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Graduation Incentives Through Conditional Student Loan Forgiveness

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ABSTRACT

Graduation Incentives Through Conditional Student Loan Forgiveness*

We evaluate a Finnish student financing reform which created substantial financial incentives for on-time graduation, and had the side effect of turning expected nominal interest rates on student loans strongly negative. We find that both the timing of graduation and the take-up of loans remained unaffected by the reform. This is consistent with earlier findings in the literature that students do not seem to process financial incentives well when framed as a loan.

JEL Classification: I22

Keywords: higher education, student finance, incentives, pace of studies, student loans

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1 Introduction

In many countries, higher education is heavily subsidized, among others through direct student financing. The effects of such subsidies on enrollment are relatively well known, with larger subsidies tending to increase enrollment.[1] Subsidies however not only create incentives to enroll, but also to remain in education rather than to graduate. To the degree that the latter does not correspond to the policy maker's aims, this raises the question whether student financing can be restructured to encourage timely graduation instead.

The effects of incentives related to student grants and bonuses often have the expected sign. For example, Häkkinen & Uusitalo (2003) find that a restriction of student aid durations seems to have decreased the time to graduation in fields with high average times to graduation. Garibaldi et al. (2012) find that a tuition profile increasing in elapsed study duration encouraged timely graduation at Bocconi University, and Heineck et al. (2006) find that an increasing tuition profile at the University of Konstantz encouraged exit from the university, in some fields mainly by graduating earlier, in other fields by exiting without a degree. In Scott-Clayton (2011), scholarships with merit requirements have positive effects on contemporary academic outcomes, even if such effects may be heterogeneous across groups (cf. e.g. Angrist et al., 2009; Leuven et al., 2010).

Effects of incentives related to student loan schemes are harder to predict, with students' behavior perhaps being affected by a combination of framing effects and debt aversion. Field (2009) describes an experiment in which NYU Law School students were either offered a conditional grant or a financially equivalent loan with conditional loan forgiveness. Students did not only seem to value the offered aid package higher when presented as a grant, they also responded much more strongly to the incentives offered when framed as related to a grant recipiency rather than related to loan forgiveness. Field shows that such behavior is unlikely to be due to students not understanding the terms of the aid package. In a similar fashion, Booij et al. (2012) find that a lack of information is not behind Dutch students' reluctance to take up financially attractive student loans.

In this paper, we study a 2005 Finnish student finance reform which aimed to incentivize timely graduation by writing off a proportion of the student loans of on-time graduates. Though the reform turned expected interest rates on student loans strongly negative, loan take-up remained unchanged

[1] See e.g. Scott-Clayton (2015) for a recent overview.

at its previous low levels. We do not find evidence of a graduation response of any economically meaningful magnitude either. This stands in contrast to Gunnes et al. (2013), who find find strong graduation responses to the introduction of a loan related financial incentive in Norway. Though the Finnish incentive was of similar magnitude as the Norwegian one for students taking out the maximum loan, the persistently low student loan take-up rates reduced the effective size of the incentive in Finland, perhaps partially explaining the lack of an effect on graduations there.

2 Institutional background

Finnish higher education is provided by polytechnics and universities, with students typically graduating with a Bachelor's degree from the former, and with a Master's from the latter. Admission into higher education is extremely competitive, with less than a third of applicants being admitted to polytechnics in any given year, and an even lower proportion being admitted to universities.

For the vast majority of students, higher education is tuition free, and student grants as well as meal and housing subsidies are relatively generous. It is possible to take government-guaranteed, commercial loans on top of these grants. Interest rates on these loans are comparable to the yields on Finnish government bonds, and low in absolute terms. Furthermore, additional loans are automatically granted to cover interest payments as long as the student is still eligible for student aid. The repayment schedule after graduation is negotiable, but it is typically about a decade in length, starting a few years after graduation.

Finnish students spend substantial amounts of time working during their studies, graduate at a mean age that is among the highest in the OECD (OECD, 2014), and should expect high earnings after graduation (Koerselman & Uusitalo, 2014). At the same time, loan take-up rates are low, and average loan amounts small, especially compared to Anglo-Saxon countries or even to Finland's Nordic neighbors (cf. Figure 1). Instead, average student loans for recent university graduates are closer to those seen in the Netherlands and Germany a decade earlier. (Usher, 2005; Studiestöd i Norden, 2014). This suggests that it would be possible for students to accelerate their studies by working less on the side, and financing their studies with loans instead.

In an attempt to encourage on-time graduation, a student finance reform was announced in Finland during the first months of 2005. Students who would

