ISSN 1503-299X

WORKING PAPER SERIES

No. 6/2011

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Financial incentives and study duration in higher education*

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Abstract

The current 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 1991 and 1995 on stipulated time were entitled to a restitution (of approximately 3,000 USD) from the Norwegian State Educational Loan Fund. Using a difference-in-difference approach, we find that the fraction of students graduating on time during the reform period increased by 10 percent, relative to a base probability of about 25 percent. The estimated effect for fully treated students (students who were aware of the reform from the start of their studies) is much higher, at 50 percent.

*We are grateful for generous comments from Hans Bonesrønning and Bjarne Strøm.

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1 Introduction

Education at all levels is often believed to be associated with positive externalities. Because of this, and in order to allow for equality of opportunity, education is subsidized in many countries. This is the case wherever students do not pay the full cost of their instructions through tuition. Several countries have even stronger subsidies, in that students' living expenses are also partly covered, either through scholarships or government loan agencies.

From human capital theory, we would expect subsidies to increase return to education and also help offsetting credit-constrained students. However, the presence of subsidies to education may not only increase the choice of attainment level in the higher education system, but may also influence the level of effort provided by students: As students are generally subsidized for each unit of time studying, and not for the degree attained, there may be incentives to spend too much time in the educational system (see among others Lindbeck, Nyberg and Weibull, (1999)). This may be particularly important if the consumption value, i.e. private, non-pecuniary return to education, is a dominant factor behind the students' study duration choice (Alstadsæter and Sivertsen, 2010; Zafar, 2009). In this case, if students are credit-constrained, increases in income support may finance increased consumption of education services, with little externalities.

It is indeed observed that many students in universities and college programs around the world do not complete their university/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 beyond estimated completion time. Similar patterns are also documented for many European countries (Brunello and Winter-Ebmer, 2003). This together with the belief that students do not exert sufficient study effort, has increased the interest in whether students react to financial incentives. The evidence in this area is however mixed and still limited.

By using rich data from the administrative registers of Statistics Norway, the aim of the current paper is to investigate the effects on study duration of a reform that rewarded students who completed their higher education degree on nominal time. The reform made students in Norway who completed certain graduate study programs between the autumn semester 1991 and the autumn semester 1995 entitled to a restitution of approximately 18,000 NOK (about 3000 USD and 34 percent of the yearly loan sum) from the Norwegian State Educational Loan

Fund if they finished the program on stipulated time. This reform focused on the intensive margin, explicitly aiming to induce students to improve the efficiency in higher education. Earlier reforms have 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 creates sharp discontinuities in the financial incentives that the 1991 to 1995 graduation cohorts face compared to the previous and subsequent cohorts. These discontinuities can be exploited to estimate the impact of the financial reward on study duration. Like all research designs that depend on a reform, there is the potential threat of confounding time effects. However, the fact that some education programs were not eligible for the restitution provides an additional comparison group that will allow a difference-in-differences approach that can correct for such confounding time effects. Our results suggest that the reform had quite strong effect on students' time to degree.

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.

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 and the student support system in Norway. Section 4 outlines the empirical strategy whereas section 5 presents the findings. Section 6 concludes.

2 Related Literature

The empirical literature on study duration in higher education is in brief twofold: (i) It focuses on the relationship between student aid and the demand side of higher education, i.e. the extensive margin and how financial subsidies can increase enrollment and investment in higher education, and (ii) how to improve the efficiency and 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 a post-secondary education. Many studies have been conducted in order to measure the effect of these systems. For instance, Dynarski (2003) finds that the college attendance dropped by more than a third, and schooling by two-thirds of a year after a shift in the financial aid policy in the United States when the Congress in 1992 eliminated the Social Security student benefit program. Dynarski (2004) also studies the effect of scholarships in the United States. Scholarships enable students to participate in the educational attainment process and whereas traditional scholarships often are eligible for A-levels student, new merit aid programs in the United States require relatively modest academic credentials. Dynarski (2004) provides evidence that most of these merit aid programs have closed racial and ethnic gaps in attendance. Skyt-Nielsen, Sørensen and Taber (2010) investigate the change in demand for college due to changes in student aid arising from a Danish reform. Their findings indicate that enrollment increases with higher subsidies, but that enrollment is less responsive than in other studies and countries. They argue that one reason for this may be that large subsidies are already in place in Denmark. The presence of borrowing constraints seems also only to deter college enrollment to a minor extent.

