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Financial incentives and study duration in higher education

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Abstract:
This paper investigates to which extent students in higher education respond to financial incentives by adjusting their study behavior. Students in Norway who completed certain graduate study programs between autumn 1990 and 1995 on stipulated time were entitled to a restitution of approximately 3,000 USD from the Norwegian State Educational Loan Fund. Comparing treated and untreated (control) programs in a difference-in-difference framework, we find that the average delay in the treatment group decreased by on average 0.8 semester during the reform period, and by 1.5 semesters in the following two years. Number of years treated matter strongly, with delays reduced by 0.23 semesters per year treated. Furthermore, there is some indication that it is important that treatment starts before the final part of the educational programs. The share of on-time graduation increases by 3.8 percentage points per year treated, from a pre-reform level of about 20 percent. Thus, a large share of the restitutions given will be for students who would otherwise not have graduated on time. A series of robustness checks indicate that our estimated effects do not reflect differential trends or omitted variables.

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| **Discussion Papers** | comprise research papers intended for international journals or books. A preprint of a Discussion Paper may be longer and more elaborate than a standard journal article, as it may include intermediate calculations and background material etc. |

Sammendrag

I denne artikkelen studerer vi effekten av finansielle incentiver på studentatferd i høyere utdanning. Studenter i Norge som fullførte enkelte studier på normert tid mellom høsten 1990 og 1995 fikk en reduksjon i studielånet på 18 000 kr fra Statens lånekasse for utdanning. Ved å sammenligne studenter ved berørte og ikke berørte studier (ved bruk av en forskjeller-i-forskjeller metode), finner vi at berørte studenter i snitt reduserte antall semester forsinket med 0,8 i reformperioden og med 1,5 semestre i de påfølgende to årene etter reformen. Vi finner at hvert studieår under reformen reduserte antall semester forsinket med 0,23. I tillegg har vi noe indikasjon på at bruken av finansielle incentiver er mest effektive når de berører studenter tidlig i utdanningsløpet. Hva angår fullføring på normert tid finner vi at andelen økte med 3,8 prosent poeng for hvert reform år, fra et nivå på 20 prosent i perioden før reformen. En rekke robusthetssjekker tyder på at den estimerte effekten ikke gjenspeiler ulike trender eller utelatte variable.

1 Introduction

Because education is believed to have positive externalities, and to promote equality of opportunity, higher education is subsidized in many countries. This is the case wherever students do not pay the full cost of their instruction through tuition. Moreover, several countries have even stronger subsidies in that students' living expenses are also partly covered, either through scholarships or through favorable student loans provided by government agencies. From human capital theory, we would expect subsidies to increase the net return to education and help to offset credit constraints. However, the presence of subsidies to education may not only increase students' attainment level, but also influence the level of effort provided by students. As students are generally subsidized for each unit of time spent studying, and not for the degree attained, there may be incentives to spend too much time in the educational system. This may be particularly important if the consumption value, i.e., the private, non-pecuniary return to education, is a dominant factor behind the students' choice of study duration (Alstadsæter and Sivertsen, 2010; Zafar, 2009). In this case, a higher level of student support may finance increased consumption of higher education, with few externalities.

It is indeed observed that many students enrolled in universities and college programs around the world do not complete their university or college degree on time. According to the U.S. Department of Education (2003), first-time recipients of bachelor's degrees between 1999 and 2000 spent on average 10 extra months finishing their degree beyond the estimated completion time. Similar patterns are documented for many European countries (Brunello and Winter-Ebmer, 2003). This result, together with the general belief that students do not exert sufficient study effort, has increased researchers' interest in whether students respond to financial incentives. The evidence in this area is mixed and remains limited.

This paper studies the effects of financial incentives on study duration using rich register data to investigate the effect of a reform that rewarded students who completed their higher education degree nominally on time. The reform entitled students in Norway who completed certain graduate study programs between the autumn semester of 1991 and the autumn semester of 1995 to a restitution of approximately 18,000 NOK (about 3000 USD and 34 percent of the average yearly loan) from the Norwegian State Educational Loan Fund if they finished the program on nominal time. This reform was among the first to focus on the intensive margin, explicitly aiming at inducing students to succeed and thereby improve the efficiency of higher education. Earlier reforms had only been concerned with the design of students' support system (loans and grants) related to the extensive margin, such as increasing enrollment and access to higher education by providing a subsidy to all students independent of performance.

The reform created sharp discontinuities in the financial incentives that the autumn 1990 to 1995 graduation cohorts faced compared to previous and subsequent cohorts. These discontinuities can be exploited to estimate the impact of the financial reward on study duration. Similar to all research designs that depend on a reform, confounding time effects are a potential threat. However, the fact that students enrolled in some education programs were not eligible for the

restitution provides an additional comparison group that will allow a difference-in-differences approach that can control for such confounding time effects.

This paper contributes to the literature by being one of the few papers addressing the causal effect of financial incentives on study duration among students at the university level. Moreover, it includes the whole student population in Norwegian higher education institutions. Previous papers with a credible research design have typically only focused on students from one particular field of study or university. Finally, it is also the first paper to directly address number of semesters delayed as dependent variable (previous papers have focused on graduation on time and student achievement).

The remainder of this paper is organized as follows: Section 2 reviews the related literature on study duration in higher education. Section 3 provides some background on the higher education system in Norway, the student support system and the financial incentive reform. Section 4 presents the data, while section 5 outlines the empirical strategy. Section 6 presents the findings and section 7 offers some conclusions.

2   Related Literature

The empirical literature on study duration in higher education has mainly focused on two issues: (i) The relationship between student aid and the demand side for higher education, i.e., the extensive margin and how financial subsidies can increase enrollment and investment in higher education, (ii) How to improve the effectiveness of higher education production by giving students financial incentives related to nominal study duration or academic performance.

Governments' student loans and grants make it easier for students to obtain higher education. Many studies have been conducted to measure the effect of these student-aid programs. For instance, Dynarski (2003) finds that college attendance dropped by more than one-third of a year and schooling by two-thirds of a year after a shift in the financial aid policy in the United States in 1992 in which Congress eliminated the Social Security student benefit program. Dynarski (2004) also studies the effect of new scholarships in the United States. Whereas traditional scholarships are often limited to high-performing students, new merit aid programs in the United States require relatively modest academic credentials. Dynarski (2004) provides evidence that most of these new merit aid programs have contributed to close racial and ethnic gaps in university attendance.

Skyt-Nielsen, Sørensen and Taber (2010) investigate the change in demand for college resulting from a Danish student aid reform. Their findings indicate that enrollment increases with higher subsidies, although enrollment is less responsive than has been reported in other studies and countries. They argue that one reason for this difference may be that large subsidies were in place in Denmark prior to the reform. Borrowing constraints seem to deter college enrollment only to a minor extent.