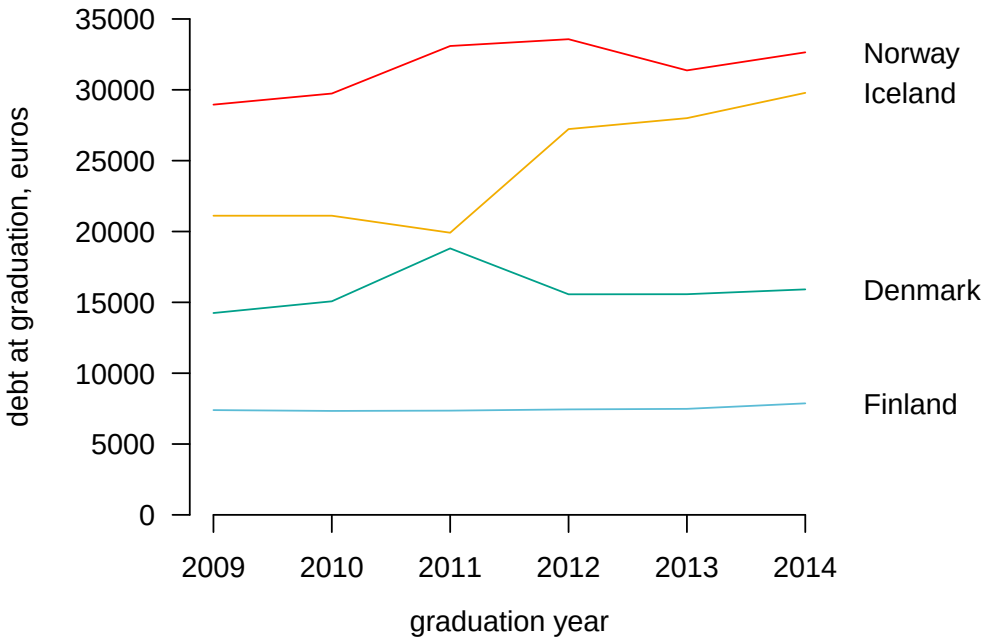

Figure 1: Trends in average student loan balances at graduation for student loan takers in four Nordic countries. Data from the countries' national student financing agencies, as collected by Studiestöd i Norden (2014).

enroll in higher education for the first time in the fall term of 2005 or later had 30% of the part of their student loan that exceeded EUR 2500 automatically deducted from their taxes payable during the years after graduation if they managed to complete their studies within a certain number of years. It should be noted that since many social security benefits are taxable in Finland as well, this does not necessarily require graduates to be employed in order to receive the benefit. In addition to the graduation incentive, monthly loan caps were increased for all students from the fall term of 2015 onward from EUR 220 to EUR 300.

The reform did not only incentivize students who were willing to take a loan of at least EUR 2500 to graduate by the deadline, it also pushed expected interest rates on student loans well into negative territory, incentivizing loan take-up itself. For university students in standard-length 300 ECTS programs, the reward associated with taking out the maximum loan was in excess of EUR 4200 (with the exact sum depending on the interest rate), while for example the same figure was in excess of EUR 2985 for students in 210 ECTS polytechnic programs. Since the interest rates on these loans are

low even in the absence of the graduation reward, the principal could always be saved and used to pay off the loan after graduation. It is therefore hard to see how any student would be substantially worse off by taking out the maximum loan. On the contrary, taking out the maximum loan makes the student eligible for the maximum reward should she graduate on time. The financial incentive therefore increased the attractiveness of the loans for all students, but increasingly so the more likely they were to be able to graduate on time.

Even if the financial incentive offered encouraged slow students to graduate faster, for almost half of the students the financial incentive deadline was not binding in the desired direction. For about a third, it could in fact have encouraged them to delay graduation with at least one semester so that graduation coincided with the deadline. This is because delayed graduation allowed students to accumulate larger loans, and therefore a larger reward. Since both slow and fast students were thus incentivized to move their graduation to the deadline, we should expect any incentive effects on graduation rates to be most pronounced at each program's deadline.

We are unaware of any simultaneous reforms in Finnish polytechnics. University students were however affected by a simultaneous degree structure reform that divided up university programs into more explicit Bachelor's and Master's phases, causing changes to the number of credits associated with individual courses. Pre-2005 cohorts were allowed to complete their degree within the old degree structure, but were in that case required to graduate before the start of the 2010 academic year if they were students of engineering, dentistry, medicine or veterinary sciences, or before the start of the 2008 academic year otherwise. Even if students in practice seem to have been able to switch to the new degree structure with relative ease, unusually large numbers of control group university students graduated at the 2008 and 2010 degree structure reform deadlines.

3 Data

We use data from the Finnish Social Insurance Institution KELA. KELA is responsible for administering student benefits, and for this reason it collects graduation records from Finnish institutes of higher education, as well as student loan balances from Finnish banks.

From the full KELA database, we select students who enrolled in a Finnish institute of higher education during the summer application rounds of 2003,

2004, 2005, and 2006. The first two of these cohorts were untreated by the financial incentive, the second two treated. Because treatment is determined by the year the student enrolled in higher education for the first time, we only include first time enrollees in each cohort. Enrollment information is incomplete before 1998, and we therefore also exclude enrollees who in the five years preceding their enrollment had a record of previous study benefit use, or who were 30 years or older at the time of enrollment.

Many programs have a secondary application round in winter, during which they accept much smaller numbers of students than during the summer application round. These students will have a pace of study that differs from those enrolled in summer since they will typically follow a course schedule designed for summer enrollees. Since few students are winter enrollees, we drop these observations. We also drop the small number of students that enroll in 180 or 330 ECTS university programs or in 270 ECTS polytechnic programs.

Our final sample contains a total of 115666 students, of which about 62% enrolled in polytechnics, and 38% in universities. As can be seen from Table 1, the number of students in our sample is slightly smaller in the younger cohorts. This seems to be due to an increase in the average age of first-time enrollees rather than due to a decrease in the total number of enrollees in the population. Removing the age restriction or controlling for age however makes little difference for our results.

