0,06	0,08
C_1997	0,10	0,10	0,10	0,10	0,10	0,07	0,13	0,10	0,09	0,09	0,13
C_1998	0,18	0,19	0,18	0,17	0,18	0,17	0,19	0,20	0,17	0,15	0,17
C_1999	0,28	0,28	0,28	0,27	0,30	0,33	0,24	0,29	0,30	0,28	0,26
C_2000	0,30	0,30	0,30	0,29	0,27	0,36	0,26	0,28	0,34	0,37	0,33
C_2001	0,04	0,04	0,04	0,04	0,00	0,00	0,00	0,00	0,00	0,00	0,00
Unemployment	8,78	8,06	8,76	9,22	8,74	8,60	9,76	9,02	8,62	8,39	8,65
	(3,74)	(3,46)	(3,74)	(3,83)	(4,04)	(3,48)	(4,03)	(3,77)	(3,70)	(3,64)	(3,82)
Pedagogics	0,02	0,01	0,02	0,03	0,03	0,02	0,01	0,02	0,02	0,03	0,03
Humanities	0,08	0,08	0,08	0,08	0,06	0,08	0,08	0,08	0,07	0,07	0,08
Social Science	0,26	0,27	0,26	0,25	0,22	0,27	0,21	0,24	0,27	0,28	0,26
Science	0,18	0,21	0,18	0,16	0,17	0,17	0,27	0,20	0,15	0,15	0,14
Technology	0,22	0,22	0,23	0,22	0,28	0,21	0,26	0,24	0,22	0,21	0,24
Other	0,02	0,01	0,02	0,02	0,02	0,02	0,01	0,02	0,02	0,02	0,02
Health	0,16	0,14	0,15	0,18	0,17	0,17	0,09	0,14	0,18	0,17	0,14
Woman	0,52	0,48	0,51	0,55	0,52	0,56	0,36	0,52	0,56	0,50	0,39
Parent	0,09	0,04	0,07	0,12	0,04	0,08	0,14	0,07	0,08	0,10	0,17
Immigrant	0,04	0,04	0,04	0,04	0,03	0,04	0,11	0,04	0,03	0,03	0,03
Age at first reg.	22,01	21,16	21,78	22,66	20,84	22,26	22,48	21,41	22,09	22,76	23,89
	(3,17)	(2,32)	(2,96)	(3,58)	(2,39)	(3,13)	(3,74)	(2,80)	(3,02)	(3,34)	(3,84)
NONE	0,44	0,00	0,00	1,00	0,43	0,45	0,46	0,40	0,47	0,50	0,55
ONE	0,30	0,00	1,00	0,00	0,31	0,30	0,29	0,30	0,30	0,31	0,29
BOTH	0,25	1,00	0,00	0,00	0,26	0,25	0,26	0,29	0,23	0,19	0,16
GPA	14,27	15,04	14,20	13,87	14,81	14,24	13,81	14,50	14,22	13,93	13,56
	(2,38)	(2,45)	(2,36)	(2,24)	(2,33)	(2,34)	(2,47)	(2,40)	(2,30)	(2,30)	(2,25)
WORK	38478	32767	38276	41905	35634	30385	0	19285	44490	68507	130816
	(34945)	(29560)	(33801)	(38007)	(31552)	(20416)		(9967)	(5238)	(10040)	(46057)
STUD	42521	43165	42491	42170	16839	58242	40079	45240	44325	38183	20555
	(19687)	(19121)	(19594)	(20059)	(1076)	(3571)	(20762)	(18535)	(18167)	(19095)	(17265)
Intermission	0,13	0,12	0,13	0,14	0,10	0,03	0,13	0,09	0,11	0,19	0,42
# of Individuals	122372	31153	37102	54117	11521	49484	4032	66323	22443	16766	8126

Note: We only include students registered during spring 2001. The definition of fields of education is based on the field in which the student acquires the most credit points. Note that teachers can be found within all fields, which explains the low share of students within pedagogics. Incomes are displayed in SEK in prices from 2001(100 SEK is approximately 10 EUR).

Table A3. The estimated reform effect on credit points achieved each semester for students with only grants and students with grants and a maximum amount of student loans before the reform. OLS with individual FE.