More recently, the potential of financial incentives to increase the study duration and performance among students has attracted attention. Financial incentives may be implemented either through direct money incentives, variation in tuition fees or restitution of student loans depending on academic results, or in our case the time to degree. The literature in this domain is however relatively small and scattered, and a lot of it relates to compulsory education (e.g. Kremer, Miguel and Thornton, 2005; Angrist and Lavy, 2009).

Leuven, Oosterbeek and Van der Klaauw (2010) implement a randomized experiment among first year economic students in Amsterdam where students who passed all first year requirements on time could earn a reward of 750 Euros. They find that this increased performance for higher ability students, but they also find negative impacts 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 is that they 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 where the old loan-based student aid system was replaced with a system of grants. The reform had only modest effect, and this effect was limited to fields with relative long durations. 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 however ambiguous. The fact that 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, distinguish our study from theirs.

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. Brunello and Winter-Ebmer (2002) find, by building on data from European countries, that an unappealing consequence of wage compression is that it might reduce the incentives to complete college in 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 reduction in collegiate resources outside the top-tier of institutions are the main explanations for the observed increase in time to degree.

3 Institutional settings and data

3.1 Higher education in Norway and the Norwegian State Educational Loan Fund

The Norwegian higher education sector is almost completely dominated by public institutions, with 85 percent of the students. Tuition is virtually nonexistent, making the direct costs of higher education very low.[1] There are three different types of institutions: universities, specialized university colleges and regional university colleges. All types offer courses both at the undergraduate and graduate level. During the 1990s undergraduate courses lasted up to four years, while most graduate courses had a total duration of five to six years. The universities offered both integrated five- or six-year courses, leading to a graduate degree (e.g. medicine, theological seminar and civil engineering) and shorter courses in different fields that could be combined to an undergraduate degree of three and a half or four years duration. Students with a relevant undergraduate degree can be admitted to graduate courses of one and a half or two years duration, such that the total stipulated duration of these degrees, including the undergraduate degrees, is five or six years. Students in specialized university colleges mostly study four- to six-year courses in specialized fields, such as business, architecture and veterinary science. Most students at regional colleges study two- or three-year professionally oriented courses (e.g. nursing, teaching, engineering and commerce).[2]

To further promote equality of opportunity, irrespective of family background, The Norwegian State Educational Loan Fund offers favorable loans which are meant to cover the students' costs of living such as housing and food during the study period.[3] There are several reasons why this loan is favorable. While studying, no interest is calculated and no repayment is required until the student has completed his/her education and (hopefully) 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

[1] The single important exception from this is a private business school with about 10 percent of the students, and which does charge significant tuition fees.

[2] From 2003, following the Bologna process, most educations have been streamlined into three-year Bachelor degrees and five-year Master degrees. Master degrees are organized either as a two-year course taken after completing a Bachelor degree, or a five-year integrated course. There is still a range of shorter courses, and some professional degrees of six years durations. The formal distinction between specialized and regional university colleges is recent, but reflects a difference which was present also during the 1990s. Also, since 2005 the number of universities have increased from four to eight through the conversion of one specialized university institutions and three regional university colleges.

[3] Note that also foreign students studying in Norway and Norwegian students abroad may benefit from the the Norwegian State Educational Loan Fund.

erased.

The Norwegian Parliament decides every year how much money to assign to the students the subsequent school year, generally adjusting this amount to keep up with students' costs of living. This sum, which amounted to 54,000 NOK (about 9,000 USD) for the academic year 1991/1992 (about 42,000 NOK of this sum was loan, and the remaining 12,000 NOK grant),[4] is the same for all students, and not affected by parental income. On the other hand, financial support has for a long time been need-based on the students' own income.

As the grant and interest benefit represent "free money" to the students, we expect a high take-up rate. Consistent with this, the fraction of students in higher education who take up loans is close to 100 percent.[5] The average loan amount per student was approximately 155,000 NOK for students completing higher education in 1994.