Table 1: Number of sample students in each of the four sampling cohorts enrolling in programs with different financial incentive deadline lengths.

			sample size by enrollment year			
ECTS	program length	years to deadline	2003	2004	2005	2006
Polytechnic programs						
210	3.5	4.5	9738	9871	9638	9502
240	4	5	8516	8486	8119	7533
University programs						
300	5	7	11010	10764	10685	10358
360	6	8	370	358	355	363

Our outcome variables are based on students' semi-annual loan balances and on their graduation dates. In our data, the former are available up until the

summer of 2012, the latter until the summer of 2013. Because monthly loan caps were increased from EUR 220 to EUR 300 for all cohorts from the fall term of 2005 onward, treated cohorts could accrue a slightly larger maximum loan than could control cohorts. Moreover, since the large majority of students take loans in multiples of these monthly caps, the raised caps may also have induced students for whom maximum loan amounts were not binding to take larger loans on average. For this reason, we use as the outcome a semester-level variable indicating whether the individual took out an additional student loan in that semester, as well as a variable indicating whether the individual took out any student loan during the six year observation period. Especially the latter should not be affected by changes in euro amounts available.

Students can graduate with multiple degrees. For polytechnic students we use as the graduation date the date of the first polytechnic Bachelor's degree, for university enrollees we use the first Master's degree. Polytechnic students who obtain a university Master's but no polytechnic Bachelor's, as well as university students who obtain a Bachelor's degree only are thus counted as having never graduated. These are very few. We do however count as having graduated students who graduate in a different field than they originally enrolled in. Since exact graduation dates vary from year to year, we aggregate graduations to the semi-annual semester level.

As background variables we use the student's gender and field of study at first enrollment. We also use father's and mother's taxable income, which we deflate to 2012 prices using Statistics Finland's Cost-of-living Index (1951:10 = 100). We average these incomes over the calendar year of enrollment and the five years following it, excluding missing values but including zeroes, and censor this average at zero and at the 99th percentile. We additionally create indicator variables for when father's or mother's taxable incomes are missing for all six years, for example when the parent is unknown or lives abroad.

Variable means for treated and control cohort students can be be found in Table 2. Intra-cohort differences in these variables are almost negligible overall, with the exception of real parental incomes, which are a few percent larger for the younger cohorts.

As described in the previous section, the tax subsidy provided incentives to graduate sooner for a limited share of students only. Table 3 shows how 54% of control cohort students graduated after their counterfactual deadline, and would thus have had an incentive to graduate sooner had they been treated. At the same time, 34% of control cohort students graduated at least one semester before the deadline, and may rather have been incentivized to delay

Table 2: Descriptive statistics for 2003 and 2004 control cohorts as well as for 2005 and 2006 treatment cohorts.

	control	treated
female	0.55	0.55
polytechnic fields		
engineering	0.33	0.31
social sciences and business administration	0.27	0.27
health and social care	0.18	0.18
shipping and tourism	0.11	0.12
fine arts and culture	0.08	0.08
agriculture, forestry and the environment	0.03	0.04
university fields		
engineering	0.25	0.24
humanities	0.20	0.20
hard sciences	0.18	0.18
social sciences	0.13	0.14
business administration	0.11	0.12
educational science	0.07	0.07
medicine	0.03	0.03
agriculture, forestry and the environment	0.02	0.02
pharmaceutical science	0.01	0.01
father's mean taxable income (EUR)	46756	48104
mother's mean taxable income (EUR)	31843	33420
took student loan in semester	0.32	0.32
took student loan ever	0.50	0.51

Notes: Deadlines for students in control cohorts are counterfactual. Euro amounts have been deflated to 2012 price levels.

their graduation under the new system in order to maximize the size of their loan.

Figure 2 shows separately by field and program length the proportion of control cohort students remaining ungraduated at different numbers of semesters after enrollment. The financial incentive deadlines have been indicated with vertical lines in the figure, while the dots represent the proportion of students not graduated by the deadline within each field. The vertical spread of the dots illustrates the large between-field variation in the proportion of students graduating on time, even when comparing programs of the same length, as indicated by the dots' horizontal position.

Table 3: Sample proportion of control cohort students graduating at least one semester before their counterfactual financial incentive deadline, proportion graduating exactly on time, as well as the proportion of students graduating late.

program length	proportion early	proportion on time	proportion late
3.5	0.41	0.09	0.50
4.0	0.23	0.17	0.59
5.0	0.34	0.11	0.55
6.0	0.76	0.07	0.17
weigthed total	0.34	0.12	0.54

