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The impact of public student grants on drop-out and completion of higher education – evidence from a student grant reform

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Health Economics Papers
2008:10

Abstract

There is limited evidence on how financial aid affects drop-out from or completion of higher education. Using a large-scale reform of the Danish student grant system to identify causal effects, discrete duration models for time-to-drop-out and time-to-completion are estimated on reliable register data. Intention-to-treat (ITT) and instrumental variable (IV) estimates of the impact of the reform and study grants are presented using various specifications of the reform effect. Both sets of parameters (ITT and IV) indicate that the reform and grants are negatively related to drop-out, although not supported by all specifications, whereas no impact is found on completion rates.

JEL: I2, C41, C31, J18

Key words: Financial aid, Higher education, Duration analysis, Policy reform, Treatment on duration

1. Introduction

The purpose of this study is to estimate the impact of public study grants on higher education outcomes in Denmark. The Danish case is interesting in the sense that the Danish educational system is one of the most generous in the world with no tuition fees and large public grants eligible to all enrolled students. In 2004, recipients of public student grants in Denmark received 10 billion DKK, corresponding to e.g. 60% of social insurance transfers, (Danmarks Statistik, 2005). In spite of these tremendous costs, surprisingly little is known of the impact of public study grants on educational progress in Denmark or elsewhere, (Dynarski, 2002).

Moreover, it is of great concern in many Western European countries that students are old when they graduate from higher education. In fact, Danish students are among the oldest in OECD countries when they graduate (OECD, 2003). If the pension age is kept constant, the consequence may be an individual loss of life time income and a social welfare loss due to reduced labour supply and income tax. There are therefore many incentives to study the impact of financial aid on drop-out rates and time-to-completion.

Recovering the impact of public student grants on educational outcomes in Denmark is difficult, because grants are eligible to all and because there is voluntary take-up (receiving grants limits allowed earnings while studying). Actual grant level is therefore likely to be endogenous to completion or drop-out outcomes due to self-selection and omitted variable biases.

To circumvent this selection problem, I use variation in grant levels from a reform of the grant system that occurred in 1988 to examine the impact it has on drop-out from and completion of higher education[1]. The reform among others raised the grant level with up to 57%. I benefit from the use of

1 More specifically we study education outcomes at the first tertiary level corresponding to level 5A according to the

unique Danish administrative longitudinal data sets on educational histories from 1984 to 2003, also including information on parental background and high school GPA.

Using various specifications of treatment effects of the grant reform and estimating both intention-to-treat effect of the reform and using the reform as instrumental variable for grants, I find that the reform seems to have lowered drop-out rates but had no impact on completion rates.

2. The impact of financial aid on education outcomes

Providing financial aid for education can be justified in many ways. One argument is that education boosts economic growth, which, when combined with social externalities, provide incentives for public intervention[2]. Another argument is that there might be barriers to free access to education. It is important to note that the barriers may be non-financial. There is some evidence that credit constraints play a small role in at least US educational attainment (Cameron & Taber, 2000; Carneiro et al., 2003) and in Denmark as well (Nielsen et al., 2006). Another argument could be, as is likely the case in Denmark that the state supports young people's independence from their parents.

A theoretical framework for understanding how financial aid affects individual choice of education is given by human capital models (Becker, 1993). In recent work, theoretical models have incorporated financial aid directly (Cameron & Taber, 2000; Card, 1999). Theory predicts that lower costs of education will ceteris paribus induce more students on the margin to enrol in education[3]. The impact

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ISCED-97 classification, or long-cycle tertiary education (lang videregående uddannelse).

2 See Winter-Ebmer (1994) and Acemoglu and Angrist (2000) for approaches to and problems of estimating external effects of education.

3 Another question is how public grants affect »other things« in the long run, e.g. the returns to education, and whether there a feed-back mechanisms to education policies and demand for education. Such general equilibrium effects are beyond the scope of this project.

on completion is more ambiguous and is discussed below. Lowering the costs of education may also be a second best solution to alleviate other types of barriers, which may be correlated with financial means.

In order to include a phenomenon like drop-out explicitly, several papers have considered the decision to continue schooling from one year to another, as opposed to models treating schooling as a one-shot decision. Such a set-up allows for the impact of consecutively up-dated information e.g. of academic achievement, current wage offers and expected future returns (Bettinger, 2004; Eckstein & Wolpin, 1999; Ehrenberg & Sherman, 1987). The problem of dropping out or when to complete essentially boils down to an optimal stopping problem. Some of these models also highlight that although working while studying may be a general human capital investment (experience), it is also likely that it may prolong the time to graduation, decrease grade point average (GPA) or increase the risk of dropping out. This is considered at length in a Danish context in (Joensen, 2007), where it is found that a limited amount of student employment is favourable to both academic and post-academic performance.

The predictions from theory from an increase in study grants can be summarized as: 1) An income effect though consumption value of education, which if education is a normal good, would increase demand for education. This gives a negative impact on drop-out but the impact on completion time is ambiguous as study time per se may yield consumption value (as opposed to consumption value derived ex post completion from a higher educational level). 2) Investment effects which works through two channels: A direct effect, that a higher student income (or lower net costs) raise the incentive to keep on studying relative to being full-time on the labour market and an indirect effect through a reduction of the incentive to work *while* studying. The latter might increase study efforts and therefore both investment effects should lower drop-out rates. However the impact on completion time is ambiguous. Higher study effort should ceteris paribus increase completion chances and decrease completion time (but may again have a positive consumption value). However, lower costs may by itself prolong

completion time, even *after* prescribed study time, as it becomes less costly to wait for the right job offer or to improve job offers by improving skills.