3.2 The "turbo" reform

Students in Norway who completed certain higher education programs between the autumn 1990 and the autumn 1995 were entitled to a restitution from the Norwegian State Educational Loan fund if they graduated on stipulated time. The restitution was approximately 18 000 NOK (about 3000 USD and 35 percent of the yearly financial support). The reform was made public July 1991. This implies that students graduating on stipulated time the autumn 1990 and spring 1991 benefited from the new incentive scheme, without being aware of it. Only students graduating the subsequent years (the academic year 1991/92 and onwards), got the chance to adapt their behavior according to the intention of the reform. Consequently, we will denote the period from the autumn 1991 to the autumn 1995 as the treatment period.

This reform creates sharp discontinuities in the financial incentives that the autumn 1991-autumn 1995 graduation cohorts face compared to previous and subsequent cohorts. We will exploit these discontinuities to estimate the impact of financial reward on study duration. The nice feature of the reform was that some education programs were not eligible for the restitution (see more below). This provides an additional comparison group that can be used

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

[5]Berg (1997) reports that 97 percent of students graduating with a higher degree get support from the Norwegian State Educational Loan Fund at some time.

in a difference-in-difference approach which correct for confounding time effects.

Although the reform was abolished the spring 1996,[6] the The Norwegian Ministry of Education suggested already in 1993/94 to terminate it. The reasons for this was the fear that the reform favored students not in need of a subsidy. It was also some doubt that the reform had any effects.[7]

3.3 Data

We use register data from Statistics Norway consisting of all students enrolled in Norwegian higher education institutions who graduated between 1988 and 1998. For each student we have information on which education program they were enrolled in, the length of the education program, start and stop dates and whether the students completed their studies on the stipulated time. The data is reported directly from the educational institutions to Statistics Norway, and are thus considered to be very accurate. We also have background characteristics such as the student's age, gender, immigrant status and parental education.

Students who were younger than 18 and older than 21 when graduating from high school (about 17 percent of the sample)[8] and students who enrolled in education programs after the reform was terminated are dropped from the sample. Some programs do not have a clear reform status, either because it is not clear from the regulations whether it does qualify for a restitution, or because the reform status changes during the reform period. We exclude students enrolled in these programs.[9] The total number of students in our sample is 26,871.

Table 1 shows how these students are distributed across the different education programs. The education programs affected by the reform (henceforth treatment group) mostly consists of students enrolled in humanities, social sciences, science and law, while medicine and agronomy dominate the groups which was not affected by the reform (henceforth control group). A majority of the students in the treatment groups is enrolled in 12 semester programs (i.e. 6

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

[7] From 1988 to 2003 students that opted for all longer study programs - 10 to 13 semesters, - were entitled to another restitution that was not linked to time to degree but only completion. The restitution was increasing in the length of the study program, ranging from around 19 000 for 10 semester programs to 46 000 for 13 semester programs. However, as this reform affected both students in the control and treatment groups equally much, we do not consider this to bias our difference-in-difference estimate.

[8] About 75 percent of these are dropped due to missing information on birth date.

[9] This amounts to 45 percent of the students in our sample. Almost 60 percent of this number is students studying civil engineering. Education programs in civil engineering became eligible for restitution at a later time than other courses. The second and third largest groups, totalling about 34 percent of the excluded students, are two groups of unspecified higher educations.

Table 1: Distribution of students across the different education programs

	Length of ed program (# semesters)	Nr of students	Percent
TREATMENT GROUP			
Humanities (cand.philo)	12	3,190	15.32
Social sciences (cand.poli)	12	3,549	17.04
Science (cand.scient)	11	5,811	27.90
Law (cand.jur)	12	5,570	26.74
Arts (music) (cand.musicae)	12	94	0.45
Theological seminar (cand.theol)	12	517	2.48
Economics (cand.oecon)	11	462	2,22
Psychology (cand.psychol)	13	932	4.47
Dentistry (cand.odont)	10	702	3,37
N		20,827	100
CONTROL GROUP			
Medicine (cand.med)	12	2,810	46.49
Agronomy (cand.agric)	10	2,337	38.67
Pharmaceutical science (cand.pharm)	10	350	5.79
Veterinary science (cand.med.vet)	12	440	7.27
Educational science (cand.paed)	13	107	1.77
N		6,044	100

years), whereas students in the control group are equally divided across 12 and 10 semester programs.