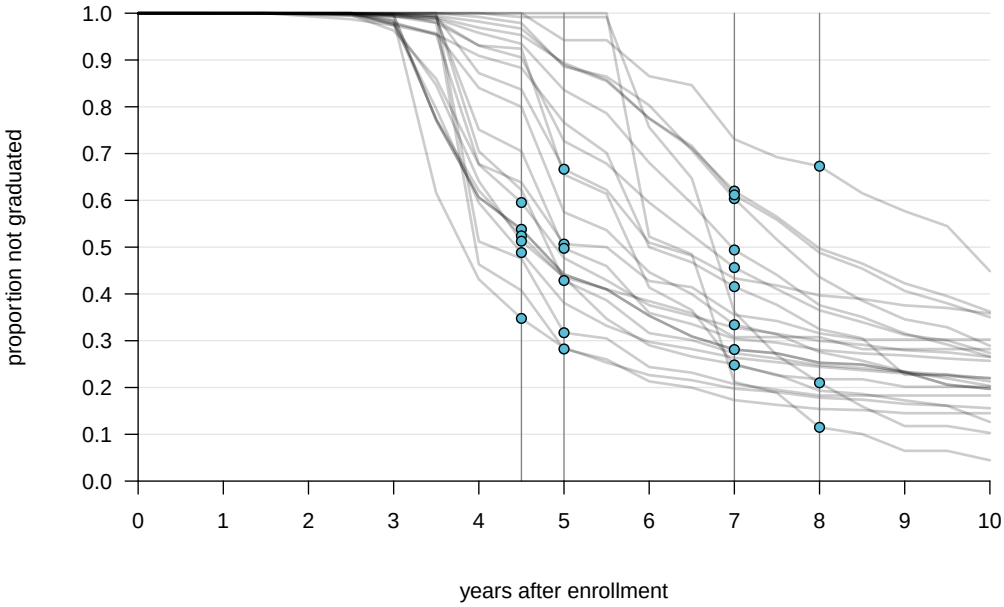

Figure 2: Survival curves by field. The figure shows the proportion of control cohort students not graduated as a function of time since enrollment separately for each combination of field and program length. Vertical lines indicate financial incentive deadlines, dots the proportion of students not graduated by their deadline in each field. There is large between-field variation in the proportion of students graduating on time.

4 Methods

4.1 Loan take-up

In our data, treatment is determined by cohort membership. A simple comparison between cohorts may however pick up not only the effect of the incentive, but also the effects of whatever other factors may differ between cohorts.

As an additional source of identification, we make use of the fact that the treatment effectively had a different intensity for different students in the treatment group: the higher the student's expected probability of graduating on time, the more attractive the financial incentive made the loans. We thus interact treatment with the proportion of students graduating before the deadline by each field of study by program length combination in the control

cohorts:

$$\begin{aligned}\mathbb{P}(\text{loan})_{i,t} &= \alpha + \beta_1 treatment_i + \beta_2(treatment_i \times ontime_i) \\ &\quad + X_i\beta_3 + D_i\beta_4 + D_{i,t}\beta_5 + \varepsilon_{i,t}\end{aligned}$$

where i denotes the individual, t the semester, X_i contains individual-fixed background variables, D_i contains dummy variables for each combination of field of study and program length, and $D_{i,t}$ dummy variables for semesters elapsed since enrollment as well as for calendar time semesters. Note that the uninteracted *ontime* variable is omitted because it is perfectly predicted by the field of study by program length dummies. Finally, $\epsilon_{i,t}$ is the individual/semester specific error. We cluster these at the individual level.

While we cannot observe students' private information on their time to graduation, between-field variation in on-time graduations is large, and students in fields with larger shares of on-time graduates should on average be more optimistic about their pace of study. We therefore expect true effects of the financial incentive on loan taking to show as a positive estimate of the $treatment \times ontime$ interaction.

4.2 On-time graduation

The incentive to take loans is not only higher for students more likely to graduate on time, the incentive to graduate on time is also higher for students willing to take on large loans. It would therefore be natural to estimate effects on on-time graduation using a similar specification. In practice however, Finnish students' loan taking behavior will turn out to be almost completely independent of their observable characteristics. Instead, we make use of the fact that students were not incentivized to graduate earlier in general, but rather to graduate specifically at the deadline. We therefore estimate a set of discrete hazard models that are variations of

$$\begin{aligned}\text{logit}(\mathbb{P}(\text{graduation})_{i,t}) &= I_{i,t}\alpha + (I_{i,t} \times treatment_i)\beta \\ &\quad + X_i\gamma_1 + D_i\gamma_2 + D_{i,t}\gamma_3 + \varepsilon_{i,t}\end{aligned}$$

where i again denotes the individual and t the semester. $I_{i,t}$ contains three dummy variables indicating whether the observation is in a semester more than one semester before the deadline, in the semester ending in the deadline itself, or in a semester after the deadline. X_i again contains individual-fixed background variables, D_i contains field of study by degree length dummies. $D_{i,t}$ contains dummy variables indicating the number of semesters passed

since enrollment. Since few students graduate immediately after enrollment, we combine some early instances of $D_{i,t}$. Again, $\epsilon_{i,t}$ is the individual/semester specific error, clustered at the individual level. Because the reward is structured in a way that incentivizes graduating exactly on time, we expect any treatment effect on graduation rates to show up as a higher estimated graduation rate specifically for treated students at their deadline, regardless of the sign of treatment effects at other distances to the deadline.

A threat to identification could come from selection into treatment by delayed enrollment. This however seems unlikely due to the fact that the reform was announced months after students would have to have made the decision to delay. Furthermore, while many students delay their enrollment by one year after being admitted, we find no evidence that the number of delayed enrollments would have increased. The reform may in theory also have affected field choice, pushing students into programs in which it is easier to graduate quickly. In practice, such behavior is severely limited by the scarcity of higher education slots, and we control for field of study in most specifications.