2.1 Previous empirical evidence

Just to get an intuition of the size of the impact of financial aid I briefly mention how financial aid affects enrolment in higher education. Dynarski reviews findings on the effect of student aid on college enrolment (Dynarski, 2002), mainly paying attention to recent US studies that uses different types of quasi-experiments like changes in aid programmes to circumvent potential selection. Most of the estimated price responses are about a 4% increase in enrolment rates for a 1000$ increase in student aid. This is close to earlier estimates of 5-7% (Leslie & Brinkman, 1987), not based on quasi-experiments. There is sparse evidence on European data. In Germany, (Lauer, 2000) finds that need-based grants (the BAföG) has a significant positive on enrolment, whereas (Baumgartner & Steiner, 2001) come to the opposite conclusion. Finally, (Nielsen et al., 2006) find that a 1000$ increase in public student grants in Denmark would raise enrolment by 1-3%.

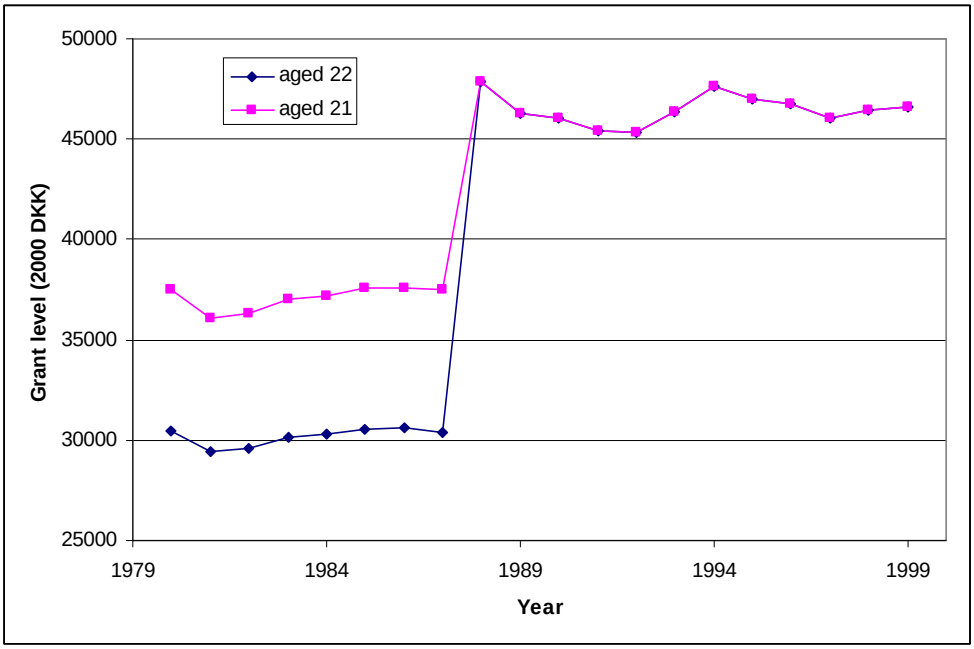
A few number of studies have examined financial determinants of retention and drop-out, accounting for selection and most find the expected negative impact (Bettinger, 2004; Singell, 2004). Dynarski studies the impact of aid on college completion (Dynarski, 2003). Although the estimated impact on years of schooling completed is positive it is insignificant and not robust. Only a modest effect of student aid on duration is found from an education reform in Finland where study grants more than doubled (Häkkinen & Uusitalo, 2003). Using a regression discontinuity design from kinks in the required tuition level determined by family income, (Garibaldi et al., 2006) find a significant effect of raising tuition in the last year of study at Bocconi University in Italy on the risk of late graduation. Another study found that the introduction of tuition fees for prolonged studies at German Universities lowers duration of study time significantly (Heineck et al., 2006).

3. The Danish Grant System

The State Education Grant and Loan Agency which administers the public student grant and loan scheme, was established in 1970. Descriptions of the Danish student grant scheme can be found e.g. in (SU-styrelsen, 1997).

Different types of reforms of the Danish student grant scheme at upper educational levels occurred during the 1980s and 1990s. A major reform of the scheme took place in 1988 (LBK, 1988). The maximum annual grant level for a student who does not live with his parents and is older than 21 increased from 30.418 DKK in 1987/88 to 47.839 DKK in 1988/89 (both in 2000 levels), i.e. an increase of 57% in real terms (SU-styrelsen, 1997; SU-styrelsen, 1999). For students aged below 22, the grant was 37.502 in 1987. There is therefore also cross-sectional variation in the effect of the reform.

Figure 1. Maximum public student grant levels.



Source: SU-styrelsen 1997; 1999. Graph shown is the maximum annual grant level for an unmarried 21 and a 22-year old not living with his or her parents. Deflated by the consumer price index (2000-level).

Figure 1 shows the grant level over time for students not living with their parents and aged below or above 22. As can be seen from the figure, the grant level was almost constant ten years before and after the reform in real terms.