74 percent of the students in our sample spent more time in the higher education system than the stipulated length of the education program. In Figure 1 we show how graduation on time changed over time from 1988 to 1998 separately for the treatment and the control group. During the whole period, the fraction of students completing their studies on time is higher in the control group than in the treatment group. The fraction of students completing their studies on time rose in the treatment group during the reform period (autumn 1991 - autumn 1995). In contrast, the control group is associated with a downward trend in the same period. Both before and after the reform both groups follow a similar pattern.

Around 80 percent of the students in in our sample were already enrolled in higher education programs when the reform was implemented. This is reported in Table 2. In the same table we also see that approximately 11 percent of the students were enrolled 1 and 2 years prior to the reform, etc.

In Appendix Table 6 we present the main descriptive statistics for our final sample.

Figure 1: Fraction of pupils graduation on time from 1988 to 1998.

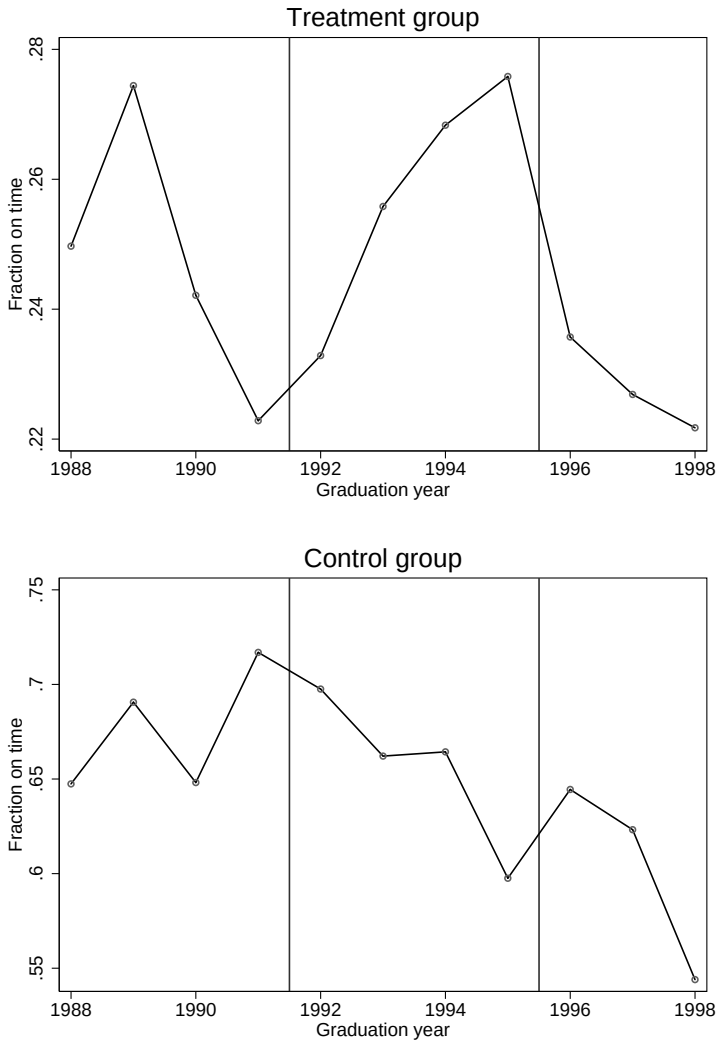

Table 2: Nr of years enrolled in higher education before the implementation of the reform

Years enrolled prior to reform	Nr of students	Percent
≤ 0	5,226	19.45
1	2,819	10.49
2	2,897	10.78
3	2,427	9.03
4	2,827	8.51
5	2,101	7.82
6	2,129	7.92
≥ 7	6,985	25.99

4 Empirical approach

We start out by assuming that the effect of the reform on graduation on time can be estimated by the following difference-in-difference equation:

$$y_{it} = \alpha + \phi D_{it1} + d_t + \eta T_{it}^{UT} + \gamma_1 (D_i \cdot d_t^T) + \gamma_2 (D_i \cdot d_t^T \cdot T_{it}^{UT}) + \beta X_i + \varepsilon_{it} \tag{1}$$