Though we are unaware of any simultaneous reforms in Finnish polytechnics, the 2008 and 2010 degree structure reform deadlines caused large spikes in graduation rates among pre-2005 university cohorts. No Finnish university students were completely unaffected by either the degree structure reform deadlines or by the financial incentive deadlines, making it hard to identify the effect of either. We therefore concentrate our analysis on the larger group of polytechnic students.

5 Results

5.1 Loan take-up

Table 4 shows the estimates from a series of linear probability models of loan take-up among polytechnic students. In the first column, we regress the probability of taking a new loan during any single semester on treatment and a constant only. Treated students were 1 percentage point less likely to take out any new student loan during any given semester, from a baseline of about 34%.

The estimate decreases only slightly in magnitude when we add controls for gender, parental income, and combinations of field of study and program length in the second column. The similarity of the estimates is due both due to cohorts being similar to each other in terms of background variables and

due to the fact that these variables jointly explain only a small proportion of the total variation in loan take-up, as evidenced by the low R^2.[2] In the third column, we furthermore add fixed effects for semesters passed since enrollment as well as for calendar time semesters. This somewhat improves R^2, and effectively reduces the adjusted between-cohort difference to zero.

In the fourth column, we add the interaction with the field-specific proportion of students graduating on time. The estimates indicate that treated students in fields in which an additional 10 percent of students graduate on time are 0.09 percentage points less likely to take out a student loan in any given semester. This interaction is however not statistically distinguishable from zero.

In columns five through seven we repeat the analysis using the probability of ever taking out a loan as the dependent variable. Because this aggregates information to the student level, we cannot include time fixed effects in these specifications. The resulting estimates are negative too, but very close to zero. None of the estimates suggest that polytechnic students reacted to the availability of negative expected interest rate loans by increasing their loan take-up.

In Table 5, we repeat the analysis on the full sample, in which university students are also included. Some of the point estimates are now positive, and occasionally significantly so. It is thus possible that small numbers of university students did in fact react to the incentive by taking out (more) student loans, but we cannot exclude such alternative explanations as a type I error caused by sampling variation, or a bias caused by the degree structure reform. The lack of unadjusted between-cohort differences in loan take-up, the relatively precisely estimated zeros for the sample of polytechnics students, and the small magnitude of the estimates in the full sample however all make it seem extremely unlikely that the financial incentive had an aggregate effect on loan take-up of any economically meaningful magnitude.

Given the lack of a loan-taking response, we would like to know whether students were aware of the incentive at all. Since students still had time to take out substantial student loans after the first of their peers graduated and received the reward, we find it unlikely that all (ungraduated) students were strictly unaware of the incentive. On the contrary, we would expect an initial absence of information about the incentive to cause students to

[2] Though it can be seen as an intermediate outcome, we also experiment with adding the student's own labor income in the calendar year of enrollment, i.e. mostly preceding enrollment, as a control. This does not substantially affect the estimates, nor does it improve R^2.

learn about it during their studies, either from their instructors or from each other. If that were the case, program ID should explain a larger part of the total variance in loan take-up in the treated cohorts. We decompose the total variance in loan take-up into within and between cohort components using a random effects model, and find that this is not the case. Program ID only explains about 2% of the total within-cohort variance in loan take-up, with the number being slightly lower in the treated cohorts regardless of whether we include university students in the sample or not.

At the suggestion of multiple (econ) seminar audiences we experiment with interacting treatment with being a student of economics. Though the interaction is positive, it is not statistically distinguishable from zero. The interaction is negative for business school students.

Table 4: Regression results on loan take-up, polytechnic students.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
dependent variable	P(new loan)	P(new loan)	P(new loan)	P(new loan)	P(any loan)	P(any loan)	P(any loan)
mean dependent variable	0.336	0.336	0.336	0.336	0.515	0.515	0.515
treatment	-0.010	-0.008	-0.001	-0.001	-0.002	0.000	0.000
	(0.003)	(0.003)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)
treatment × ontime				-0.009			-0.035
				(0.023)			(0.031)
intercept	yes	yes	yes	yes	yes	yes	yes
field FE	no	yes	yes	yes	no	yes	yes
other controls	no	yes	yes	yes	no	yes	yes
semesters since enrollment FE	no	no	yes	yes	no	no	no
halfyearly calendar time FE	no	no	yes	yes	no	no	no
individuals	71403	71403	71403	71403	71403	71403	71403
observations	721720	721720	721720	721720	71403	71403	71403
R^2	0.000	0.019	0.042	0.042	0.000	0.018	0.018

Outcomes are the halfyearly probabilities of taking any new student loans during any given semester (specifications 1–4), and the probability of ever taking a student loan at all during the observation period (specifications 5–7). Data cover the first six years after enrollment for all sample students. Note that the estimated treatment effect is evaluated at the mean of the *ontime* interaction term in specifications (4) and (7). Controls are indicator variables for all combinations of field and program length, gender, mean father's and mother's taxable income, their squares, as well as indicator variables for when these are missing, indicator variables for the number for semesters since enrollment, and indicator variables for calendar time semesters.