	Only grants	Grants and loans
r	0.063***	-0.112***
	(0.019)	(0.008)
ONE*r	0.064***	0.070***
	(0.024)	(0.009)
BOTH*r	0.164***	0.152***
	(0.025)	(0.010)
S_2	-0.660***	-0.787***
	(0.016)	(0.011)
S_3	-1.235***	-1.553***
	(0.021)	(0.018)
S_4	-1.620***	-2.298***
	(0.029)	(0.026)
S_5	-1.925***	-3.048***
	(0.033)	(0.032)
S_6	-2.074***	-3.606***
	(0.038)	(0.038)
S_7	-2.514***	-4.281***
	(0.041)	(0.041)
S_8	-2.986***	-4.880***
	(0.045)	(0.043)
S_9	-2.992***	-4.969***
	(0.047)	(0.044)
S_10	-3.335***	-5.351***
	(0.049)	(0.045)
S_11	-3.215***	-5.235***
	(0.050)	(0.046)
S_12	-3.510***	-5.524***
	(0.052)	(0.046)
S_Fall	-0.414***	-0.385***
	(0.009)	(0.004)
cum_p	0.057***	0.060***
	(0.001)	(0.001)
cum_p^2	-0.000***	-0.000***
	(0.000)	(0.000)
Pedagogic	0.050	0.053***
	(0.035)	(0.012)
Humanities	-0.055*	-0.041***
	(0.029)	(0.009)
Science	-0.104***	-0.053***
	(0.023)	(0.008)
Technology	-0.097***	-0.019**
	(0.025)	(0.009)
Other	-0.020	0.039***
	(0.037)	(0.014)
Healthcare	0.050	0.035**
	(0.044)	(0.015)
Have children	-0.125***	-0.213***
	(0.041)	(0.017)
Local unempl. rates	0.050***	0.020***
	(0.003)	(0.001)
Constant	1.448***	1.819***
	(0.036)	(0.016)
Observations	105,162	418,018
R-squared	0.317	0.449
Number of id.	11,521	49,484
ll	-134241	-476610

Note: Robust standard errors are in parenthesis. *p<0.1, **p<0.05, p<0.001***.

Table A4. The estimated reform effect on credit points achieved each semester for students with only grants and students with grants and a maximum amount of student loans before the reform, including control for intermissions. OLS with individual FE.

	Only grants	Grants and loans
r	-0.017	-0.116***
	(0.012)	(0.006)
ONE*r	0.002	0.030***
	(0.015)	(0.007)
BOTH*r	0.045***	0.065***
	(0.016)	(0.007)
S_2	-0.274***	-0.671***
	(0.014)	(0.010)
S_3	-0.529***	-1.270***
	(0.018)	(0.016)
S_4	-0.730***	-1.851***
	(0.024)	(0.024)
S_5	-0.884***	-2.365***
	(0.027)	(0.029)
S_6	-0.945***	-2.710***
	(0.031)	(0.034)
S_7	-0.883***	-2.867***
	(0.034)	(0.038)
S_8	-1.232***	-3.304***
	(0.037)	(0.040)
S_9	-1.140***	-3.239***
	(0.038)	(0.041)
S_10	-1.397***	-3.550***
	(0.040)	(0.042)
S_11	-1.208***	-3.355***
	(0.039)	(0.043)
S_12	-1.470***	-3.640***
	(0.041)	(0.043)
S_Fall	-0.430***	-0.428***
	(0.009)	(0.004)
cum_p	0.034***	0.053***
	(0.001)	(0.001)
cum_p^2	-0.000***	-0.000***
	(0.000)	(0.000)
Pedagogic	0.044**	0.065***
	(0.021)	(0.009)
Humanities	-0.047***	-0.028***
	(0.018)	(0.007)
Science	-0.010	-0.013**
	(0.014)	(0.006)
Technology	-0.057***	-0.015**
	(0.016)	(0.007)
Other	0.012	0.039***
	(0.024)	(0.011)
Healthcare	0.044*	0.026**
	(0.023)	(0.010)
Have children	-0.011	-0.056***
	(0.026)	(0.013)
Local unempl. rates	0.011***	0.004***
	(0.002)	(0.001)
Intermissions	-1.769***	-1.521***
	(0.012)	(0.008)
Constant	2.111***	2.091***
	(0.025)	(0.013)
Observations	105,162	418,018
R-squared	0.555	0.576
Number of id.	11,521	49,484
ll	-111728	-422009

Note: Robust standard errors are in parenthesis. *p<0.1, **p<0.05, p<0.001**.

Table A5. The estimated reform effect on annual earnings for students with only grants and students with grants and a maximum amount of student loans before the reform, OLS with individual FE.

	Only grants	Grants and loans
r	-0.101***	0.102***
	(0.035)	(0.015)
ONE*r	-0.151***	-0.177***
	(0.045)	(0.019)
BOTH*r	-0.423***	-0.401***
	(0.046)	(0.019)
Y_2	0.304***	-0.108***
	(0.024)	(0.015)
Y_3	1.014***	0.551***
	(0.046)	(0.030)
Y_4	1.911***	1.803***
	(0.060)	(0.040)
Y_5	3.062***	3.182***
	(0.069)	(0.046)
Y_6	3.839***	4.149***
	(0.077)	(0.049)
cum_p	-0.033***	-0.015***
	(0.001)	(0.001)
cum_p^2	0.000***	0.000***
	(0.000)	(0.000)
Pedagogic	0.123**	0.127***
	(0.053)	(0.020)
Humanities	-0.012	0.032**
	(0.043)	(0.016)
Science	0.104***	0.107***
	(0.037)	(0.015)
Technology	-0.035	-0.090***
	(0.042)	(0.019)
Other	0.125*	-0.018
	(0.065)	(0.024)
Healthcare	-0.074	0.036
	(0.060)	(0.025)
Have children	-1.426***	-0.922***
	(0.090)	(0.040)
Local unempl. rates	-0.115***	-0.088***
	(0.005)	(0.002)
Constant	2.564***	2.166***
	(0.058)	(0.026)
Observations	57,885	236,400
R-squared	0.400	0.425
Number of id.	11,521	49,484
ll	-92390	-355496

Note: Robust standard errors are in parenthesis. *p<0.1, **p<0.05, p<0.001**.