It is noted that the reform altered other aspects of the grant and loan system. The change in the grant level was however the most important and sizeable change. Other changes include changes in public loan levels, that dependence upon parental income of receipt of grants was lowered from age 20 to age 19, and that the maximum amount of earnings allowed for while receiving study grants increased as well. Finally, a maximum grant level during the entire time of study was introduced through a voucher system, where students could manage receipt of their own monthly study grants within a limit corresponding to grants during the prescribed study time plus up to a year spend changing field of study (the first time) or failing exams. Additional study grants could potentially be obtained through individual application.

4. Data

I use an administrative register data constructed by the Local Institute of Government Studies (AKF), obtained from Statistics Denmark which at the time of initiation of this study covered the years 1984-2003. The sample consists of 10% randomly drawn individuals from the Danish population. The educational records include information on whether being enrolled in a given education, highest degree attained (both as per November in a given year) and time of enrolment.

I only consider educations with designated course load equivalent to five years of full-time studies (and where this prescribed time has not changed from 1984 to 2003), which the reform was aimed at. In Denmark, this is the standard length of an university education, following high school. However, I can not follow individuals from enrolment till completion, as I only observe four years prior to the reform

(in 1988). Therefore, to ensure that the sample includes students in a given year of study who experience the reform and a control group of students in the same year of study, who do not experience the reform, I only use education spells of at most four years of length.

Due to these data limitations, I construct two datasets: one sample for the study of drop-outs and one sample for the study of completions. In the first sample, which I denote sample A, I select individuals who graduate from high school and enrol at the first tertiary education level for the first time during the years 1984 to 1990. For each of these years, I follow individuals four years ahead and register their education records. I further restrict this sample to individuals who enrolled in higher education within at most two years of completion of high school (92% of the sample), and who were below 25 of age at enrolment. In this way I get a more homogeneous sample both with respect to previous education history and grant entitlement. The second sample of students, denoted sample B, is limited to students who have been enrolled in higher education for four years in a given year from 1984 to 1990 and they are followed four years ahead, i.e. up to the eight study year. The time from completion of high school to enrolment in higher education is again limited to at most 2 years and age at enrolment to 25 or below.

I define completion when I observe enrolment in one year but not the next and where the highest degree attained increases the following year, and the degree attained corresponds to the education previously enrolled in. A drop-out is defined as a case, where I observe the student being enrolled in one year and not the next without completion. Drop-out excludes students who switch to another study program at a tertiary level, but includes students who enrol in a lower level program and also include students who re-enrol after more than one year.

5. Methods

I use duration models for both time-to-completion and time-to-drop-out. These have also been used in related studies, e.g. (Ehrenberg & Mavros, 1995; Heineck et al., 2006; Häkkinen & Uusitalo, 2003; van Ours & Ridder, 2003). Due to the need for specific samples for drop-out and completion I use single-risk models, as opposed to a perhaps more appropriate competing risks model.

To define the model let T_i be the period of time that individual i is enrolled in education until completion (the model is stated likewise for drop-out) and y_{it} be dummies for whether individual i completes an education in period t. Let h_{it} be the individual completion hazard in period t, i.e. the probability of completing education at t conditional on having studied for t periods:

(1) $$h_{it} = P(T_i = t \mid T_i \geq t)$$

I use a discrete duration data model, as only annual outcomes are observed. Letting t_i be the observed time at which individual i experiences an event, the likelihood can be written in the form of a discrete choice model (Sueyoshi, 1995; Allison, 1982):

(2) $$L = \prod_{i=1}^{n} \prod_{s=1}^{t_i} h_{it_i}{}^{y_{is}} (1 - h_{it_i})^{(1-y_{is})}$$

I choose a logit specification for the discrete hazard. The discrete hazard therefore depends on a baseline period effect, $\bullet_t$, public student grants (GRANT), and other regressors, X, in the following way:

(3) $$\log\left(\frac{h_{it}}{1-h_{it}}\right) = \alpha_t + \delta GRANT + X\Pi$$

5.1 Treatment effects and their identification

As the actual level of grants received is likely to be endogenous to completion or drop-out time, the model as stated in (3) is likely to give biased results.

I use two different sources of variation in grant levels arising from the 1988 reform of the grant system to seek to identify causal parameters: 1) One relying solely on the time-series change in 1988 of the grant level. 2) Another relying on different changes in the maximum grant level for students aged 22 and students younger than 22. I refer to changes in the study grant due to the 1988 reform as a reform treatment.

I explore three different reform treatment specifications using 1): the pure time series change in grant levels. The simplest specification is obtained with a simple dummy for being treated at all (i.e. whether being enrolled after the reform). A second specification is based on period-specific treatments, i.e. separate dummies for being enrolled after the reform in each specific year of study (first, second, etc.). Finally, years of treatment at a given duration of study time is a third alternative, which has been used e.g. in (Ehrenberg & Mavros, 1995). The period-specific formulation is most demanding since it requires that both treated and untreated are observed in the specific time periods, which is ensured in the sample used here. It also means that imposing the first or the third specification, treatment effects are identified among others from a functional form assumption. Given that all students are eligible for the grant, the interpretation of the reform treatment is that it is the impact of being treated relative to no treatment until then, see (Sianesi, 2002) for a related approach in labour economics.