Table 5: Regression results on loan take-up, all students.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
dependent variable	P(new loan)	P(new loan)	P(new loan)	P(new loan)	P(any loan)	P(any loan)	P(any loan)
mean dependent variable	0.319	0.319	0.319	0.319	0.504	0.504	0.504
treatment	-0.003	-0.001	0.005	0.005	0.009	0.010	0.010
	(0.002)	(0.002)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
treatment × ontime				0.024			0.010
				(0.018)			(0.024)
intercept	yes	yes	yes	yes	yes	yes	yes
field FE	no	yes	yes	yes	no	yes	yes
other controls	no	yes	yes	yes	no	yes	yes
semesters since enrollment FE	no	no	yes	yes	no	no	no
halfyearly calendar time FE	no	no	yes	yes	no	no	no
individuals	115666	115666	115666	115666	115666	115666	115666
observations	1237721	1237721	1237721	1237721	115666	115666	115666
R^2	0.000	0.023	0.052	0.052	0.000	0.021	0.021

Outcomes are the halfyearly probabilities of taking any new student loans during any given semester (specifications 1–4), and the probability of ever taking a student loan at all during the observation period (specifications 5–7). Data cover the first six years after enrollment for all sample students. Note that the estimated treatment effect is evaluated at the mean of the *ontime* interaction term in specifications (4) and (7). Controls are indicator variables for all combinations of field and program length, gender, mean father's and mother's taxable income, their squares, as well as indicator variables for when these are missing, indicator variables for the number for semesters since enrollment, and indicator variables for calendar time semesters.

5.2 Time to graduation

Figure 3 shows survival curves for polytechnic students, with the proportion of students remaining ungraduated indicated at different numbers of semesters after enrollment. Treated students' graduations have been indicated with a black curve in the figure while control cohort students are shown in gray. The two survival curves lie practically on top of each other. Treatment cohorts graduate slightly faster towards the end of the observation period, but only after the financial incentive deadlines (indicated with black dots and darker vertical lines) have come and gone.

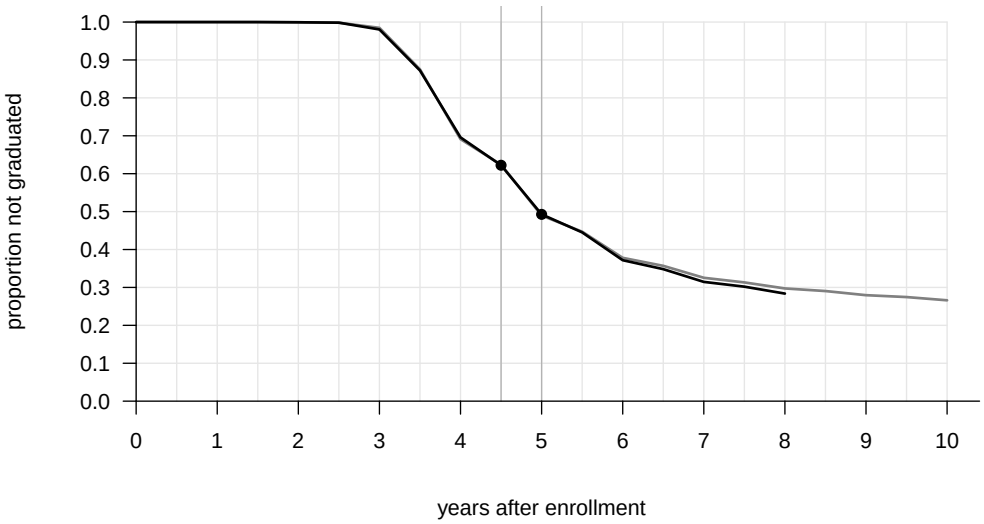

Figure 3: Polytechnic students' survival curves. Treatment (black curve) and control (gray curve) cohorts have virtually identical raw graduation rates. Black dots and darker vertical lines indicate financial incentive deadlines for treatment cohorts in 3.5-year and 4-year programs respectively. Graduation rates are substantially similar at the deadlines for treatment and control cohorts.

In Figure 4, we have split the treated polytechnic students by cohort and program length. To each of the resulting four treatment survival curves we have then added the time-shifted survival curve of the equivalent control cohorts in gray. Again, treatment survival curves lie practically on top of the comparison curves, making the latter hard to make out at all. The figures leave little room for either large differences in baseline graduation rates between cohorts in the absence of the incentives, or for substantial

calendar year effects. Most notably, no differences in graduation rates can be seen specifically at the financial incentive deadlines indicated by the black dots in the figure, where we would expect the difference in graduation rates between treatment and control cohorts to be most pronounced.