Table A6. The estimated reform effect on annual earnings for students with only grants and students with grants and a maximum amount of student loans before the reform, including control for intermissions.OLS with individual FE.

	Only grants	Grants and loans
r	-0.044	0.073***
	(0.030)	(0.013)
ONE*r	-0.082**	-0.119***
	(0.039)	(0.017)
BOTH*r	-0.295***	-0.289***
	(0.040)	(0.017)
Y_2	-0.240***	-0.411***
	(0.023)	(0.014)
Y_3	-0.168***	-0.271***
	(0.045)	(0.028)
Y_4	0.340***	0.315***

	(0.059)	(0.039)
Y_5	0.828***	0.866***
	(0.071)	(0.045)
Y_6	1.383***	1.525***
	(0.078)	(0.049)
cum_p	-0.004***	-0.002***
	(0.001)	(0.001)
cum_p^2	0.000***	0.000***
	(0.000)	(0.000)
Pedagogic	0.108**	0.107***
	(0.046)	(0.018)
Humanities	-0.027	0.013
	(0.037)	(0.015)
Science	0.026	0.066***
	(0.032)	(0.014)
Technology	-0.053	-0.067***
	(0.036)	(0.017)
Other	0.097*	-0.008
	(0.055)	(0.022)
Healthcare	-0.031	0.062***
	(0.047)	(0.021)
Have children	-1.600***	-1.150***
	(0.086)	(0.039)
Local unempl. rates	-0.082***	-0.074***
	(0.004)	(0.002)
Intermissions	1.966***	1.995***
	(0.029)	(0.017)
Constant	1.930***	1.884***
	(0.051)	(0.023)
Observations	57,885	236,400
R-squared	0.509	0.511
Number of id.	11,521	49,484
ll	-86579	-336376

Note: Robust standard errors are in parenthesis. *p<0.1, **p<0.05, p<0.001**.

Table A7. The estimated reform effect on credit points achieved each semester for individuals with different earnings (E) before the reform. OLS with individual FE.

VARIABLES	E=0	0<E<37’	37’<E<55’	55’<E<92’	E>92’
r	-0.009	-0.049***	-0.038***	0.029*	0.026
	(0.030)	(0.008)	(0.013)	(0.015)	(0.019)
ONE*r	0.071*	0.071***	0.052***	0.039*	0.039
	(0.040)	(0.010)	(0.017)	(0.020)	(0.028)
BOTH*r	0.180***	0.161***	0.173***	0.133***	0.167***
	(0.041)	(0.010)	(0.018)	(0.023)	(0.035)
S_2	-0.621***	-0.688***	-0.696***	-0.738***	-0.751***
	(0.023)	(0.007)	(0.011)	(0.011)	(0.015)
S_3	-1.189***	-1.311***	-1.342***	-1.449***	-1.569***
	(0.031)	(0.009)	(0.017)	(0.016)	(0.018)
S_4	-1.640***	-1.852***	-1.890***	-1.942***	-1.972***
	(0.042)	(0.013)	(0.023)	(0.021)	(0.022)
S_5	-1.967***	-2.358***	-2.455***	-2.380***	-2.083***
	(0.047)	(0.015)	(0.026)	(0.023)	(0.024)
S_6	-2.259***	-2.679***	-2.737***	-2.532***	-2.177***
	(0.053)	(0.018)	(0.030)	(0.026)	(0.026)
S_7	-2.585***	-3.035***	-3.072***	-2.788***	-2.251***
	(0.058)	(0.020)	(0.032)	(0.027)	(0.027)
S_8	-2.948***	-3.462***	-3.484***	-3.090***	-2.422***
	(0.064)	(0.021)	(0.034)	(0.029)	(0.029)
S_9	-3.017***	-3.505***	-3.524***	-3.104***	-2.426***
	(0.066)	(0.022)	(0.036)	(0.030)	(0.029)
S_10	-3.386***	-3.877***	-3.851***	-3.393***	-2.569***
	(0.069)	(0.023)	(0.037)	(0.032)	(0.030)
S_11	-3.288***	-3.778***	-3.740***	-3.277***	-2.508***
	(0.071)	(0.023)	(0.038)	(0.032)	(0.030)
S_12	-3.570***	-4.106***	-4.020***	-3.540***	-2.645***