where D_{it1} is a dummy variable taking the value 1 for individuals belonging to the treatment group, d_t is a vector of dummy variables for year of graduation, T_{it}^{UT} is the time from first enrollment to implementation of the reform, d_t^T is a dummy variable taking the value 1 if the individual graduated during the treatment period (i.e. the period during which the students knew about the reform and were treated, autumn semester 1991 to autumn semester 1995), X_i is a vector consisting of the control variables (dummy variables for age, gender and the length of the study program - i.e. the number of semesters ranging from 10 to 13 semesters, as well as parental education and immigrant status) described in the appendix in Table 6, and ε_{it} is a random error term. Our parameters of interest are the difference-in-difference parameters γ_1 and γ_2. These measure the effect of the incentive on whether the student completed on time. γ_1 measures the reform effect on students who are fully treated, i.e. who were aware of the reform from the start of their studies and also graduated under the reform. Note that only 4.5 years passed from the reform was introduced before it was discontinued. Thus, few if any students will be fully treated and the estimated effect for the fully treated will involve an extrapolation.

We expect students who were only partly treated - i.e. who started studying before the reform, but graduated during the the reform period - to show a smaller response to the reform. Some students may already have been delayed without opportunity to catch up prior to the reform, such that they would never be eligible for a restitution, and thus the reform had no direct effect on these students. We capture this by letting the reform effect decrease linearly with time from enrollment to reform. The reform effect for a partly treated student will thus be $\gamma_1 + \gamma_2 \cdot T_{it}^{UT}$, with γ_2 expected to be negative. Note that although we model this probability as linear in T_{it}^{UT} it need not be proportional. It may for example be the case that delays most often occur during the last few years studying. In that case there may be a large change in the probability of completion on time by going from not treated at all to being treated one or two

years, but a smaller change by going from treated two years to treated three or four years. In Eq. (1) this will be reflected as $\gamma_1 > -\gamma_2 \cdot T_{it}^{UT,MAX}$, where $T_{it}^{UT,MAX}$ is the highest value of T_{it}^{UT} (about six years).

Although our main focus will be on estimating Eq. (1), we will also presents results from a simpler specification, disregarding time spent untreated:

$$y_{it} = \alpha + \phi D_{it1} + d_t + \gamma(D_i \cdot d_t^T) + \beta X_i + \varepsilon_{it} \tag{2}$$

In this specification our parameter of interest is the difference-in-difference parameter γ. This parameter is the average change in y_{it} in the treatment group over the course of the reform period, minus the average change in y_{it} in the control group over the same time period. Thus, with this specification we estimate the total reform effect during the reform period,[10] pooling students with different time spent studying before the reform.

We will also estimate a more general specification than Eq. (1):

$$y_{it} = \alpha + \phi D_{it1} + d_t + \eta T_{it}^{UT} + \tilde{\gamma}_{1,t}(D_i \cdot d_t) + \gamma_2(D_i \cdot d_t^T \cdot T_{it}^{UT}) + \beta X_i + \varepsilon_{it} \tag{3}$$

In this specification we replace the difference-in-difference parameter γ_1 with a vector of year-specific parameters $\tilde{\gamma}_{1,t}$. Thus, the reform effect for a student that graduates in year t, and have studied T_{it}^{UT} years before the implementation of the reform is given as $\tilde{\gamma}_{1,t} + \gamma_2 \cdot T_{it}^{UT}$. Note that the first term varies with year of graduation, while the parameter in the second term, a term which is meant to capture the effect of delays that have already occurred at the time of implementation, is assumed constant. The reason for introducing this more general specification are as follows. First, within the reform period it allows us to study the dynamics of the introduction of the reform, e.g. to capture how information of the reform spread, or non-linearities in the effect of time untreated. Second, it allows us to introduce "placebo tests" outside of the reform period, as a robustness check for our findings. If we find a significant (placebo) "reform effect" before the introduction of the reform, this may be an indication that what our estimates pick up is not really a reform effect, but rather something else, e.g. differential trends in the share that graduated on time.

We can also not rule out persistent effect after the reversal of the reform. Similarly to some

[10] There may also be a reform effect after the reform period, see below for a discussion.

students being partly treated at the end of their studies, there will be some who are partly treated at the beginning of their studies, i.e. students who study during the reform period, but who graduate after its end. Unless they expect the reform to be discontinued, these students may adapt their behavior during the reform period, and thus have a lower probability of being delayed. If this is the case we should expect to see some continued reform effect after the end of the reform period. Also, the reform may have a lasting influence if the share completing on time changes, and this in turn influences the norms or preferences for completing on time. However, as there should also be a direct effect of the incentive, we would expect any estimated effect after the reversal of the reform to be smaller than the estimated effect during the reform period.