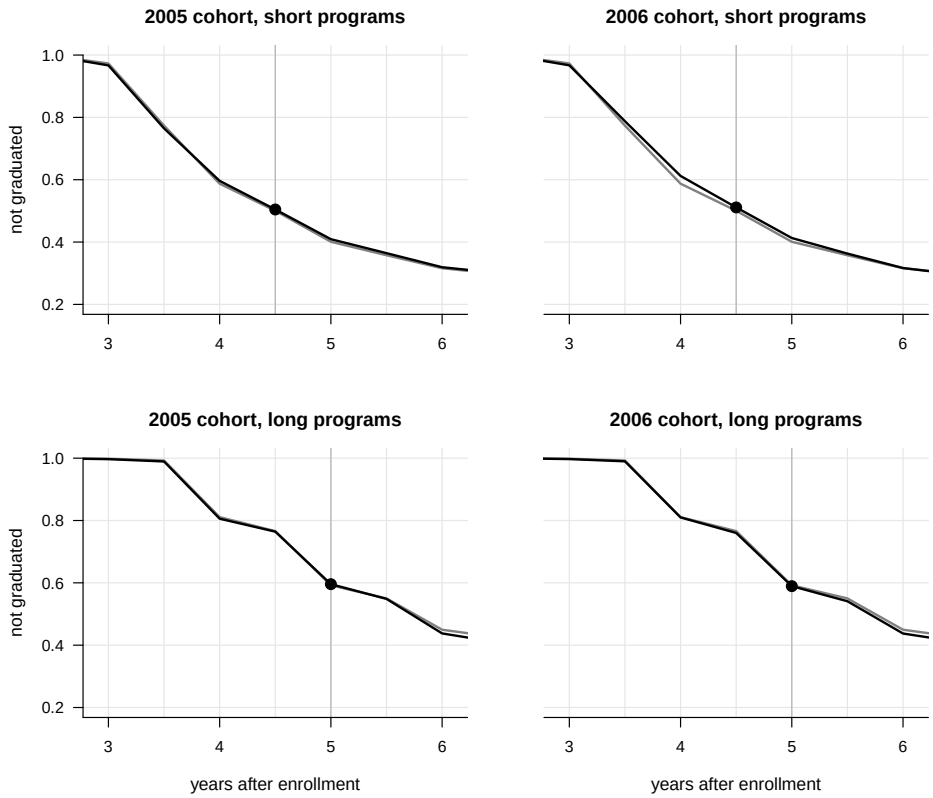

Figure 4: Polytechnic students' survival curves for 2005 and 2006 treatment cohorts separately for 3.5 (top panel) and 4 (bottom panel) year programs. Barely visible underneath the treated students' survival curves are control cohort survival curves in gray. Black dots and darker vertical lines indicate treatment cohort financial incentive deadlines.

Though the graphical analysis of polytechnic students' graduations above leaves little room for substantial effects of the financial incentive on graduation rates, we want to compute economically meaningful point estimates and associated standard errors of what differences between treated and untreated students there may be.

We start by estimating the average hazard conditional on treatment only. As can be seen from the average marginal effects in column (1) of Table 6, treatment cohorts experienced slightly larger graduation hazards on average, of the order of a 0.1 percentage point larger graduation probability in any

Table 6: Impacts of the financial incentive on polytechnics students' graduation hazards.

	(1)	(2)	(3)
mean dependent variable	.0627	.0627	.0627
treatment	0.0011	0.0005	
	(0.0004)	(0.0005)	
treatment × before deadline			-0.0014
			(0.0007)
treatment × at deadline			0.0010
			(0.0018)
treatment × after deadline			0.0029
			(0.0009)
intercept	yes	yes	no
before deadline	no	no	yes
at deadline	no	no	yes
after deadline	no	no	yes
field FE	no	yes	yes
semester FE	no	yes	yes
other controls	no	yes	yes
individuals	71403	71403	71403
observations	773677	773677	773677

Average marginal effects. Estimates based on a discrete hazard model with a logit link function. Outcomes are the graduation hazard in each semester. Controls are field indicators, semester after enrollment indicators, gender, father's and mother's average taxable income, their squares, and indicators of when these are missing. Individual-clustered standard errors in parentheses.

single semester, from a baseline of 6.27%. In the second specification, we add controls for field of study, parents' taxable incomes, their squares, and missing indicators, as well as gender. We also add indicator variables for the number of semesters elapsed since enrollment. After controlling for these factors, the between-cohort difference shrinks to roughly half the raw difference at 0.05 percentage points.

In the third column, we interact treatment with indicators of whether any student/semester observation occurred more than one semester before the student's financial incentive deadline, in the semester leading up to the financial incentive deadline, or after it. If increased graduation rates in the treated cohorts are an effect of the financial incentives, this should show up as an increased number of graduations at the deadline, or potentially before it, but certainly not after the deadline has passed. This is not the pattern that

is visible from the table. The number of students remaining ungraduated by the deadline is in fact larger in the treatment group, with the increased graduation rates among treated cohorts only occurring after the deadline has passed. This makes it seem unlikely that even the small estimates in columns (1) and (2) represent causal effects of the incentive.

Unadjusted survival curves for university students can be seen from Figure 5, with treated 2005 and 2006 cohorts indicated separately in black, and the untreated 2003 and 2004 cohorts in gray. As was the case with the polytechnic students, the *treated* university students show identical graduation patterns to each other, the two black curves in fact lying exactly on top of each other in the figure. The two control cohort university students however experienced higher graduation rates at the 2008 and 2010 degree reform deadlines, and their survival curves are substantially different both from each other and from those of the treated cohorts. The difference is particularly visible after 5 years for the 2003 cohort in the figure. The reaction to the degree structure reform deadline suggests that it possible for Finnish students to graduate faster if suitably motivated. It is also noteworthy that the survival curves are flatter immediately after the degree structure reform deadlines, a pattern which suggests that substantial numbers of students who would otherwise have graduated immediately after their deadline moved their graduation forward by one semester to coincide with the deadline instead.