More recently, the potential of financial incentives to increase students' study efficiency

and performance has attracted attention. Financial incentives may be implemented through direct money incentives, reduction in tuition fees or forgiveness of student loans depending on academic results, or in our case, the time to degree. The literature in this domain is relatively small and scattered. Leuven, Oosterbeek and Van der Klaauw (2010) implement a randomized experiment among first-year economics students in Amsterdam where students who passed all of their first-year requirements on time could earn a reward of 750 Euros. This incentive increased performance for higher-ability students, but they also find a negative effect for less able students. Using a regression discontinuity design on data from Bocconi University in Italy, Garibaldi, Giavazzi, Ichino and Rettore (2007) show that if tuition in the last year of the program is raised by 1,000 Euros, the probability of late graduation decreases by 6.1 percentage points with respect to a benchmark average probability of 80 percent. Common for these two latter studies only use data for one particular university or field of study.

More in line with our study, Häkkinen and Uusitalo (2003) evaluate the effect of a student aid reform in Finland that was intended to shorten the duration of university studies. The reform relied on a new system that replaced the old loan-based student aid system with a system of grants. The reform had only a modest effect that was limited to fields with relatively long durations of education. Furthermore, most of the decline in the observed time to degree can be explained by an increase in the unemployment rate that reduced student employment opportunities. In the same spirit, Heineck, Kifmann and Lorenz (2006) apply a duration analysis to examine the effects on study duration of an additional tuition fee for students enrolled in university programs (in Germany) beyond the regular completion time. Their findings are ambiguous, however. Unlike our study, both Häkkinen and Uusitalo and Heineck et al. cannot fully control for confounding time factors as they only compare students before and after the reform.

There is also some evidence that observed excess time to graduation may be explained by labor market variables (such as wage differentials and employment protection) and attributes of the funding scheme of tertiary education. By using data on European countries, Brunello and Winter-Ebmer (2003) find a negative association between wage compression completing college on time, and that excess time to graduation is significantly higher in countries with stricter employment protection. Bound, Lovenheim and Turner (2007) provide evidence that increased stratification in U.S. higher education and reduced availability of resources to institutions below the top tier, are the main explanations for the observed increase in time to degree. Joensen (2011) establishes a structural model where students may study and work simultaneously. She finds that there are non-linear effect of students' working hours on academic achievement, and bonuses related to merit aid or on-time-graduation can be effective to amend academic outcomes such as graduation rates and time-to-graduation.

Not much is known about the optimal length of studying to obtain a certain degree. One exception is Brodaty, Gary-Bobo and Prieto (2008) who provide evidence that individuals in France with longer than average time-to-graduation have significantly lower wages and employment rates in their early career, indicating that speed signals ability.

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3 Institutional settings and the "turbo" reform

3.1 Higher education in Norway

The Norwegian higher education sector is almost completely dominated by public institutions, which have 85 percent of enrolled students. Tuition fees are virtually zero, making the direct costs of higher education very low.[1] There are three different types of higher education institutions: universities, specialized university colleges and regional university colleges. All three types offer courses at both the undergraduate and graduate levels.

During the 1990s, undergraduate programs typically lasted up to four years, and most graduate programs had a total duration of five to six years. Most students at regional colleges enrolled in two- or three-year professionally oriented programs (e.g., nursing, teaching, engineering and commerce), whereas students in specialized university colleges mostly enroll in four- to six-year programs in specialized fields, such as business, architecture and veterinary science. Universities used to offer two tracks: integrated five- or six-year programs leading to a graduate degree (e.g., medicine, theological seminary and civil engineering) or shorter programs in different fields that could be combined to eventually earn a Master's degree. Such a Master's degree typically consisted of two parts; a relevant undergraduate degree which lasted for three to four years, and a graduate degree with a duration of one and a half or two years. Thus, the total stipulated duration of these degrees, including the undergraduate degree, was five or six years. This latter study program bears some resemblance to the American university system, although there was no "core curriculum" for undergraduates in Norway. Students in Norway who wished to begin a graduate program had to complete a related undergraduate program.[2]

3.2 The Norwegian state educational loan fund

To further promote equality of opportunity in higher education irrespective of family background, the Norwegian State Educational Loan Fund offers favorable loans to students who enroll in higher education programs. This type of support is meant to cover the students' costs of living, such as housing and food, during the study period. The loan terms are favorable in several respects. No interest is calculated, and no repayment is required until the student has completed his/her education and entered the labor market. Also, the loan may be fully or partially waived if the student, for one or another reason, does not have sufficient income after completing his/her education. In the case of death, the loan is waived.

The Norwegian Parliament decides every year how much money to assign to students during the subsequent school year, generally adjusting this amount to keep up with students' costs

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[1] The single important exception to this rule is a private business school that accounts for about 10 percent of the students and charges significant tuition fees.

[2] Since 2003, following the Bologna reform, most educational programs have been streamlined into three-year Bachelor's degrees and five-year Master's degrees. Moreover, the formal distinction between specialized and regional university colleges is recent, but reflects a difference that was also present during the 1990s. Also, since 2005 the number of universities has increased from four to eight through the conversion of one specialized university institutions and three regional university colleges.

of living. This sum, which amounted to 54,000 NOK (about 9,000 USD) for the 1991/1992 academic year (about 42,000 NOK of this sum was given as a loan, and the remaining 12,000 NOK was a grant),[3] is the same for all students and is not affected by parental income. On the other hand, financial support has for long been need-based and depends on students' own income and wealth.

The grant and interest benefit represent "free money" to the students, hence the fraction of students in higher education who take up loans is close to 100 percent. Berg (1997) reports that 97 percent of students graduating with a higher degree receive support from the Norwegian State Educational Loan Fund at some time. The average loan amount per student was approximately 155,000 NOK for students completing higher education in 1994. Note that this is an average for student with shorter and higher degrees. For the students we consider, who have higher degrees, the average loan is likely to be higher.

3.3   The "turbo" reform

Students in Norway who completed certain graduate education programs between autumn 1990 and autumn 1995 were entitled to restitution from the Norwegian State Educational Loan fund if they graduated on stipulated time. The restitution was 18000 NOK (about 3000 USD). The reform was announced as a part of the 1991 National Budget. The proposed budget was made public in October 1990, and passed in November/December. Searching newspaper archives, we have found no indication that the reform was expected or even discussed before announced in the National Budget. On the contrary, according to newspaper reports there were even some uncertainties related to the implementation of the reform. The political situation in Norway was unclear during the autumn 1990. The center-right minority government that had proposed the restitution ceded office to the Labor party in early November, i.e., before the 1991 National Budget was passed. The Labor party apparently was opposed to the reform, but voted in favor of it by a mistake. The new system was introduced in the regulations for the State Educational Loan Fund from July 1991. Students graduating on stipulated time in autumn 1990 and spring 1991 benefited from the new incentive scheme.