	(0.074)	(0.024)	(0.039)	(0.033)	(0.032)
S_Fall	-0.385***	-0.407***	-0.412***	-0.389***	-0.275***
	(0.013)	(0.004)	(0.006)	(0.007)	(0.008)
cum_p	0.058***	0.058***	0.057***	0.061***	0.065***
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
cum_p^2	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Pedagogic	0.061	0.048***	0.053**	0.028	0.021
	(0.081)	(0.014)	(0.022)	(0.028)	(0.047)
Humanities	-0.081*	-0.068***	-0.081***	-0.115***	-0.100***
	(0.042)	(0.010)	(0.018)	(0.022)	(0.037)
Science	-0.064*	-0.100***	-0.116***	-0.165***	-0.187***
	(0.036)	(0.008)	(0.015)	(0.019)	(0.032)
Technology	-0.059	-0.086***	-0.112***	-0.167***	-0.243***
	(0.040)	(0.010)	(0.018)	(0.023)	(0.037)
Other	-0.042	-0.009	-0.027	-0.048	-0.066
	(0.083)	(0.015)	(0.026)	(0.034)	(0.062)
Healthcare	0.102	0.097***	0.097***	0.152***	0.205***
	(0.076)	(0.017)	(0.027)	(0.033)	(0.052)
Have children	-0.360***	-0.309***	-0.342***	-0.336***	-0.237***
	(0.066)	(0.017)	(0.025)	(0.025)	(0.025)
Local unempl. rates	0.026***	0.029***	0.029***	0.023***	0.021***
	(0.004)	(0.001)	(0.002)	(0.002)	(0.003)
Constant	1.628***	1.687***	1.714***	1.695***	1.453***
	(0.056)	(0.014)	(0.024)	(0.028)	(0.039)
Observations	39,547	603,467	204,092	161,069	88,768
R-squared	0.291	0.313	0.333	0.310	0.319
Number of id.	4,032	66,323	22,443	16,766	8,126
ll	-51381	-773975	-261776	-211841	-114028

Note: Robust standard errors are in parenthesis. *p<0.1, **p<0.05, p<0.001**.

Table A8. The estimated reform effect on credit points achieved each semester for individuals with different earnings (E) before the reform, including intermissions. OLS with individual FE.

VARIABLES	E=0	0<E<37’	37’<E<55’	55’<E<92’	E>92’
r	-0.031	-0.080***	-0.081***	-0.055***	-0.033***
	(0.021)	(0.005)	(0.008)	(0.009)	(0.011)
ONE*r	0.042	0.021***	0.011	0.006	0.022
	(0.027)	(0.007)	(0.011)	(0.012)	(0.017)
BOTH*r	0.075***	0.052***	0.049***	0.024*	0.073***
	(0.028)	(0.007)	(0.012)	(0.014)	(0.021)
S_2	-0.250***	-0.361***	-0.328***	-0.183***	-0.016
	(0.023)	(0.006)	(0.010)	(0.011)	(0.016)
S_3	-0.520***	-0.692***	-0.636***	-0.428***	-0.228***
	(0.028)	(0.008)	(0.014)	(0.013)	(0.019)
S_4	-0.757***	-1.009***	-0.927***	-0.593***	-0.354***
	(0.037)	(0.011)	(0.019)	(0.018)	(0.021)
S_5	-0.912***	-1.270***	-1.174***	-0.772***	-0.398***
	(0.041)	(0.013)	(0.022)	(0.019)	(0.021)
S_6	-1.088***	-1.453***	-1.329***	-0.837***	-0.436***
	(0.047)	(0.015)	(0.025)	(0.021)	(0.023)
S_7	-1.144***	-1.434***	-1.259***	-0.789***	-0.406***
	(0.050)	(0.017)	(0.027)	(0.022)	(0.024)
S_8	-1.376***	-1.745***	-1.578***	-0.999***	-0.497***
	(0.055)	(0.018)	(0.029)	(0.024)	(0.025)
S_9	-1.314***	-1.647***	-1.483***	-0.895***	-0.467***
	(0.056)	(0.018)	(0.030)	(0.024)	(0.025)
S_10	-1.585***	-1.927***	-1.750***	-1.124***	-0.555***
	(0.059)	(0.019)	(0.031)	(0.025)	(0.026)
S_11	-1.433***	-1.744***	-1.543***	-0.939***	-0.479***
	(0.059)	(0.019)	(0.031)	(0.025)	(0.025)
S_12	-1.670***	-2.031***	-1.818***	-1.178***	-0.586***
	(0.062)	(0.020)	(0.032)	(0.026)	(0.027)
S_Fall	-0.394***	-0.434***	-0.456***	-0.404***	-0.264***
	(0.014)	(0.004)	(0.006)	(0.007)	(0.007)