Finally, the second source of variation, 2), gives rise to a differences-in-differences strategy by using the simple dummy treatment specification, adding a dummy for being at least 22 of age in the current year of study as well as their interaction. This specification is more robust than the three former to changes

over time in the outcomes, not being due to the reform. Note however that, as most people are above 22 when they complete their studies, this identification strategy can only be used to estimate the impact of grants on drop-out rates, not on completion rates.

There are two distinct ways to use these specifications. The literature on the impact of financial aid has focused on intention-to-treat effects, see e.g. (Dynarski, 2003), arguing that this is the policy parameter of interest, since policy makers typically do no control take-up of grants. The intention-to-treat specification is obtained by replacing (3) by the model:

$$\log\left(\frac{h_{it}}{1-h_{it}}\right) = \alpha_t + \delta_{ITT} Reform + X\Pi \tag{4}$$

Where *Reform* could be any set of variables associated with the four specifications listed above: simple dummy, period-specific dummy, years of treatment or a diff-in-diff specification.

Alternatively, one can estimate the impact of grant levels using the reform changes as instrumental variables for grant levels. This is done by a standard control function approach, following e.g. (Rivers & Vuong, 1988):

$$\log\left(\frac{h_{it}}{1-h_{it}}\right) = \alpha_t + \delta Grant + \eta e + X\Pi \tag{5}$$

Where e is the residual from a linear regression of *Grant* on X and reform variables. Standard errors have to be corrected in the second stage, and I use a nonparametric bootstrap with 50 replications to do this. If the instruments are valid, these estimates can be interpreted as average treatment effects. That is,

they identify the impact from forcing individuals to receive grants (full compliance) for a random individual[4], therefore separating the question of compliance from that of effectiveness of the given intervention. Furthermore, the IV estimates also differ from the ITT estimates as the latter are estimates of the compound effect of the reform. Since the reform altered other aspects of the grant system, the ITT effect can not be attributed entirely to changes in grant levels.

6. Empirical analysis

The presentation of the analyses is separated in three: first I present ITT effects of the reform treatment on drop-out and completion. Then I present IV effects using the reform treatment specifications as instrumental variables for grants. Finally, I present more descriptive results of the impact of treatment on other outcomes. References to significance levels will be at a five percent level unless otherwise is mentioned.

It should be noted, that this model does not control for selection into higher education, which therefore may bias results, see e.g. (Cameron & Heckman, 1998; Cameron & Heckman, 2001).
Initial estimations controlled for unobserved heterogeneity using discrete random effect models like (Heckman & Singer, 1984) with up to four points of support. The estimates controlling for unobserved heterogeneity are almost identical to those without, so the simpler models are presented.

6.1 Intention-to-treat effects of the student grant reform on drop-out rates

In table 1 I present descriptive statistics for sample A. There are 1543 individuals observed for up to four years, yielding a total of 4763 period-person specific observations. The statistics are for all observations, i.e. including repeated observations per individual. 32, 25, 22 and 20% of these

[4] Note though that if the constant effect assumption implicit in (3) is violated, the estimates identify local average treatment effects (under a monotonicity condition), see (Imbens & Angrist, 1994).

observations are for the first up to respectively the fourth years of study, denoted by the year-of-study dummies, dt1,…, dt4. 51% of the observations are after the reform as denoted by "Treat".

Table 1. Descriptive statistics for sample A (drop-out analysis) and sample B (completion analysis).

	Sample A		Sample B	
Variable	Mean	Std.dev	Mean	Std.dev
Drop-out	0,128	0,334	0,033	0,180
Complete	0,001	0,035	0,254	0,435
Period dummies:				
dt1	0,324	0,468	0,398	0,490
dt2	0,254	0,435	0,284	0,451
dt3	0,221	0,415	0,192	0,394
dt4	0,200	0,400	0,125	0,331
Treatment variables:				
Treat	0,508	0,500	0,558	0,497
Treat*Aged 22	0,318	0,466	0,558	0,497
Aged 22	0,416	0,493	1,000	0,000
Treat 1. year	0,215	0,411	0,311	0,463
Treat 2. year	0,299	0,458	0,334	0,472
Treat 3. year	0,287	0,453	0,248	0,432
Treat 4. year	0,177	0,381	0,125	0,331
Years treated	0,982	1,192	1,023	1,155
Controls:				
Man	0,612	0,487	0,645	0,479
GPA	8,000	1,911	8,000	1,322
Missing GPA	0,043	0,203	0,016	0,127
Parental education	0,397	0,489	-	-
Log(parental income)	5,305	1,306	-	-
Negativ income	0,048	0,214	-	-
Applicants 1. priority	17565	1239	-	-
Capacity in appl. year	13540	934	-	-
Sabbatical years	0,676	0,735	0,690	0,759
Observations	4763		1411	
Individuals	1543		562	

Notes: See text for description of samples. Treat is a dummy for studying after the reform.