5 Results

The difference-in-difference estimates from Eq.(1) and Eq.(2) are reported in Table 3. The table includes 3 specifications. The first column reports the estimates of the simplest model based on Eq.(2), whereas the second and third specifications (column (2) and (3)), are based on Eq.(1). The difference between the second and third specification is that we in the latter include control variables for students' background.

As can be seen from column (1), the average effect of the reform amounts to 2 percentage points which corresponds to a 10 percent increase in the number of students graduating on time. This average effect does not distinguish between fully treated students (students who were aware of the reform from the start of their studies) and partly treated students (students who were enrolled in higher education one or more years before the implementation of the reform and only had limited possibility to adjust their behavior).

Columns (2) and (3) present results where we control for time untreated while studying. In these specifications the difference-in-difference parameter has the interpretation of the expected reform effect on a fully treated student. The reform is estimated to increase the probability of a fully treated student of graduating on time by 13 percentage points, which translates to a 50 percent increase in the number of students graduating on time. Also, this effects is similar to the baseline difference in completion on time between the reform and control groups (conditional on length of the program and years enrolled prior to the reform). Thus, a fully treated student

Table 3: The effect of financial incentives on graduating on time, estimated by OLS

	Spec1		Spec 2		Spec 3	
Treatment Group	-0.164	(0.009)***	-0.145	(0.009)***	-0.147	(0.009)***
In Treatment	0.024	(0.011)**	0.132	(0.017)***	0.132	(0.017)***
Years enrolled prior to reform			-0.067	(0.002)***	-0.067	(0.002)***
Years enrolled prior to reform*In Treatment			-0.021	(0.003)***	-0.021	(0.003)***
Length of ed program (10 = ref)						
-11 semesters	-0.511	(0.011)***	-0.454	(0.011)***	-0.455	(0.011)***
-12 semesters	-0.393	(0.010)***	-0.304	(0.010)***	-0.300	(0.010)***
-13 semesters	-0.575	(0.012)***	-0.419	(0.012)***	-0.413	(0.013)***
Male					0.027	(0.005)***
Mothers' ed (ref = up sec basic, 11-12 years)						
-<=Low sec (0-10 years)					-0.001	(0.007)
-Up sec, final or post-sec, non-tert (13-14 years)					-0.009	(0.008)
-Tertiary ed, undergrad lev (14-17)					-0.005	(0.006)
->=Tertiary ed, grad lev (18-20+)					-0.015	(0.012)
-Missing inf					0.009	(0.022)
Fathers' ed (ref = up sec basic. 11-12 years)						
-<=Low sec (0-10 years)					0.002	(0.009)
-Up sec, final or post-sec, non-tert (13-14 years)					-0.015	(0.007)**
-Tertiary ed, undergrad lev (14-17)					-0.011	(0.007)
->=Tertiary ed, grad lev (18-20+)					-0.033	(0.007)***
-Missing information					0.019	(0.017)
Immigrant status						
(ref = non-immigrant)						
-Immigrants					0.002	(0.029)
-Norwegian-born to immigrant parents					-0.012	(0.042)
-Foreign-born with one Norwegian-born parent					-0.018	(0.021)
-Norwegian-born with one foreign-born parent					-0.022	(0.010)**
-Foreign-born to Norwegian-born parents					-0.024	(0.016)
-Missing information					0.002	(0.035)
R-square	0.202		0.280		0.283	
N	26871		26871		26871	

Note: Included in all specifications are a constant term, dummy variables for graduation year and dummy variables for length of study program. Standard errors are heteroskedasticity robust. */**/*** statistically significance at the 10/5/1 percent level.

in the treatment group is expected to have a similar probability of completing on time as an otherwise similar student in the control group.

A likely reason for the discrepancy between the average effect in column (1) and the effect for a fully treated student in columns (2) and (3) is, as already pointed out, that students who enrolled in study programs before the reform may already be delayed in their study progression and hence cannot respond directly to the new incentive. We find that for each year enrolled in the higher education system prior to the reform, the chance of graduating on time is reduced by 2 percent points.