cum_p	0.034***	0.038***	0.035***	0.029***	0.028***
	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)
cum_p^2	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Pedagogic	0.047	0.063***	0.068***	0.029*	0.066**
	(0.049)	(0.009)	(0.014)	(0.016)	(0.028)
Humanities	-0.041	-0.041***	-0.042***	-0.064***	-0.073***
	(0.029)	(0.007)	(0.011)	(0.013)	(0.023)
Science	-0.013	-0.026***	-0.024**	-0.064***	-0.051***
	(0.025)	(0.006)	(0.009)	(0.011)	(0.018)
Technology	-0.038	-0.055***	-0.061***	-0.096***	-0.109***
	(0.028)	(0.006)	(0.011)	(0.013)	(0.021)
Other	-0.035	0.007	-0.002	-0.014	-0.008
	(0.056)	(0.011)	(0.017)	(0.020)	(0.035)
Healthcare	0.041	0.048***	0.054***	0.069***	0.093***
	(0.047)	(0.010)	(0.015)	(0.017)	(0.026)
Have children	-0.167***	-0.089***	-0.105***	-0.098***	-0.097***
	(0.045)	(0.012)	(0.017)	(0.015)	(0.015)
Local unempl. rates	0.008***	0.007***	0.003***	0.001	0.002
	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)
Intermissions	-1.533***	-1.659***	-1.730***	-1.827***	-1.783***
	(0.019)	(0.005)	(0.008)	(0.009)	(0.012)
Constant	2.031***	2.140***	2.238***	2.233***	1.910***
	(0.041)	(0.010)	(0.017)	(0.018)	(0.024)
Observations	39,547	603,467	204,092	161,069	88,768
R-squared	0.497	0.527	0.563	0.589	0.608
Number of id.	4,032	66,323	22,443	16,766	8,126
ll	-44597	-661692	-218681	-170070	-89462

Note: Robust standard errors are in parenthesis. *p<0.1, **p<0.05, p<0.001**.

Table A9. The estimated reform effect on annual earnings for individuals with different earnings (E) before the reform.OLS with individual FE.

VARIABLES	E=0	0<E<37’	37’<E<55’	55’<E<92’	E>92’
r	-0.079	0.046***	0.087***	-0.066**	0.043
	(0.051)	(0.014)	(0.024)	(0.029)	(0.046)
ONE*r	-0.061	-0.195***	-0.226***	-0.126***	0.067
	(0.066)	(0.018)	(0.031)	(0.039)	(0.067)
BOTH*r	-0.372***	-0.440***	-0.497***	-0.289***	-0.229***
	(0.058)	(0.017)	(0.033)	(0.043)	(0.084)
Y_2	0.064***	0.078***	0.059***	0.280***	1.289***
	(0.024)	(0.009)	(0.018)	(0.021)	(0.032)
Y_3	0.207***	0.626***	1.060***	1.704***	2.794***
	(0.052)	(0.017)	(0.036)	(0.038)	(0.049)
Y_4	0.748***	1.571***	2.238***	2.715***	3.059***
	(0.074)	(0.024)	(0.046)	(0.046)	(0.057)
Y_5	1.566***	2.620***	3.302***	3.623***	3.499***
	(0.092)	(0.028)	(0.052)	(0.053)	(0.064)
Y_6	2.300***	3.464***	4.035***	4.235***	3.865***
	(0.103)	(0.031)	(0.056)	(0.057)	(0.069)
cum_p	-0.016***	-0.019***	-0.032***	-0.053***	-0.070***
	(0.002)	(0.000)	(0.001)	(0.001)	(0.002)
cum_p^2	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Pedagogic	-0.038	0.128***	0.143***	0.165***	-0.192
	(0.091)	(0.019)	(0.034)	(0.044)	(0.147)
Humanities	0.030	0.041***	0.075***	0.117***	0.018
	(0.052)	(0.014)	(0.029)	(0.038)	(0.078)
Science	0.104**	0.125***	0.147***	0.084**	0.169***
	(0.052)	(0.013)	(0.025)	(0.033)	(0.065)
Technology	0.108*	-0.002	-0.012	-0.013	-0.041
	(0.060)	(0.015)	(0.031)	(0.040)	(0.077)
Other	-0.018	0.047**	0.050	0.056	-0.129
	(0.109)	(0.023)	(0.044)	(0.055)	(0.126)
Healthcare	0.074	-0.043*	-0.073*	-0.119**	-0.380***

	(0.093)	(0.022)	(0.039)	(0.048)	(0.090)
Have children	0.084	-0.750***	-1.185***	-1.232***	-1.168***
	(0.124)	(0.033)	(0.051)	(0.056)	(0.073)
Local unempl. rates	-0.088***	-0.087***	-0.100***	-0.096***	-0.108***
	(0.007)	(0.002)	(0.003)	(0.004)	(0.006)
Constant	1.137***	1.721***	2.798***	3.563***	4.679***
	(0.091)	(0.021)	(0.040)	(0.049)	(0.086)
Observations	21,296	333,650	112,714	87,450	46,038
R-squared	0.358	0.406	0.369	0.306	0.240
Number of id.	4,032	66,323	22,443	16,766	8,126
ll	-31038	-501468	-184793	-155197	-93881

Note: Robust standard errors are in parenthesis. *p<0.1, **p<0.05, p<0.001**.