"Treat 1. year" refers to treatment in the first year of study. There are 61% men in the sample and 13% of the observations are drop-outs. Less than 1 % of the observations register a completion (i.e. before pre-scribed study time). GPA has been normalized to the mean performance level in all years to correct

for grade inflation[5]. 40% of the student-observations have either a mother or a father with a tertiary education (being short, medium or long-cycle). I observe parental gross income and use the income of the parent with highest income in the year the student was 20 years of age. The mean of log annual income is 5,3 corresponding roughly to 200 thousand DKK, roughly 40 thousand $. I also have information on number of applicants with first priority in the given year and capacity (enrolled plus vacant slots) in the year the student enrolled, as additional controls. The latter are included to roughly account for the supply-side restrictions in the Danish education system[6].

Table 2. ITT effects of the reform on drop-out rates from discrete duration models.

	Simple treatment		Diff-in-Diff		Period-specific		Years treated	
	Estimate	Std.err	Estimate	Std.err	Estimate	Std.err	Estimate	Std.err
Treat	0,0394	0,1661	0,1193	0,1940				
Treat*Aged 22			-0,2557	0,2210				
Aged 22			0,6910	0,1737				
Treat 1. year					0,1424	0,2128		
Treat 2. year					-0,0221	0,1655		
Treat 3. year					-0,6078	0,2311		
Treat 4. year					0,3575	0,4502		
Years treated							-0,2165	0,0999
-2LogL	3277,351		3260,02		3269,583		3272,702	
AIC	3325,351		3312,02		3321,583		3320,702	

Notes: 4763 person-specific observations from sample A, see text. All estimates include baseline dummies for year of study. Further controls include high school GPA (corrected for grade-inflation), log highest parental income, sabbatical years after high school, dummies for missing GPA, negative parental income, one parent having tertiary education and seven dummies for region of study and three dummies for field of study, and number of applicants and capacity in the year enrolled.

Table 2 presents results of estimates of the intention-to-treat parameters from the student grant reform. Four set of results are presented, all with the same set of controls and period specific baseline hazard but with varying treatment specification as discussed in the section on the empirical model. The controls are all the variables in table 1 described under the heading "controls", as well as dummies for

[5] The Danish grade system at during the 1980s and 1990s consists of the grades 0, 3, 5,…, 11, 13, where 0 is for the totally unacceptable, 6 is just passed and 13 is for the extraordinary performance. 8 is mean performance.
[6] They are obtained from KOT, "den Koordinerede Tilmelding", who administrates applications and admissions. Students apply for enrolment in a given education at a given university, making up to three prioritized choices. Enrollment decisions are mainly based on high school GPA, where requirements vary over time and across educations and universities, but also on experience and waiting lists from year to year.

institutional region (municipality of the university) and major of study (humanities, social science, natural science and health sciences). These variables should control for a large part of the variation in drop-out rates due to institutional differences and due to differences in selection arising from differences in ability and educational background which forms expectations and performance that may be driving the decision to receive grants. Baseline dummies and control coefficients are not presented, but are available from the author upon request. The baseline hazard is decreasing almost linearly in all four specifications, except the one with period specific treatment, where it is constant between the second and third year. The impact of the control variables is very alike across all specifications. High school GPA, parental income and sabbatical years have high significant and negative effects on the hazard to drop-out, whereas parental education and number of applicants (supply side restrictions) have no significant impacts. Gender differences are only barely significant, men having a lower drop-out hazard. There are also significant regional and field of study differences.

It is seen from the first set of results, table 2 (1) from the simple treatment specification, that treated have a higher hazard rate to drop-out than non-treated (the variable "treat" has a positive coefficient), but that the coefficient is far from being significant. Turning to the diffs-in-diffs specification, (2), the coefficient of interest is the interaction between treatment and the dummy for studying at age 22 or after. This coefficient is negative, corresponding to a lower risk of drop-out when the grant increases, but again insignificant. Looking at the third specification, it is seen that the impact of the reform differs during the first four years of study, (3), and that there is a large negative effect and significant effect in the third year. The impact is insignificant in the other years. Finally years of treatment also has a large negative and significant coefficient, see (4).

These results seem to suggest that there is a weak negative relationship between grant level and drop-out risk. But what is its magnitude? The years-of-treatment specification yields an estimate of the log-odds

of the hazard of dropping out of -0,217 for an additional year of treatment. This is equivalent to an odds-ratio of 0,80 and roughly corresponds to a 20% lower hazard of dropping out (as the initial probability of drop-out is low). The effect describes the impact of the reform in a given year of study compared to a scenario, where the reform was enforced one year earlier. If the impact is ascribed solely to the change in grant levels, the effects can be interpreted as the impact of an increase of the grant of 17.400 DKK (equal to the reform change for individuals living by themselves, aged more than 21), corresponding to approximately 3500$. Dividing the impact by 3.5, I get a rough estimate that raising grants by 1000$ decreases the drop-out hazard by 5,7%, which curiously enough is not far from the impact on "enrolment hazards" reported in US studies on 4 % (Dynarski, 2002) and a bit on the upper side of Danish results that grants raise the enrolment hazard into all post-high school educations by 1-3 % (Nielsen et al., 2006). Note though that set aside the fact that enrolment and drop-out are two separate outcomes, these results are not comparable, as I focus on drop-out from first level tertiary education, and (Nielsen et al., 2006) include all post-secondary enrolments.