Controlling for gender and background variables does not change the diff-in-diff estimates. This is also an indication that also also unobservable individual characteristics are not correlated with being in the treatment group during the reform period. Note further that students enrolled in 10 semester programs have a higher chance to graduate on time relative to students in longer study programs. This finding is also stable across the model specifications. The correlations between the remaining control variables and the probability of graduating on time are mostly statistically insignificant. One exception is that male students have a 3 percentage points higher probability to graduate on time than female students.

In addition to an effect of time untreated, it may be that the reform effect will change during the reform period as students grow more accustomed to the reform, and possibly align their behavior and norms with the reform's intention. In order to test this assumption we present the year-specific difference-in-difference estimates in Table 4. We find indications of an increasing reform effect. The fraction of students completing their studies on nominal time increases (almost) for each year. An alternative interpretation of this finding may be that the year-specific reform effects control for non-linearities in the relationship between time untreated and probability of graduation on time. If this is the case, the findings corresponds to an effect of the reform that increase more than linearly with time treated. Thus, it implies that it is particularly important that treatment starts early, consistent with a hypothesis that study habits matter.

With difference-in-difference studies there is always a concern that the estimate is picking up differential time trends or other shocks. To check the robustness of the reform effect, we investigate the trends in study duration both before and after the reform period for the

Table 4: Year-specific difference in difference variables

	Spec 4		Spec 5	
Treatment Group	-0.148	(0.010)***	-0.149	(0.010)***
In Treatment 1991	0.018	(0.021)	0.016	(0.021)
In Treatment 1992	0.092	(0.023)***	0.094	(0.023)***
In Treatment 1993	0.119	(0.023)***	0.119	(0.023)***
In Treatment 1994	0.116	(0.023)***	0.116	(0.023)***
In Treatment 1995	0.136	(0.023)***	0.135	(0.023)***
Years enrolled prior to reform	-0.070	(0.002)***	-0.070	(0.002)***
Years enrolled prior to reform*In Treatment	-0.013	(0.002)***	-0.013	(0.002)***
Length of ed program (10 = ref)				
-11 semesters	-0.450	(0.011)***	-0.451	(0.011)***
-12 semesters	-0.300	(0.010)***	-0.297	(0.010)***
-13 semesters	-0.415	(0.013)***	-0.408	(0.013)***
Male			0.027	(0.005)***
Controlling for parental background	No		Yes	
R-square	0.280		0.283	
N	26871		26871	

Note: See Table 3

treatment and control groups. We do this by estimating placebo reform "effects" for each year 1988-1998. Table 5 shows that we do find a statistically significant reform effect for most of the reform years (except the year of introduction, 1991), and that we do not find any significantly positive effects in any of the other years.[11]

In particular, we would be concerned about an estimated positive reform "effect" in the years before the introduction of the reform. However, the estimates indicate a somewhat negative "effect" in the period before the reform. This suggests that what we have captured is a pure reform effect and hence no effect of differential time trends. We do find some signs of positive - if insignificant - effects after the reform is discontinued.[12] As discussed in Section 4, this may be weak evidence for some kind of spillover or lasting effect of the reform.

[11]Note that in Tables 3 and 4 the reform effect is estimated relative to the years 1988-1990 and 1996-1998. In Table 5 all effects are relative to 1989, the last year before the introduction of the reform. As differences in completion rates between the treatment and comparison groups varies randomly over time, this influence the values of the estimated coefficients on the reform variables. However, the difference between any of the reform coefficients is unaffected.

[12]In particular if we compare to an average of the pre-reform years, rather than just 1989.