Table A10. The estimated reform effect on annual earnings for individuals with different earnings (E) before the reform, including intermissions. OLS with individual FE.

VARIABLES	E=0	0<E<37’	37’<E<55’	55’<E<92’	E>92’
r	-0.069	0.051***	0.103***	0.001	0.096**
	(0.048)	(0.012)	(0.022)	(0.026)	(0.043)
ONE*r	-0.027	-0.143***	-0.173***	-0.088***	0.065
	(0.062)	(0.016)	(0.027)	(0.034)	(0.060)
BOTH*r	-0.320***	-0.345***	-0.362***	-0.180***	-0.133*
	(0.055)	(0.015)	(0.029)	(0.038)	(0.076)
Y_2	-0.239***	-0.319***	-0.493***	-0.457***	0.321***
	(0.028)	(0.009)	(0.018)	(0.022)	(0.036)
Y_3	-0.469***	-0.298***	-0.212***	0.036	0.801***
	(0.060)	(0.018)	(0.036)	(0.040)	(0.063)
Y_4	-0.161*	0.216***	0.347***	0.547***	0.851***
	(0.082)	(0.024)	(0.046)	(0.050)	(0.067)
Y_5	0.374***	0.807***	0.891***	1.011***	1.073***
	(0.098)	(0.029)	(0.054)	(0.057)	(0.074)
Y_6	0.942***	1.425***	1.397***	1.417***	1.324***
	(0.109)	(0.032)	(0.058)	(0.061)	(0.079)
cum_p	0.000	0.002***	-0.004***	-0.016***	-0.026***
	(0.002)	(0.000)	(0.001)	(0.001)	(0.002)
cum_p^2	0.000***	0.000**	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Pedagogic	-0.052	0.111***	0.108***	0.122***	-0.244*
	(0.083)	(0.017)	(0.030)	(0.039)	(0.141)
Humanities	-0.001	0.015	0.046*	0.069**	-0.011
	(0.049)	(0.013)	(0.025)	(0.034)	(0.072)
Science	0.070	0.068***	0.053**	0.003	0.031
	(0.048)	(0.012)	(0.022)	(0.029)	(0.058)
Technology	0.104*	-0.015	-0.041	-0.049	-0.153**
	(0.056)	(0.014)	(0.027)	(0.034)	(0.069)
Other	-0.025	0.029	0.028	-0.002	-0.218*
	(0.104)	(0.020)	(0.038)	(0.048)	(0.118)
Healthcare	0.132	0.000	-0.008	-0.046	-0.272***
	(0.090)	(0.019)	(0.031)	(0.037)	(0.076)
Have children	-0.049	-0.974***	-1.451***	-1.501***	-1.320***
	(0.121)	(0.032)	(0.049)	(0.052)	(0.068)
Local unempl. rates	-0.079***	-0.072***	-0.080***	-0.078***	-0.091***
	(0.006)	(0.001)	(0.003)	(0.004)	(0.006)
Intermissions	1.055***	1.576***	1.933***	2.058***	2.119***
	(0.043)	(0.012)	(0.020)	(0.023)	(0.037)
Constant	0.894***	1.374***	2.342***	3.048***	4.180***
	(0.086)	(0.019)	(0.036)	(0.044)	(0.079)
Observations	21,296	333,650	112,714	87,450	46,038
R-squared	0.408	0.489	0.474	0.417	0.326
Number of id.	4,032	66,323	22,443	16,766	8,126
ll	-30174	-476352	-174577	-147553	-91115

Note: Robust standard errors are in parenthesis. *p<0.1, **p<0.05, p<0.001**.

Table A11. Sensitivity analysis. The estimated reform effect on credit points achieved each semester. OLS with individual FE.