6.2 Intention-to-treat effects of the student grant reform on completion rates

Table 1 also contains descriptive statistics for sample B, which is used for the completion analyses. Recall that the sample is created by sampling students who have been enrolled in the same education for four years and followed through up to the eight year of study. This sample only consists of 562 individuals, with a total of 1411 period-person specific observations. 56% of these observations are after the reform and 40, 28, 19 and 13% are studying in respectively their fifth up to eighth year. There are 65% men in the sample and 25% have completed the education after the seventh year of study. Only 3% drop out. There are very few observations with information on parental education and income in this sample (since this information is only obtained for younger students), so these are not included in the analyses of completion.

In table 3 I present estimates of intention-to-treat parameters from the student grant reform on completion rates. Three sets of results for different reform treatment specifications are presented as the diffs-in-diffs strategy is not feasible, since all students in this sample are aged 22.

The baseline hazard has an inverse U-shape, increasing in sixth and seventh year but decreasing in the eight. The control variables show that the impact of high school GPA is high, positive and significant, whereas gender and sabbatical years are not significant. There are regional and field-of-study significant differences.

Table 3. ITT effects of the reform on completion rates from discrete duration models.

	Simple treatment		Period-specific		Years treated	
	Estimate	Std.err	Estimate	Std.err	Estimate	Std.err
Treat	0,1164	0,1525				
Treat 5. year			0,1864	0,1635		
Treat 6. year			0,0490	0,2021		
Treat 7. year			0,2056	0,3188		
Years treated					0,1310	0,0801
-2LogL	1432,578		1430,088		1430,472	
AIC	1470,578		1472,088		1468,472	

Notes: 1411 person-specific observations from sample B, see text. All estimates include baseline dummies for year of study. Further controls include high school GPA (corrected for grade-inflation), sabbatical years after high school, dummies for missing GPA, seven dummies for region of study and three dummies for field of study.

From table 3, it is seen that all specifications predict that the grant reform has a positive impact on the hazard to completion. However, all effects are insignificant on a five percent level whereas the impact of years of treatment is barely significant on a ten percent level. The impact for one additional year of treatment on logg-odds of completion is 0,13.

6.3 Instrumental variable estimates of the impact of grants on drop-out and completion

Table 4 contains results from instrumental variable estimates of received annual grant levels from discrete duration models for drop-out using sample A, using reform specifications as instrumental variables.

First, treating study grants as exogenous, the first row shows that the log-odds of the hazard of dropping out is 0,16 lower for individuals with a grant level that is 10.000 DKK higher in a given year, when controlling for the same variables as in the intention-to-treat models, including high school GPA and parental education and income. The difference is highly significant. This corresponds to an 8% lower hazard of dropping out when grants are raised by approximately 1000 US$. This estimate is more than a third larger than the intention-to-treat estimate. The next four pairs of results show the impact of grant levels using the four reform treatment specifications from table 2 as instrumental variables for study grants. The first stage results are presented at the bottom of the table. They show that with one exception all treatment specifications are highly significant in a linear regression on actual grants received. The exception is the diffs-in-diffs specification, which is therefore disregarded in the following. As the dependent variable in the first stage regressions, grant level, is expressed in 10.000 DKK, the simple dummy formulation suggests that the reform raised study grants by 16.116 DKK on average in a given year. Looking at the upper part of the table again, controlling for endogeneity gives different results depending upon the treatment specification. Very high and significant effects of grant size on drop-out hazards are found using the period-specific treatments and years of treatment while the simple treatment gives an insignificant and lower effect. However, as indicated by the coefficient on the residual from the first stage, exogeneity of grants can not be rejected in any of the specifications.

Table 4. Student grant effects on drop-out rates from discrete duration models.

	Discrete logit-duration		Discrete IV-logit-duration							
Instrument specification:			Simple treatment		Diff-in-diff		Period-specific		Years treated	
Dependent: Drop-out	Estimate	Std.err	Estimate	Std.err	Estimate	Std.err	Estimate	Std.err	Estimate	Std.err
GRANT/10^4	-0,163	0,034	-0,109	0,067	-0,063	0,066	-0,222	0,087	-0,220	0,087
RESIDUAL/10^4			-0,069	0,075	-0,130	0,074	0,072	0,097	0,070	0,097
-2LogL	3262,1		3261,2		3259,0		3261,5		3261,6	
First stage:										
Treat			1,6116	0,0428	1,570	0,051				
Treat*Aged 22					0,052	0,084				
Aged 22					0,345	0,072				
Treat 1. year							0,514	0,050		
Treat 2. year							0,865	0,056		
Treat 3. year							0,821	0,072		
Treat 4. year							0,415	0,134		
Years treated									0,704	0,020

Note: All estimates include the same controls as in table 2. The first set of rows present the impact of grant levels using simple logit and four sets of IV-logit estimates. In the latter different specifications of reform treatments are used as instrumental variables for grants and first stage OLS results are presented at the bottom of the table.

Table 5. Student grant effects on completion rates from discrete duration models

	Discrete logit-duration		Discrete IV-logit-duration					
Instrument specification:			Simple treatment		Period-specific		Years treated	
Dependent: Completion	Estimate	Std.err	Estimate	Std.err	Estimate	Std.err	Estimate	Std.err
GRANT/10^4	0,168	0,045	0,139	0,196	0,293	0,175	0,291	0,182
RESIDUAL/10^4			0,030	0,202	-0,134	0,182	-0,131	0,188
-2LogL	1418,8		1418,7		1418,2		1418,3	
First stage:								
Treat			0,785	0,091				
Treat 5. year					0,608	0,098		
Treat 6. year					0,343	0,125		
Treat 7. year					0,235	0,186		
Years treated							0,441	0,047

Note: All estimates include the same controls as in table 3, see also table 4.