The termination of the reform was announced in summer 1995, and the last students who could benefit from the reform was the autumn 1995 graduation cohort.[4] The reform was controversial throughout its lifetime, and even proposed discontinued in the 1994 National Budget, but at that time continued by the national assembly. Until its termination the future of the reform was uncertain. Shortly before it was discontinued it was expected that a similar policy would be introduced in its place. However, this did not happen. Thus, it is unclear what expectations students during the reform period had about the future of the reform.

The reform created sharp discontinuities in the financial incentives that the autumn 1990 to autumn 1995 graduation cohorts faced compared to previous and subsequent cohorts. We

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[3]Source: This figure and the following figures concerning loans and grants are taken from the website of the Norwegian State Educational Loan Fund, http://www.lanekassen.no/, unless stated otherwise.

[4]See White Paper number 14 (1993-94).

will exploit these discontinuities to estimate the impact of financial reward on study duration. The autumn 1990 graduates had little time to respond to the changed incentives, and it was at that time still unclear whether the reform would actually be implemented. For this reason we denote the period from spring 1991 to autumn 1995 as the treatment period. Conveniently for our analysis, students in some education programs were not eligible for the restitution (see below). This rule provides a comparison group that can be used in a difference-in-differences approach that corrects for confounding time effects.

From 1988 to 2003, students who opted for any longer study programs lasting 10- to 13-semesters were entitled to another restitution that was not linked to time to degree, but degree completion. The restitution was increasing with the length of the study program, ranging from around 19 000 NOK for 10-semester programs to 46 000 NOK for 13-semester programs. However, as this reform affected all students equally, we do not expect it to bias our difference-in-differences estimate.

3.4 What should we expect of this reform?

The extent to which students respond to such a reform depends on the design of the financial incentive: The reform entitled students who completed certain graduate study programs to a restitution of 18,000 NOK from the Norwegian State Educational Loan Fund if they completed their studies on stipulated time. The reward was hence received after at least five years of studying and corresponded (for the average student) to about 10 percent of the total loan amount.

Building on incentive theory, we know that incentives are more likely to be effective when the award are given on shorter terms. That is, when the reward comes more quickly, perhaps at the end of every term, and not after more than five years in the higher education system, as in our case (Holmström and Milgrom, 1987). In addition, the form taken by the subsidy itself - no direct cash reward, but a loan reduction (where the remainder is repaid and discounted over several years) - might be perceived as a somewhat low-powered incentive.

On the other hand, students might be quite constrained financially, that is, 18, 000 NOK in loan reduction is perceived as a larger sum of money than it is for an average worker, and hence students might be more willing to respond to the incentive than other types of individuals. More-over, the educational outcome being incentivized, i.e., study duration, might be something that students can quite easily adjust without suffering excessive effort costs. In addition, students understand the mapping between study effort and the study duration, so the responsiveness of the students' effort to the incentive should be quite good.

4 Data and Descriptive Statistics

We use register data from Statistics Norway, consisting of all students who were predicted (or expected) to graduate from Norwegian higher education institutions between 1983 and 1997.

The data source is the Norwegian National Education Database. The database builds on data from the 1960 and 1970 censuses, and has been continually updated since 1974. From 1974 onwards, the data are reported directly from the educational institutions to Statistics Norway and are thus considered to be very accurate. However, a large number of individuals have 1974 or 1975 as their registered first entry into higher education, even though they in reality started earlier. To get accurate measures of time of enrollment, graduation and semesters delayed, we begin our sample with students who were predicted to graduate in 1983. These students enrolled in higher education in 1976 or later. As the the admission system to higher education changed in 1993, the last students we include in our sample are the predicted 1997-graduates (who enrolled in 1992 or earlier).

In the remainder of the paper we will denote predicted graduation year simply as graduation year. An individual student's graduation year is then his or her year of first entry in higher education plus the length of the education program the student enrolled in. For each student we have information on whether the student completed a higher degree or not, and in case of completion, whether graduation was on time, and number of semesters delayed. Note that graduation on time and semesters delayed relate to the completed program, which may differ from the one the student enrolled in. These variables are calculated by comparing the stipulated duration of the completed program with the total time spent studying. Thus, in the case of students changing programs, semesters delayed do not only include those spent on the final program. Not completed means that we do not have any record of the student completing a higher degree. We observe the students until 2007, i.e., at least ten more years after predicted graduation.

Students who are not observed to complete get the value zero on the on-time completion variable. Furthermore, we truncate number of semesters delayed at the 5th and 95th percentile. These correspond to -2, i.e., 2 semesters before stipulated completion, and 12 semesters, respectively. This truncates a small number of positive and negative outliers, that may be the result of coding errors. We do not expect this to have much influence on our results, as relatively few observations are affected, and even fewer by more than a few semesters. Furthermore, the incentive is unlikely to have much impact when the student already is finishing on or before time, or much delayed. Estimating the reform effect for our truncated variable, the original untruncated and two alternative truncated variables confirms that this has little impact on our results. However, setting an upper limit to the number of semesters delayed allows consistent treatment of students who did not complete any higher education. We consider these to be maximum delayed (and impute using the 95th percentile).

In addition to data on enrollment in and completion of higher education, we also have background characteristics such as the student's age, gender and parental education.[5] Students who were younger than 18 or older than 21 when graduating from high school (about 30 percent of the sample) are excluded from the sample. About 76 percent of these are dropped due to

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[5]As non-Western immigrants amount to less than 1 percent of the sample, we choose not to control for immigrant status.

Table 1: Distribution of students across the different education programs

	Length of ed program (Years)	No of students	Percent
TREATMENT GROUP (N=36,377)			
Science (cand.scient)	5.5	8,736	24.02
Humanities (cand.philo)	6	8,194	22.53
Law (cand.jur)	6	8,146	22.39
Social sciences (cand.polit)	6	5,736	15.77
Psychology (cand.psychol)	6.5	1,820	5.00
Dentistry (cand.odont)	5	1,385	3.81
Theological seminar (cand.theol)	6	1,197	3.29
Economics (cand.oecon)	5.5	1,081	2.97
Arts (music) (cand.musicae)	6	82	0.23
CONTROL GROUP (N=9,989)			
Medicine (cand.med)	6	4,505	45.10
Agronomy (cand.agric)	5	3,904	39.08
Veterinary science (cand.med.vet)	6	617	6.18
Pharmaceutical science (cand.pharm)	5	608	6.09
Educational science (cand.paed)	6.5	355	3.55

missing information on date of completion of high school. These are mainly elderly people and immigrants. Some programs do not have a clear reform status, either because it is not clear from the regulations whether students enrolled in the program qualified for restitution or because the reform status changed during the reform period. This amounts to 45 percent of the students in our sample. Almost 60 percent of this number comprises students studying civil engineering. Education programs in civil engineering became eligible for restitution at a later time than other programs did. The second and third largest groups, totaling about 34 percent of the excluded students, are two groups of students enrolled in unspecified higher education programs. We exclude students enrolled in these programs. The total number of students in our sample is 46,366. Appendix Table A.1 gives an descriptive overview of the main variables used in this analysis.