Towards the end of the observation period, around the time of the financial incentive deadlines indicated by the black dots in the figure, treated cohorts catch up to control cohorts in terms of cumulative graduations. The higher graduation rate among treated students may however not be due to the financial incentive, but rather to the corresponding control cohort students already having graduated earlier. Particularly telling in this respect is that treated students do *not* show unusually low graduation rates after the financial incentive deadline like the students responding to the degree structure reform deadlines did after passing theirs. Rather, treated students continue to graduate at higher rates after their deadlines.

Though the effect of the financial incentive on university students' graduation rates is fundamentally unidentified, the lack of a substantial loan taking response, the absence of a graduation response among polytechnic students, and an absence of a reduction in graduations immediately after the financial incentive deadline all suggest that any effects on university students' graduation rates are small at best.

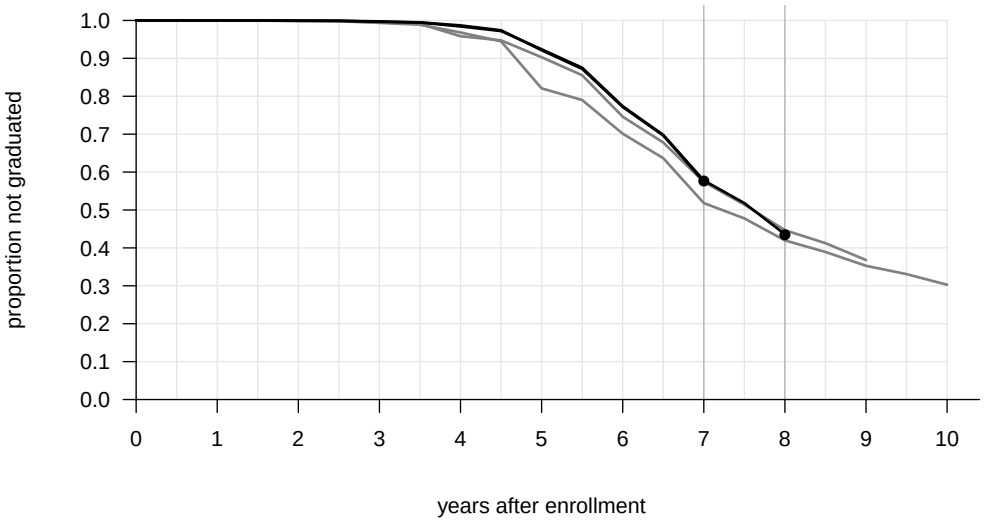

Figure 5: University students' survival curves. Control cohorts (gray curves) see their graduation rates spike early on because of a simultaneous degree structure reform. Treatment cohorts (black curves) catch up around the time when they hit their financial incentive deadlines (black dots).

6 Discussion

In this paper, we evaluate a Finnish student finance reform which rewarded on-time graduation by forgiving 30% of the student loan balance exceeding EUR 2500. Though expected interest rates on student loans were pushed well into negative territory by the incentive, transferring thousands of euros to on-time graduates willing to take substantial student loans, we can exclude even small effects on loan take-up.

For polytechnic students, we find that the policy did not affect graduation rates either, while for university students the effect is poorly identified, but appears to be small at best. A partial explanation may be that in the absence of a loan taking response, the average reward among all treated on-time graduates turned out to be only around 600 euros. This stands in contrast to a similar policy in Norway, where baseline loan take-up was much higher, the effective size of the reward was multiple times larger, and the policy had a substantial effect on graduations (Gunnes et al., 2013).

For Finnish policy makers, our results suggest that the financial incentive mainly functions as a lump sum transfer scheme to graduates. In other countries this might at least encourage enrollment into higher education, but Finnish institutes of higher education are already heavily oversubscribed. That the funds used for the financial incentive are therefore likely better used elsewhere.

To researchers and policy makers outside Finland, the question remains why Finnish students fail to react to the incentives offered. One possibility is that students were strictly unaware of the incentive, newspaper coverage and information from the Finnish Social Insurance Institution KELA notwithstanding. While this may be a somewhat plausible explanation when looking at behavior immediately after enrollment, we however find that loan take-up is unaffected even after up to six years, when some students had already received the reward. This suggests that a lack of information in the most fundamental sense is not behind the lack of a response.

Finland has low baseline take-up rates of student loans, potentially pointing to psychological barriers to student loan take-up. In and of itself, this does not explain the lack of a *change* in loan take-up. However, neither information made available by KELA nor newspaper coverage seems to have framed the changes made to the student financing system explicitly as the introduction of an incentive or reward, but more as the introduction of a conditional transfer. It is possible that the mental step from condition to incentive was too large for most students, especially if their reasoning is clouded by debt-

aversion.

While the literature suggests relatively consistent effects of grants on post-enrollment behavior, responses to loan-related schemes seem less consistent. In line with the earlier literature, our findings therefore suggest that incentives built into student financing should perhaps be framed as grants given as rewards rather than as loans given with conditions.

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