Table 5: Placebo Testing

	Spec 6		Spec 7	
Treatment Group	-0.122	(0.020)***	-0.124	(0.020)***
In Treatment 1988	-0.070	(0.029)**	-0.071	(0.029)**
In Treatment 1990	-0.080	(0.028)***	-0.080	(0.028)***
In Treatment 1991	-0.008	(0.028)	-0.009	(0.028)
In Treatment 1992	0.066	(0.029)**	0.068	(0.029)**
In Treatment 1993	0.093	(0.029)***	0.093	(0.029)***
In Treatment 1994	0.091	(0.029)***	0.091	(0.029)***
In Treatment 1995	0.110	(0.029)***	0.109	(0.029)***
In Treatment 1996	0.013	(0.028)	0.012	(0.028)
In Treatment 1997	-0.035	(0.028)	-0.034	(0.028)
In Treatment 1998	0.013	(0.030)	0.015	(0.030)
Years enrolled prior to reform	-0.070	(0.002)***	-0.071	(0.002)***
Years enrolled prior to reform*In Treatment	-0.013	(0.002)***	-0.013	(0.002)***
Length of study program (10 = ref)				
-11 semesters	-0.450	(0.011)***	-0.452	(0.011)***
-12 semesters	-0.302	(0.010)***	-0.298	(0.010)***
-13 semesters	-0.415	(0.013)***	-0.409	(0.013)***
Male			0.027	(0.005)***
Controlling for parental background	No		Yes	
R-square	0.281		0.283	
N	26871		26871	

Note: See Table 3

5.1 Design of the reform incentive and heterogeneous effects

The average incentive effect on fully treated students is estimated to be very strong. However, the reaction to such an incentive is neither empirically nor theoretically straightforward. The extent to which a student responds to the incentive will naturally depend on how it is designed. Building on incentive theory, we know for instance that incentives are more likely to be effective when the award standards are more short terms, i.e. comes more quickly, perhaps at the end of every term, and not after more than five years as in our case (Holmström and Milgrom, 1987). In addition, the form taken by the subsidy itself - no direct cash reward, but only a relatively small loan reduction (where the remaining part is going to be repaid and discounted over several years) - might feel like a very low powered incentive.

That we despite these theoretical considerations do find quite strong effects indicates that students are quite responsive to financial incentives. One reason may be that students in general are quite constrained financially. Another possibility is that the educational outcome being incentivized - the nominal study duration- is something that the students can easily adjust to without suffering too high effort costs.

Although students on average seem to respond well to the incentive, the effect may vary across students due to for instance relative difference in financial positions, academic preparation (e.g. Leuven et al., 2010), access to information, and the interactions of these factors. However, we do not find any significant evidence that students of higher educated parents respond differently to the incentive than students of lower educated parents.

6 Conclusion

Ensuring equal opportunities in access of higher education a central aim of policy makers. An important policy instrument in this regard has been state provided loans and grants for students. However, as this support reduces the marginal cost of studying, it may have undesired consequences in the form of reduced study efficiency. In this paper we investigate the effects on study duration of a reform that rewarded students who completed their higher education degree on nominal time.

We find strong effects of an incentive aiming at increased study efficiency. The number of students that graduated on nominal time rose with about 10 percent. Thus, for 10 students

given a restitution, one would otherwise not have graduated on nominal time. However, the scope for the the reform was reduced by the fact that many students had a limited possibility to adjust their behavior. The estimated effect on a fully treated student, which corresponds to the expected effect of the incentive if it is in operation for a longer time, is much higher, indicating an increase of 10 percentage points, i.e. 50 percent of the base probability or one additional student graduating on time for every third restitution given.

This suggests that students respond strongly to financial incentives. The reason might be that students in general are quite constrained financially and/or that students can quite easily adjust their 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 this result. Potential mechanisms may include increased study intensity, for example by reducing part-time work, or graduation with a smaller amounts of human capital, which could result in lower earnings after graduation. Both may or may not be undesired, depending on the strengths of the effects. To distinguish between such potential mechanisms, and to investigate further consequences and the desirability of the reform requires further research.

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A Appendix

Table 6: Descriptive statistics for the estimation sample, reported are fractions

Students graduating on time (share)	0.265
Female (share)	0.511
Average age end of high school	19.1
Mother's education (share)	
-<=Low sec (0-10 years)	0.140
-Up sec basic, 11-12 years	0.370
-Up sec, final or post-sec, non-tert (13-14 years)	0.116
-Tertiary ed, undergrad lev (14-17)	0.302
->=Tertiary ed, grad lev (18-20+)	0.047
-Missing inf	0.026
Father's education (share)	
-<=Low sec (0-10 years)	0.100
-Up sec basic, 11-12 years	0.222
-Up sec, final or post-sec, non-tert (13-14 years)	0.163
-Tertiary ed, undergrad lev (14-17)	0.219
->=Tertiary ed, grad lev (18-20+)	0.260
-Missing inf	0.036
Immigrants	0.103
Total number of students	26,871