Table 1 shows how the students are distributed across the different education programs. The programs affected by the reform (the treatment group) mainly are law, science, humanities and social sciences. A majority of these programs were non-integrated study programs (i.e., separate undergraduate and graduate degrees), where the last or two last years were devoted to write a master thesis. The programs not affected by the reform (the control group) consist mainly of integrated five- or six year programs, medicine and agronomy being the two most important once. A majority of the students in the treatment group were enrolled in 12-semester programs (i.e., six years), while students in the control group are equally divided across 10 (five years)- and 12 - semester programs.

Although the reform gave an incentive to graduate on time, our main outcome variable will be the number of semesters delayed. The average delay for the students in our sample is 5.0
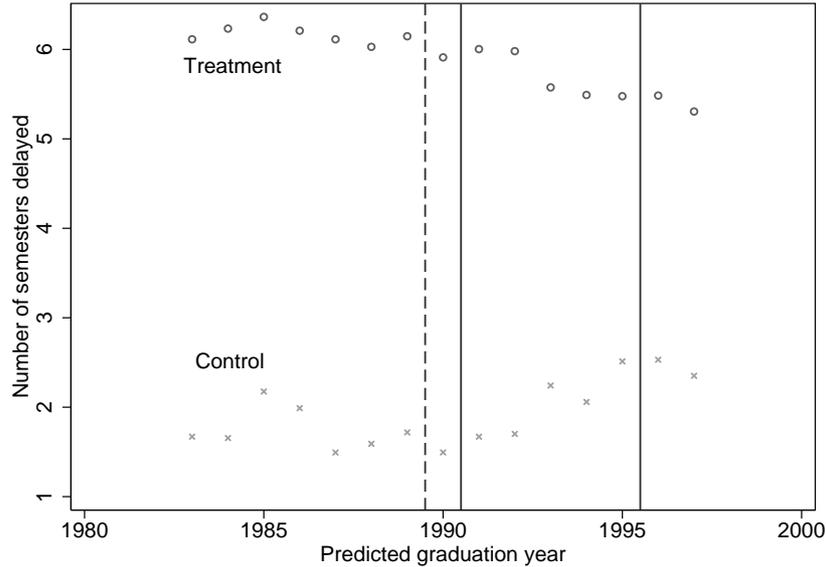
Figure 1: Number of semesters delayed, 1983 to 1997.

semesters (standard deviation equals 4.7). This variable provide a more precise indication of the extent to which the reform triggered students to increase their study effort, and hence reduce excess time. It captures the overall change in study behavior, not only whether the student succeed in reducing reducing delay from positive to zero. Also, semesters delayed is the most policy-relevant variable as it gives a better indication of private and social cost associated with late graduation.[6] For completeness we also investigate whether more students did complete on time. This will give us an indication of the share of students who increased their study effort enough to actually get the "bonus". Finally, we check if the reform had an effect on completing at all. About 75 percent of the students in our sample did not complete on time, whereas about 19 percent did not complete at all.

In Figure 1, we show how numbers of semesters delayed changed for students with graduation ranging from 1983 to 1997 separately for the treatment and the control group. During the whole period, the number of semesters delayed is lower in the control group than in the treatment group. However, the fraction of students delaying their studies declined in the treatment group during the reform period (autumn 1990 to autumn 1995). In contrast, the control group is associated with a upward trend in the same period. Before the reform both groups follow a similar pattern.

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[6]While we are able to calculate the cost to treat (see section 6), we can not say much about the cost-benefit of the reform, because we do not know the costs and benefits of delayed graduation. For instance, the consumption value of attending university is hard to evaluate, and more time in higher education may have a productivity effect, irrespectively of degree earned. The cost will include direct teaching costs (which may be small for the relevant programs) as well the opportunity cost of the students' time and loss of experience accumulation. However, this may be (partly) offset if the students work while studying.

Table 2: Average number of years spent studying under the reform, by year of graduation and treatment status

	Control Group	Treatment Group	Total
1983-1990	0.000	0.000	0.000
1991	1.000	1.000	1.000
1992	2.000	2.000	2.000
1993	3.000	3.000	3.000
1994	4.000	4.000	4.000
1995	5.000	5.000	5.000
1996	4.542	4.972	4.902
1997	3.587	4.026	3.941
Total	1.713	2.218	2.109

The degree of exposure to the reform varies considerably between students in our sample, thus we will allow the effect of the reform to depend on years studied during the reform, as detailed in the next section. In the remainder of the paper we will denote year studied during the reform as *years treated*, for students both in the treatment and control groups. We measure years treated as the number of years a student was in higher education under the reform, without having passed her *predicted* graduation time. When a student passes the *predicted* time of graduation, she or he is delayed, and thus no longer eligible for the restitution, i.e., the treatment ends. With the treatment period ranging from 1991 to 1995, the maximum value of years treated is five years. In our sample, 19 percent of the students were enrolled during the whole reform-period, five years. 18 percent were enrolled for four years, whereas 42 percent had their graduation before the reform (see Appendix Table A.2).

In Table 2 we report the average number of years treated during the reform by year of graduation, separately for students in the treatment and control groups. Students with graduation in 1990 or earlier did not have any chance to respond to the reform. Students who graduated in 1991 were enrolled one year under the reform, having some opportunity to react, and so on. Because of the short life of the reform relative to the length of the study programs, the last students to graduate under the reform would largely have started before its introduction. Furthermore, the first students to graduate after the termination of the reform would mostly have enrolled before or concurrently with the introduction of the reform. Thus, a majority of these student would be treated during the entire reform, i.e. five years. For each year after the reform ended, graduates' first enrollment will be later, and reform exposure less. Our sample ends with the 1997-graduates, who were enrolled about four years under the reform. Note that the number of years enrolled under the reform is somewhat less in the control group, were the programs on average are of somewhat shorter duration.

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5   Empirical approach

To estimate the effect of the turbo reform we will rely on the following difference-in-difference framework. D_i is a dummy variable that equals one if student i is in the treated programs and zero if he/she belongs to the control group:

$$y_{it} = \alpha + \phi D_i + d_t + \eta T_{it}^T + \gamma_1(D_i \cdot T_{it}^T) + \gamma_2(D_i \cdot d_t^T) + \gamma_3(D_i \cdot d_t^{PT}) + \beta X_i + \varepsilon_{it} \qquad (1)$$

The outcome variable y_{it} measures how many semesters student i is delayed. T_{it}^T is years treated, as discussed in the previous section. d_t is a set of dummy variables for graduation year, d_t^T is a dummy variable equal to one if graduation year is during the reform period, d_t^{PT} is a dummy variable equal to one if graduation year is after the reform period (1996-1997). X_i is a vector of covariates such as dummy variables for age, gender, length of the study program and parental education (described in Appendix A.1), and ϵ_{it} is a random error term. As already mentioned, we will also look at two additional outcome variables, namely completing on time and completing at all.