	Baseline: Specification 7, from Table 1	Including individuals with missing parental information	Including only individuals less than 30 years of age at enrollment	Unrestricted control for cumulative credit points: dummy variables
r	-0.013**	-0.004	-0.007	0.004
	(0.006)	(0.005)	(0.006)	(0.006)
ONE*r	0.044***	0.034***	0.039***	0.055***
	(0.008)	(0.007)	(0.008)	(0.007)
BOTH*r	0.126***	0.117***	0.120***	0.152***
	(0.008)	(0.008)	(0.008)	(0.007)
S_2	-0.666***	-0.661***	-0.669***	-1.019***
	(0.004)	(0.004)	(0.004)	(0.006)
S_3	-1.275***	-1.271***	-1.276***	-1.621***
	(0.006)	(0.006)	(0.006)	(0.008)
S_4	-1.772***	-1.763***	-1.775***	-1.907***
	(0.008)	(0.008)	(0.008)	(0.010)
S_5	-2.195***	-2.184***	-2.200***	-2.271***
	(0.010)	(0.009)	(0.010)	(0.011)
S_6	-2.450***	-2.434***	-2.458***	-2.499***
	(0.011)	(0.010)	(0.011)	(0.012)
S_7	-2.718***	-2.703***	-2.722***	-2.799***
	(0.012)	(0.011)	(0.012)	(0.012)
S_8	-3.099***	-3.081***	-3.102***	-3.172***
	(0.013)	(0.012)	(0.013)	(0.013)
S_9	-3.113***	-3.095***	-3.115***	-3.193***
	(0.013)	(0.013)	(0.013)	(0.014)
S_10	-3.442***	-3.422***	-3.447***	-3.514***
	(0.014)	(0.013)	(0.014)	(0.014)
S_11	-3.330***	-3.310***	-3.335***	-3.410***
	(0.014)	(0.013)	(0.014)	(0.014)
S_12	-3.622***	-3.599***	-3.633***	-3.697***
	(0.014)	(0.014)	(0.015)	(0.015)
S_Fall	-0.414***	-0.412***	-0.414***	-0.404***
	(0.002)	(0.002)	(0.002)	(0.002)
cum_p	0.056***	0.056***	0.057***	
	(0.000)	(0.000)	(0.000)	
cum_p^2	-0.000***	-0.000***	-0.000***	
	(0.000)	(0.000)	(0.000)	
c_2				1.388***
				(0.006)
c_3				1.958***
				(0.008)
c_4				2.397***
				(0.008)
c_5				2.684***
				(0.009)
c_6				2.954***
				(0.010)
c_7				3.038***
				(0.011)
c_8				3.108***
				(0.011)
c_9				3.238***
				(0.012)
c_10				3.394***
				(0.013)
c_11				3.244***
				(0.013)
c_12				3.705***
				(0.013)
Pedagogic	0.038***	0.036***	0.037***	0.040***
	(0.011)	(0.010)	(0.011)	(0.010)
Humanities	-0.079***	-0.078***	-0.081***	-0.096***
	(0.008)	(0.008)	(0.008)	(0.008)
Science	-0.114***	-0.114***	-0.115***	-0.124***

	(0.007)	(0.006)	(0.007)	(0.006)
Technology	-0.105***	-0.104***	-0.104***	-0.083***
	(0.008)	(0.008)	(0.008)	(0.007)
Other	-0.018	-0.025**	-0.018	-0.024**
	(0.012)	(0.012)	(0.012)	(0.011)
Healthcare	0.109***	0.110***	0.111***	0.121***
	(0.012)	(0.012)	(0.013)	(0.012)
Have children	-0.253***	-0.251***	-0.251***	-0.265***
	(0.011)	(0.011)	(0.012)	(0.011)
Local unempl. rates	0.034***	0.034***	0.035***	0.039***
	(0.001)	(0.001)	(0.001)	(0.001)
Constant	1.631***	1.627***	1.616***	1.433***
	(0.010)	(0.010)	(0.011)	(0.010)
Observations	1,135,649	1,219,021	1,089,466	1,135,649
R-squared	0.299	0.299	0.298	0.326
Number of id.	122,372	130,933	117,251	122,372
ll	-1.474e+06	-1.584e+06	-1.416e+06	-1.452e+06

Note: Robust standard errors are in parenthesis. *p<0.1, **p<0.05, p<0.001**. c_1 – c_12 are dummy variables for total credit points achieved at each semester. The cumulative credit points are split into 10 credit point intervals, (i.e., 0-10, 11-20,..., 120-).