In a similar manner table 5 presents estimates of the impact of grant levels on completion rates using sample B. It shows that when treating grants as exogenous, the log-odds of the hazard of completion is significant and 0,17 higher for individuals who receive 10,000 more in study grants in a given year.

Three sets of instrumental variables results are presented in the next set of columns, one for each treatment specification possible in sample B. The lower part of the table shows that the period-specific treatment effects are not all significant in the first stage regressions on grants. For the two other specifications, the reform significantly raise grant levels also in later years of study. The impact of grants is just slightly lower in the IV estimation using the simple dummy treatment specification than in the model treating grants as exogenous, while almost double in size in the IV estimation using years-of-treatment or period-specific specifications. Both are insignificant though and it can not be rejected that student grants are exogenous, so results remain inconclusive.

6.4 Additional results

Finally, to gain some insight into the potential mechanisms through which the impact of grants might work, I have estimated a number of models of the impact of the reform on intermediate outcomes: whether the reform induce a higher take-up rate of grants, lowered work hours and labour earnings while studying, as well as the impact on start age and high school GPA for the enrolled. These are presented in table 6 for sample A and for the three former outcomes in table 7 for sample B.

To keep things simple I have estimated logit or least squares models for individuals in a given year of study using the simple treatment specification only, but including all the controls from the drop-out and completion estimations.

Table 6. Impact of simple dummy reform treatment on various outcomes in sample A.

Sample:		1. year		2. year		3. year		4. year	
Dependent variable		Estimate	Std.err	Estimate	Std.err	Estimate	Std.err	Estimate	Std.err
GRANT>0		1,526	0,301	0,706	0,326	1,689	0,498	0,954	0,356
	-2LogL	1535,16		779,819		626,52		533,206	
	N	1545		1210		1054		954	
WORK HOURS (ATP)		-0,088	0,021	-0,073	0,013	-0,091	0,018	-0,095	0,016
	Adj. R2	0,253		0,128		0,197		0,187	
	N	1545		1210		1054		954	
LN(EARN)		-0,118	0,114	-0,300	0,089	-0,189	0,126	-0,135	0,123
	Adj. R2	0,195		0,112		0,091		0,090	
	N	1344		1028		878		803	
START AGE		0,405	0,063						
	Adj. R2	0,169							
	N	1545							
GPA		-0,249	0,123						
	Adj. R2	0,0391							
	N	1545							

Notes: Simple logit and OLS estimates in samples of students in their first, second, third and fourth year of study.
All estimates include the same controls as in table 2.

While the first stage results presented above indicated that the reform raised average grants received, it did not indicate whether the reform induced more people to take-up of study grants. This is explored in the first row of table 6, which contains results where the dependent variable is a dummy for a positive grant level. The logit results suggest that the reform induced more students to receive grants in all the first four years of study and the effects are highly significant.

The second and third set of results shows the impact on work hours and labour earnings while studying. Work hours are measured through compulsory payments to a supplementary labour market pension, which depend upon hours of work. I have corrected the payments across years to account for changes in the fixed rates over time. The rates change discretely with hours of work, so it is only a rough proxy. Nothing is paid for work less than 9 hours per week, so I am probably missing important information for some student work. One may worry that these measures are biased, if students drop-out in a year and then start working. However, as ATP and earnings are measured in a given calendar year and the educational record is recorded in November in the same year, this problem should be minor.

It is seen from table 6, that work hours seem to be reduced by the reform in all study years, consistent with the larger take-up and reception of study grants. However, for earnings, although all estimates have the expected negative sign, only the impact in the second year of study is significant. Finally, the grant reform seems to have raised start age and lowered the mean GPA from high school for those attending the first year of study. The latter results is consistent with expectations, if mainly lower ability students find it costly to attend higher education and the reform induce more of these individuals to enrol. The higher start age after the reform may arise if young students, graduating recently from high school, do not compare the new system to the old, whereas individuals who graduated some years ago, but did not find it beneficial to enrol in higher education at the time, find the new system relatively more attractive compared to the old system.

Table 7. Impact of simple dummy reform treatment on various outcomes in sample B.

		5. year		6. year		7. year	
		Estimate	Std.err	Estimate	Std.err	Estimate	Std.err
GRANT>0		0,540	0,248	0,317	0,248	0,454	0,342
	-2LogL	517,387		407,277		290,353[a]	
	N	562		401		271	
WORK HOURS (ATP)		-0,078	0,021	-0,088	0,024	-0,158	0,035
	Adj. R2	0,1653		0,1533		0,229	
	N	562		401		271	
LN(EARN)		0,144	0,096	0,022	0,112	-0,020	0,163
	Adj. R2	0,1257		0,1427		0,2286	
	N	498		348		233	

Notes: Simple logit and OLS estimates in samples of students in their fifth, sixth and seventh year of study. All estimates include the same controls as in table 3.