Our critical assumption is that, absent any reform, treatment and control follow similar trends. This allows us to use the control group to control for time-variation unrelated to the reform and to estimate the reform effect as a difference-in-difference. As discussed in relation to Figure 1, we believe this assumption is reasonable.

Our parameters of interest are the difference-in-differences parameters γ_1, γ_2 and γ_3. γ_1 measures the reform effect for each year treated, and is expected to be negative (less delay with more treatment).γ_2 and γ_3 capture that the effect of the reform may not be proportional with time treated.[7] That is, to allow for more general effects, we add interaction terms between the dummy variable for being in the treatment group and the dummy variables for whether the student's graduation year was during or right after the reform period. Hence, the respective reform effects for students with graduation during and after the reform period become $\gamma_1 T_{it}^T + \gamma_2$ and $\gamma_1 T_{it}^T + \gamma_3$. Conditional on the effect of years treated (γ_1), γ_2 and γ_3 capture, among other things, the potential effect timing of the incentive may have on semesters delayed. If being treated late in the study progression is more important than being treated early (e.g. because delays tend to arise in the last part of a degree), the effect of the incentive should be larger for students graduating during the reform-period than for students graduating after the reform-period, $\gamma_2 < \gamma_3$ (i.e., more of a reduction in delay for those graduating during the reform period). Likewise, if the opposite is the case (e.g. because of habit formation in early studying years), $\gamma_3 < \gamma_2$.

However, given that the effect of the reform need not be proportional to years treated, we do not have any clear *a priori* expectation of neither signs nor the relative magnitudes of γ_2

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[7]The reform effect need not even be linear, however, as the range in years untreated is not very great, the linear approximation used is likely to be reasonably good.

and γ_3.[8] Furthermore, $\gamma_3 < \gamma_2$ could also reflect a change in study norms where there is a persistent reform effect, e.g. because excess time is less accepted among peers or potential employers. While the estimated values may give indications of which of the above stories are the more relevant ones, we can not with any degree of certainty distinguish between potential mechanisms. In particular, as we do not have any post-reform students that are not treated to a large degree in our sample, it is hard to judge if there is any persistent effect for non-treated students.

As previous research has found different effects for high and low ability students (Leuven et al., 2010), we also estimate models where we interact years treated with a dummy variable for whether at least one of the parents have a higher degree (corresponding to 15+ years of schooling, the dummy variable is denoted H_i).

$$
\begin{aligned}
y_{it} =\ & \alpha + \phi D_i + d_t + \eta T_{it}^T + \gamma_1 (D_i \cdot T_{it}^T) + \gamma_2 (D_i \cdot d_t^T) + \gamma_3 (D_i \cdot d_t^{PT}) \\
& + \gamma_1^H (D_i \cdot T_{it}^T \cdot H_i) + \theta_1 (D_i \cdot H_i) + \theta_2 (T_{it}^T \cdot H_i) + \beta X_i + \varepsilon_{it}
\end{aligned}
\tag{2}
$$

In Equation (2) the estimated reform effect for a student stipulated to graduate in the reform period is $(\gamma_1 + \gamma_1^H \cdot H_i) \cdot T_{it}^T + \gamma_2$ (similarly with γ_3 replacing γ_2 for a student with stipulated graduation after the reform), i.e., it is allowed to vary with parental education. Note that we let the dependency of the reform effect on graduation during/after the reform period be the same, irrespective of parental education.

With difference-in-differences studies there is always a concern that the estimates are altered by differential time trends or other shocks. To check the robustness of the reform effects, we investigate the trends in study duration for all three outcome variables both before and after the reform period for the treatment and control groups. We do this by estimating a more general difference-in-differences equation than equation (1) containing year-specific effects (so-called placebo "reform" effects).

$$
y_{it} = \alpha + \phi D_i + d_t + \eta T_{it}^T + \tilde{\gamma}_t (D_i \cdot d_t) + \beta X_i + \varepsilon_{it}
\tag{3}
$$

In this specification, we replace the difference-in-differences parameter γ_1 with a vector of year-specific parameters $\tilde{\gamma}_t$. Thus, the average reform effect for a student with stipulated graduation in year t is now given as $\tilde{\gamma}_t$, where it in equation (1) would be $\gamma_1 \cdot \overline{T}_{it}^T + \gamma_2 \cdot d_t^T + \gamma_3 \cdot d_t^{PT}$. Note however that testing for (placebo) "reform effects" is only relevant before the introduction of the reform. As already discussed, given the short time-span of the reform, students graduating after the termination of the reform may have reduced their time to degree. Thus, any evidence of a persistent effect after the reversal of the reform does not violate our common trends assumption. Another reason for introducing year-specific difference-in-differences variables is that this approach allows us to study the dynamics of the introduction of the reform in a more flexible

---

[8]$\gamma_1 T_{it}^T + \gamma_2$ and $\gamma_1 T_{it}^T + \gamma_3$ will be linear approximations of the reform effect. If the effect of the reform is not proportional to years treated, γ_2 and γ_3 can have any sign, depending on the deviation from proportionality. If the effect of treatment increases more (less) than proportionally with years treated we expect γ_2 and γ_3 to be negative (positive).

way.

The control group consists mainly of education programs that select students on grades from upper secondary school. Once inside, the teaching tends to be classroom-like and strictly organized, where the students' progress is taken well care of. The treatment group, on the other hand, mostly consists of education programs with low requirements for admission (humanities, social sciences, science). The programs typically have a loosely organized structure, with little tutoring, and with students being responsible for their own progression and graduation time. This does not need to be a problem for the identification of any reform effects as we in our difference-in-difference approach control for any time-constant difference in the on-time graduation rates, as well as time-varying effects that are shared between the groups. Still, in order to study a treatment group more comparable to the control group, we will also look separately at students in the cand.jur (law) program. While there were not strict admission requirement in law, there was a cut-off after two years, such that the students completing a law degree were a selected group.[9] Furthermore, like the programs in the control group, the law program had a very well-defined structure and expected progression.