Figures

Figure A1. The distribution of age at enrollment

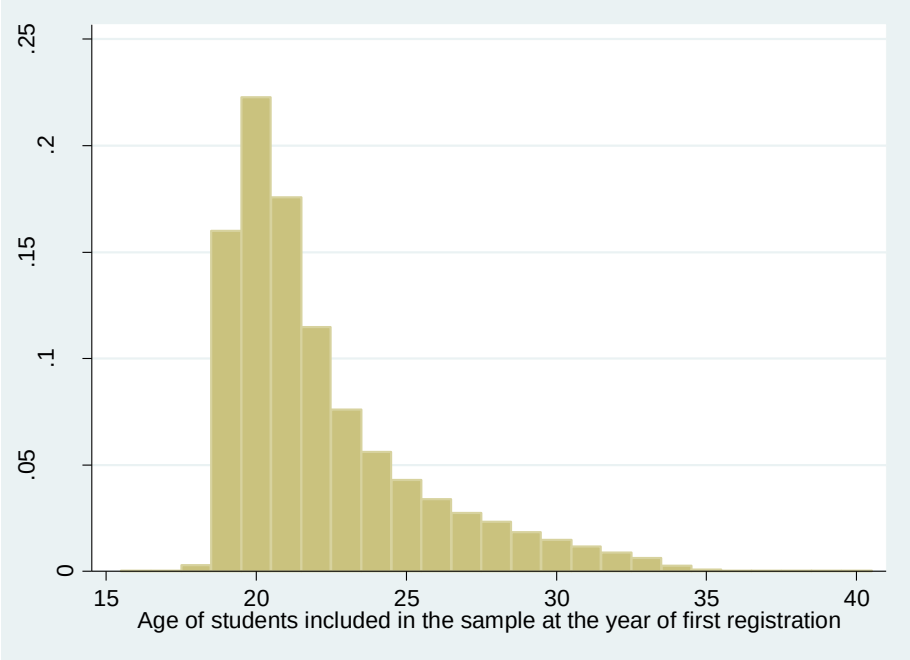
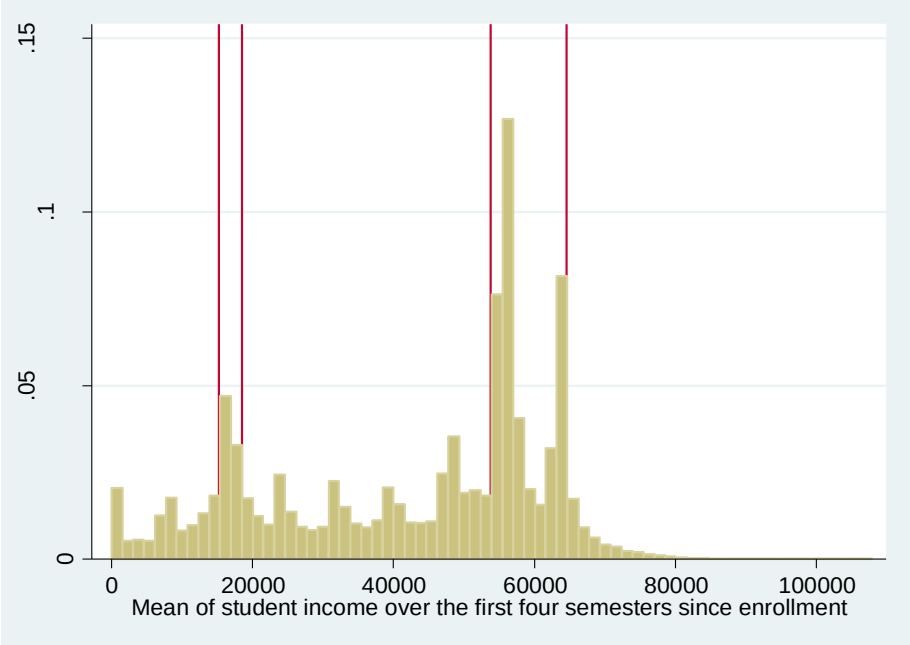
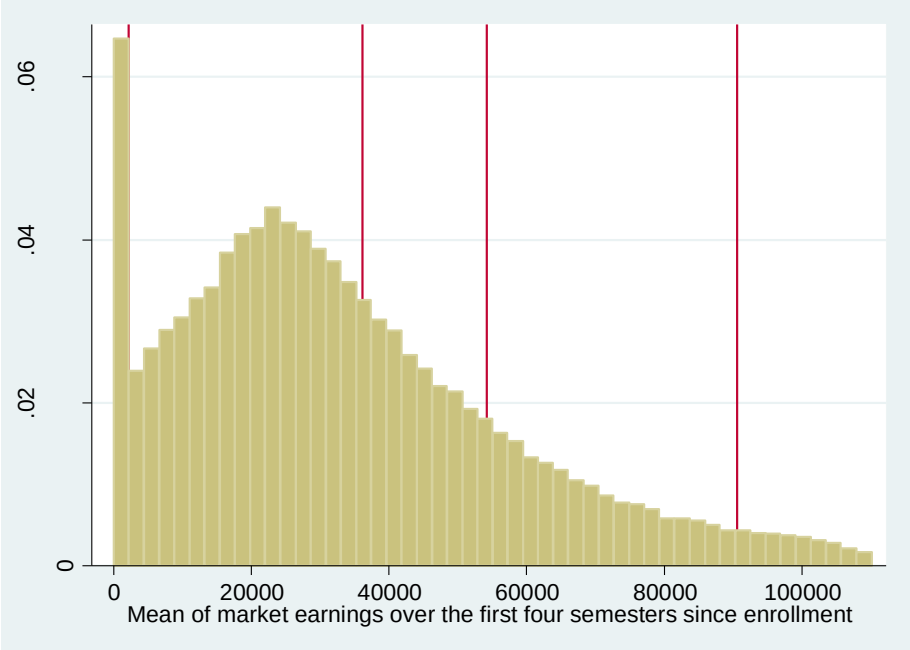


Figure A2. The mean amount of student aid during the first two years of study in SEK.



Note: Incomes are displayed in SEK, in 2001 years prices (100 SEK is approximately 10 EUR). The vertical lines indicate the thresholds for the sub-sample analysis.

Figure A3. The mean earnings during the first two years of study in SEK.



Note: Incomes are displayed in SEK, in prices from 2001(100 SEK is approximately 10 EUR). The vertical lines indicate the thresholds for the sub-sample analysis.

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