Using sample B, the reform only has a significant (positive) effect on take-up rates in the fifth year of study but not in succeeding years. Hours of work seem to be significantly reduced by the reform, but again no impact is found on earnings.

7. Discussion

The purpose of this study is to add to the literature studying the impact of financial aid upon educational outcomes. The paper contributes to the literature in several ways. First of all, it contributes to the relatively small literature focusing on drop-out and completion, as opposed to the larger literature on enrolment. Furthermore, the impact of a large-scale reform of the Danish grant system occurring in 1988 is used to identify the impact of public student grants. Only recently have studies sought to identify exogenous variation in financial aid to estimate the impact on education outcomes. Moreover, the study is conducted on highly reliable and accurate register data of educational histories in real time with information on important background variables such as high school GPA and parental income. As a caveat, the study provides a discussion of estimation of time-varying treatments on duration

outcomes, as well as comparison of different treatment specifications and provides both intention-to-treat and instrumental variable estimates.

Using intention-to-treat specifications, I find that the large-scale reform has no significant impact on completion rates. However, as discussed below, I cannot rule out that this is due to an imperfect sample design for the study of this question. As opposed to these results, the evidence on the impact on drop-out rates is more mixed. Four treatment specifications are used and three of them indicate lower drop-out rates after the reform. However, when applying simple treatment or the diffs-in-diffs specification no significant impact of the reform is found, whereas applying a years of treatment or specific year-of-study impacts, significant effects are found. For the significant estimates, the impact of the reform is to reduce drop-out by around 20% per year. Ascribing the effect entirely to the change in grant level, which is probably over-doing it a bit since the reform included other changes, yields the impact that raising study grants by 1000$ lower drop-out rates by roughly 5,7%.

As opposed to intention-to-treat estimates, which masks take-up with causal treatment effects but may be policy relevant when take-up is not controlled by politicians, instrumental variable estimates identify (under conditions described above) causal impacts given full take-up. I use the reform variables as instruments for grant levels in completion and drop-out duration models. The reform has a large significant impact on grant levels. However, the results again vary according to the specification of the reform variables. For drop-out rates I find large (negative) significant effects of the grant level on the drop-out hazard using year-of-study specific and years-of-treatment specification. The impact of grant levels is much larger than simple estimate assuming exogeneity of grant levels. For completion rates, I again find a large positive but insignificant impact.

Although it should be noted that exogeneity can not be rejected, the larger effect on drop-out can be ascribed to a problem of invalid instruments or that the effect identified is a local average treatment effect due to heterogeneous responses. If the latter is the case, it may suggest that the reform has been well structured, with an impact for those who needed it.

The problem of invalid instruments can arise if one of the two instrumental variable conditions are not fulfilled. The first condition is that the instrument has a high partial explanatory power of grant levels. This is the case as F-values are larger than 100 for most specifications. The second instrumental variable condition states that the instrument is independent to individual completion and drop-out hazards conditional on other controls. This is violated if substantial changes occur at the same time as the grant reform, that is, if the impact on drop-out and completion hazards of factors not accounted for, are drastically different for the cohorts entering higher education before and after the reform. This will also affect the interpretation of the intention-to-treat estimates. The most obvious factors not accounted for are general labour market conditions, such as unemployment risk and wages differentials among education groups in general and specifically for new graduates. The relative differences in unemployment rates among education groups have been rather stable in the 1980s, see e.g. (Groes et al., 2004). The same can be said about the ratio of youth and adult unemployment rates and for differences in lifetime income and annual income among education groups. Therefore, there is no immediate indication that important labour market factors should account for the reform impact in 1988. In addition, the period-specific treatment or the diff-in-diff specifications are more robust to such criticism. It is also noted that for the simple reform treatment specification, controlling for calendar year effects does not change the results.

The study has limitations though. One limitation seems to be that I only observe enrolment four years prior to the reform, although it is partly circumvented by use of sample B, conditioning on enrolment

for at least four years. Another limitation is exactly that I condition on enrolment in higher education (and survival for four years in the second sample). Controlling for high school GPA and parental background might though account for part of these selection effects.

8. Conclusion

This study has presented evidence on the impact of grant levels on drop-out from and completion of higher education, based on quasi-experimental evidence arising from national policy reforms. The evidence overall supports (although with some uncertainty) the view that a better financial situation for students decrease drop-out rates, but also suggest that it has no impact on completion after four-to eight years of study. Although the impact of student grants on completion is theoretically ambiguous, one may also explain these conflicting results by the finding that the reform induced students with a lower GPA (and therefore ceteris paribus likely a longer completion time) to enrol. Note also, that it can not be ruled out that *more* students will eventually complete. Finally, the results indicate that the lower drop-out rate partially may be explained by a reduction of work hours while studying, as intended by the reform, although lower hours of work is not accompanied by higher completion rates.

Acknowledgements

This research was supported by the Social Science Research Foundation, grant 24-04-0252, for which we are grateful. We thank the Institute of Local Government Studies (AKF) for providing access to the data. In particular, we thank Eskil Heinesen and Helena Skyt Nielsen for valuable comments and suggestions. We also thank participants at the Health Economics Seminar, SDU for comments. Thanks to Den Koordinerede Tilmelding for data on number applicants in the 1980s.

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