While we control for common year effects, there is still a possibility that there are some changes over time that differ between treatment and control, and may impact on the outcome. We address a number of such potential confounders both in the main specification and in robustness checks. First, the composition of the groups may change over time. By including covariates describing the students' family background in X_i we control for this to the extent that such changes correlate with our observed characteristics. Second, the composition of the treatment and control groups in terms of detailed programs may change over time. For example, a response to the reform may be that more students may choose programs that are easy to complete on time. We address this by adding program-specific effects, and also program-specific trends. Third, the number of students enrolled increases over time. Larger numbers of students may mean that average student is weaker and that teaching quality decreases. Both should contribute to increase delays. We address this by controlling for the log of each program's cohort size, and also allow the effect of this variable to vary between the treatment and control groups. Finally, conditions in the labor market might have influenced the treatment and control groups differently (e.g. Häkkinen and Uusitalo, 2003).[10] As a final robustness check, we will control for the (national) unemployment rate. Because we only have yearly unemployment data we estimate and control for the difference between the treatment and control groups in effect of unemployment.[11]

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[9]Only students with grade B or better on their two-year exam could continue to study law.

[10]The period under investigation started with a quite strong recession but the economy started to boom around 1993.

[11]The effect of unemployment on the control group will of course be captured by the year effects.

6 Results

Column (1) - (4) in Table 3 report different variations of Equation (1). In the first column we disregard years treated and only estimate the effect of graduating during or right after the reform-period, i.e. γ_2 and γ_3. On average, delays were reduced by 0.81 semesters during the reform period, and by 1.52 semesters in the first two post-reform years. Both effects are statistically significant at the one percent level. Modeling the reform effect as proportional to years treated, γ_1 (column (2)), each year of treatment reduces delay by 0.28 semesters. This effect is also statistically significant at the one percent level, and amounts to 1.4 semesters for a student who is treated during the entire duration of the reform (0.28*5 years). Note that the baseline difference between the treatment and control groups is as high as 3.7 semesters, thus, even though the reform effect is substantial, it far from eliminates the difference between the groups.

The more flexible specification in column (3) (including γ_1, γ_2 and γ_3) reduces the effect of each year treated marginally to 0.23 . On the other hand, both γ_2 and γ_3 are reduced substantially, compared to column (1). This suggests that the reform effect mostly depends on the amount of time treated, rather than the timing of the incentive, i.e. whether a student graduated during the reform or after the reform ended. However, γ_2 and γ_3 contribute to a larger reform effect, as they are still negative, if not significant. Although γ_2 and γ_3 are not significantly different, the larger absolute value of γ_3 may suggest that early treatment is important.

Controlling for gender and background variables in column (4) does not change the difference-in-difference estimates much, indicating that observable characteristics do not influence the treatment group and the control group differently. This result also indicates that the relative composition of the treatment and control groups in terms of individual characteristics do not change over time. As for the estimated coefficients on the background variables, male students and students of higher-educated parents are on average less delayed than female students and students whose parents have shorter education.

Column (5) in the same table present results from estimating Equation (2) where we interact the dummy variable for whether at least one of the parents have higher education with number of years treated. There are no indications that the effect differs across students from different background, as the point estimate is close to zero and also not statistically significant.

To investigate the relationship between financial incentives and study duration further, we also look at other outcome variables than the number of semesters delayed. The results are reported in Table 4. Apart from the dependent variable, the models presented are equivalent to the model in column (4) in Table 3 .[12]

As the bonus attached to the financial incentive was given to students who completed their studies on time we start out by regressing the reform effects on a dummy variable indicating whether the student has graduated on time or not, see column (1). The share of students

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[12]These models are estimated with a linear probability model, using a logit model does not change our conclusions.

Table 3: The effect of financial incentives on semesters delayed estimated by OLS

	(1)	(2)	(3)	(4)	(5)
Treatment	3.698 (0.073)***	3.667 (0.070)***	3.679 (0.074)***	3.566 (0.074)***	3.573 (0.074)***
Treatment*reform years (γ_2)	-0.812 (0.106)***		-0.097 (0.202)	-0.066 (0.201)	-0.062 (0.201)
Treatment*post reform years (γ_3)	-1.524 (0.136)***		-0.388 (0.279)	-0.330 (0.278)	-0.326 (0.278)
Years treated		-0.337 (0.156)**	-0.254 (0.162)	-0.285 (0.161)*	-0.245 (0.164)
Treatment*Years treated (γ_1)		-0.277 (0.024)***	-0.229 (0.055)***	-0.223 (0.055)***	-0.221 (0.061)***
Male				-0.539 (0.042)***	-0.538 (0.042)***
Mother's ed (ref = $\leq$compulsory)					
- *Intermediate*				-0.320 (0.064)***	-0.335 (0.064)***
- *Tertiary*				-0.365 (0.072)***	-0.399 (0.083)***
Father's ed (ref = $\leq$compulsory)					
- *Intermediate*				-0.251 (0.074)***	-0.257 (0.074)***
- *Tertiary*				-0.459 (0.077)***	-0.526 (0.106)***
Parents tertiary ed.					0.230 (0.107)**
Years treated*parents tertiary ed					-0.061 (0.038)
Years treated*parents tertiary*Treatment					-0.010 (0.038)
R-squared	0.125	0.125	0.125	0.134	0.134
Nr. of observations	46366	46366	46366	46366	46366

Note: Included in all specifications are year dummies for graduation year and for length of education program and a constant term. Specification (4) and (5) also include dummies for missing information on parental education and dummies for age when graduating from high school. Standard errors are heteroskedasticity robust. */**/*** statistically significance at the 10/5/1 percent level.

Table 4: The effect of financial incentives on completing on time and completing at all, estimated by OLS

Dependent variable	(1) On-time graduation		(2) Not completed		(3) On-time graduation	
Treatment	-0.235	(0.008)***	0.135	(0.006)***	-0.225	(0.009)***
Treatment*reform years	-0.067	(0.021)***	-0.009	(0.016)	-0.084	(0.022)***
Treatment*post reform years	-0.078	(0.029)***	-0.026	(0.022)	-0.102	(0.031)***
Years treated	0.055	(0.016)***	0.009	(0.013)	0.060	(0.017)***
Treatment*Years treated	0.038	(0.006)***	-0.007	(0.004)	0.040	(0.006)***
Included in sample	All students		All students		Students graduated	
R-squared	0.201		0.023		0.230	
Nr. of observations	46366		46366		37627	

Note: In column (1) we impute those not graduated as delayed. In column (3) we exclude student that did not complete.. Included in all specifications are year dummies for graduation year, dummies for length of education program, students' background characteristics and gender and a constant term. Standard errors are heteroskedasticity robust. */**/*** statistical significance at the 10/5/1 percent level.

graduating on time increases with about 3.8 for each year treated, from a baseline probability of about 20 percent. However, this is partly offset by a negative constant term, such that for students treated for four or five years, the probability of on-time graduation increases by 8-11 percentage points. Even though the reform was not meant to have an impact graduating *per se*, we can not rule out that this was the case. In column (2) the dependent variable is a dummy which equals one if the student did not complete. All the reform effects are small and statistically insignificant, suggesting that the reform did not influence graduation. Hence, when excluding student that did not complete (column (3)), it is therefore not surprising that the effect on completing on time is basically unaltered.

From the estimates in Tables 3 and 4 it is possible to do some back-of-the-envelope calculations of the cost to treat of the reform. With three years of treatment, which about corresponds to the average for those treated, we get an estimated effect of a reduction of delays by about 0.8 semesters for those graduating during the reform ($\hat{\gamma}_1 \cdot 3 + \hat{\gamma}_2 = -0.79$, using column (3) from Table 3). Furthermore, this means an about 4 percentage points increase in the share graduating on time ($\hat{\gamma}_1 \cdot 3 + \hat{\gamma}_2 = -0.036$, using column (1) from Table 4). If we extrapolate the effect of the the reform to six years of treatment - this is an out-of-sample prediction, and should be interpreted with caution - the duration of most treated program, the corresponding effects are about 1.5 semesters less delay and 16 percentage points more on time. Thus, with three years of treatment, for each six restitutions given, in about one case the student graduates on time because of the reform. With six years of treatment, the corresponding figure is one for every 1.8 restitutions given.[13]

The year-specific difference-in-difference estimates (based on Equation 3) for all three out-

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[13]While we are able to calculate the cost to treat, we can not say anything about the cost benefit of the reform, because we do not know the costs and benefits of delayed graduation.

Table 5: Placebo Testing

Dependent variable	(1) Nr of sem delayed		(2) On-time graduation		(3) Not completed	
Treatment	3.680	(0.183)***	-0.264	(0.020)***	0.142	(0.014)***
Treatment*Year (ref=1989)						
-1983	0.081	(0.258)	0.066	(0.028)**	0.026	(0.021)
-1984	0.119	(0.260)	0.015	(0.028)	0.024	(0.021)
-1985	-0.344	(0.272)	0.032	(0.028)	-0.027	(0.022)
-1986	-0.275	(0.270)	0.009	(0.028)	-0.038	(0.022)*
-1987	0.010	(0.259)	0.025	(0.028)	-0.005	(0.020)
-1988	-0.114	(0.266)	0.020	(0.028)	-0.007	(0.021)
-1990	-0.219	(0.262)	0.032	(0.028)	-0.030	(0.020)
-1991	-0.276	(0.265)	-0.008	(0.028)	-0.020	(0.021)
-1992	-0.074	(0.256)	-0.011	(0.027)	0.003	(0.020)
-1993	-1.198	(0.262)***	0.105	(0.028)***	-0.061	(0.021)***
-1994	-1.073	(0.246)***	0.118	(0.026)***	-0.044	(0.019)**
-1995	-1.430	(0.252)***	0.145	(0.027)***	-0.057	(0.020)***
-1996	-1.455	(0.250)***	0.120	(0.027)***	-0.051	(0.020)***
-1997	-1.626	(0.243)***	0.158	(0.026)***	-0.067	(0.019)***
R-squared	0.134		0.201		0.023	
Nr. of observations	46366		46366		46366	

Note: Included in all specifications are year dummies for graduation year, dummies for length of education program, students' background characteristics and gender and a constant term. Standard errors are heteroskedasticity robust. */**/*** statistically significance at the 10/5/1 percent level.

come variables (number of semesters delayed, graduating on time, and not completing) are reported in Table 5. There are no indications of (placebo) "reform effects" on the number of semesters delayed before the implementation of the reform. The sign of the estimated coefficients vary, and none are statistically significant. This statistically confirms our impression that the trends in Figure 1 are parallel, and indicates that our identifying assumption is indeed justified. We do find two significant pre-reform "effects" for the other two outcomes (one of these, only at the 10-percent level). However, testing a large number of coefficients, it is not surprising that some are significant. Furthermore, even the significant pre-reform estimates are small, and the pre-reform estimates show no obvious pattern or other indication of pre-reform differential trends.

There are also no significant reform effects in the three first years of the reform, but consistent positive effects after that. While the reform effect does not increase linearly, the year-specific estimates are not very precise, thus we can not rule this out. Taken at face value, the estimates suggest a slow but lasting impact of the reform. This result may imply that it is particularly important that treatment starts early and hence complements previous findings that $\gamma_3 < \gamma_2 < 0$. The students affected seems to be those with about two or more years left of their studies. Recall that for the treatment programs a large part of these last two years, was to write a thesis.

Our year-specific results seem to indicate that it is important that students are exposed to the incentive at the latest when starting this part of their studies.

Column (2) presents results for on time graduation. The pattern in this column matches that in column (1), with indications of a slow but lasting impact. Finally, column (3) shows the effects on not completion. Unlike in Table 4 we here find an effect on the share completing higher education for the later years. However, this effect is to small to explain the increase in on time graduation in column (2).

6.1 Sensitivity checks

Restricting the treatment group to the cand.jur (law) program (the program in the treatment group with most similarities to the programs in the control group) the estimated reform effects are similar, and generally not significantly different from those in Table 3. We do see somewhat smaller overall effects, with delay reduced by on average 0.32 semesters during the reform period and 0.88 semesters after the reform period. The flexible reform effect is a reduction in delay by 0.22 semesters for each year treated (not significantly different from the effect in Table 3), but this is partly offset by positive estimates for γ_2 and γ_3 (the latter being borderline significant). The fact that students graduating during the reform-period are more delayed (positive γ_2) may be due to a delayed effect, as in Table 5, being the linear approximation to an slow initial response. The positive γ_3, which is the larger but also more imprecise of the two coefficients, indicates that there is less of a lasting effect of the reform for the law students. Law students initially treated to a larger degree than other treated students increase their delay at the termination of the reform. These results are reported in Appendix Table A.3.

Table 6 shows other robustness checks. Each of these control for variables that may potentially have different impacts on the treatment and control groups, as discussed in the Empirical approach section. Column (1) and (2) show that the results are not much affected by controlling for program-specific effects or program-specific linear trends. Because the treatment group is a sum of specific programs, we can not simultaneously estimate program-specific effects and the difference between the treatment and control groups. Thus, this coefficient is not reported in 6. Column (3) further adds the (natural) logarithm of the program-specific cohort size. Larger numbers of students may mean that average student is weaker and that teaching quality decreases. Both should contribute to increase delays. In column (3) we find weak evidence of reduced delays with increasing cohort size, which however does not influence our estimated reform effect. In column (4) we allow the effects of cohort size to differ between the treatment and control groups. The estimated coefficients are practically identical, thus, this is of little consequence for our results. Finally, in column (5) we control for treatment-specific effects of the unemployment-rate at the time of graduation. While we do find that increasing unemployment reduces the delay in the control group relative to the treatment group, our estimated reform effect is similar to our previous estimates. We have also investigated how the truncation of the

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Table 6: Robustness checks. The effect of financial incentives on semesters delayed, estimated by OLS.

	(1)	(2)	(3)	(4)	(5)
Treatment*reform years(γ_2)	-0.025 (0.196)	0.206 (0.224)	0.157 (0.225)	0.157 (0.228)	0.026 (0.248)
Treatment*post reform years(γ_3)	-0.214 (0.269)	0.064 (0.319)	-0.048 (0.324)	-0.048 (0.324)	0.062 (0.319)
Years treated	-0.367 (0.153)**	-0.138 (0.188)	-0.109 (0.188)	-0.109 (0.193)	-0.149 (0.188)
Treatment*Years treated (γ_1)	-0.241 (0.054)***	-0.241 (0.059)***	-0.222 (0.060)***	-0.222 (0.067)***	-0.212 (0.061)***
Log(program-specific cohort size)			-0.372 (0.216)*		
Control*log(program-spec cohort)				-0.368 (0.452)	
Treatment*log(program-spec cohort)				-0.373 (0.245)	
Control*National unemployment rate					-0.124 (0.074)*
Program-fixed effects	Yes	Yes	Yes	Yes	Yes
Program-specific linear trends	No	Yes	Yes	Yes	Yes
R-squared	0.191	0.195	0.195	0.195	0.195
Nr. of observations	46366	46366	46366	46366	46366

Note: See Table 5

dependent variable impacts on our results, finding it to have little significance.[14]

7 Conclusion

Ensuring access and equal opportunity in higher education is a central aim of policy makers. A pivotal policy instrument in this regard are state-provided grants and favorable students' loans. However, subsidizing time spent studying in order to increase students' level of attainment may have undesired consequences in the form of reduced study efficiency because the support reduces the marginal cost of studying.

In this paper we study the effects of financial incentives on study duration. Using rich register data to investigate the effect of a reform that rewarded students who completed their higher education degree nominally on time in a difference-in-difference framework. We find that the share of on-time graduation increases by 3.8 percentage points per year treated, from a pre-reform level of about 20 percent. Thus, a large share of the restitution given will be for students who would otherwise not have graduated on time. Moreover, in order to capture to overall effect of the reform, we find that the average delay in the treatment group decreased by on average 0.8 semester during the reform period, and by 1.5 semesters in the following two years. The large effect in the first post-reform years points to a strong effect of the duration of the treatment, with delays reduced by 0.23 semesters per year treated. Furthermore, there is some indication that it is important that treatment starts before the final part of the educational programs, potentially indicating that early treatment is important to establish efficient study habits. A series of robustness checks indicate that our estimated effects do not reflect differential trends or omitted variables.

Our results suggest that students respond quite strongly to the financial incentive. Reasons for this may be that students live on a small budget and are quite constrained financially. Moreover, a restitution of 18 000 NOK may be perceived as a powerful enough incentive to trigger a change in students study behavior as it roughly corresponds to the monetary gains associated with a one semester part-time job or alternatively a summer-job. In addition, the response to the incentive may also indicate that students can quite easily adjust their study duration and that they understand well the mapping between study effort and study duration.

However, it is difficult to draw clear policy implications from our findings as we do not know the underlying mechanisms that are driving these results. Potential mechanisms may include increased study intensity, for example, by reducing part-time work, or graduation with less human capital, i.e., lower grades and/or fewer credits. Further research is necessary to distinguish between such potential mechanisms and to investigate further consequences of the reform.

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[14]We have estimated our baseline model with the untruncated data on semesters delayed, as well as truncating on the 10th/90th percentile and on the 1th/99th percentile. Results are omitted for brevity, but available upon request.

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A   Appendix

Table A.1:   Descriptive statistics for the estimation sample, in fractions

OUTCOME VARIABLES	
Nr of semesters delayed	4.981
Students graduating on time	0.248
Students not completing	0.188
EXPLANATORY VARIABLES	
Years treated	2.109
Average length of study programs	11.5
Male	0.498
Average age end of high school	19.12
Mother's education	
- Compulsory (0-10)	0.156
- Intermediate (11-14 years)	0.495
- Tertiary (15 - 20+)	0.323
- Missing	0.026
Father's education	
- Compulsory (0-10)	0.108
- Intermediate (11-14 years)	0.396
- Tertiary (15 - 20+)	0.457
- Missing	0.039

Table A.2: Average number of years enrolled under the reform, by treatment status

	Control Group		Treatment Group		Total	
	Nr	Percent	Nr	Percent	Nr	Percent
0	4,960	49.65	14,635	40.23	19,595	42.26
1	613	6.14	2,151	5.91	2,764	5.96
2	643	6.44	2,422	6.66	3,065	6.61
3	1,081	10.82	2,715	7.46	3,796	8.19
4	1,492	14.94	6,728	18.50	8,220	17.73
5	1,200	12.01	7,726	21.24	8,926	19.25
Total	9,989	100	36,377	100	46,366	100

Table A.3: The effect of financial incentives on graduating on time for cand.jur (law) students, estimated by OLS

	A1	A2	A3	A4	A5
Treatment	2.206 (0.098)***	2.185 (0.093)***	2.135 (0.099)***	2.101 (0.098)***	2.110 (0.098)***
Treatment*reform years (γ_2)	-0.315 (0.138)**		0.398 (0.274)	0.456 (0.272)*	0.461 (0.272)*
Treatment*post reform years (γ_3)	-0.881 (0.169)***		0.638 (0.367)*	0.713 (0.366)*	0.712 (0.366)*
Years treated		-1.108 (0.204)***	-1.177 (0.213)***	-1.185 (0.213)***	-1.121 (0.216)***
Treatment *Years treated (γ_4)		-0.119 (0.031)***	-0.224 (0.072)***	-0.228 (0.071)***	-0.205 (0.077)***
Parents tertiary					0.242 (0.153)
Year treated*parents tertiary					-0.071 (0.040)*
Year treated*parents tertiary*treat					-0.041 (0.045)
Controlling for					
Background char and gender	No	No	No	Yes	Yes
R-squared	0.089	0.091	0.092	0.102	0.102
Nr of observations	18135	18135	18135	18135	18135

Note: See Table 5
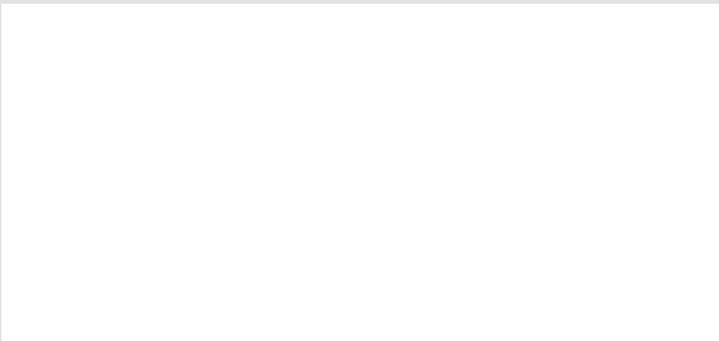
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