

Estimating the Effect of Student Aid on College Enrollment: Evidence from a Government Grant Policy Reform*

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Abstract

In this paper, we investigate the responsiveness of the demand for college to changes in student aid arising from a Danish reform. We separately identify the effect of aid from that of other observed and unobserved variables such as parental income. We exploit the combination of a kinked aid scheme and a reform of the student aid scheme to identify the effect of direct costs on college enrollment. To allow for heterogeneous responses due to borrowing constraints, we use detailed information on parents' assets. We find that enrollment is less responsive than found in other studies and that the presence of borrowing constraints is at most borderline significant. Based on a simple structural model, borrowing constraints deter college enrollment only to a minor extent. This is, however, in large due to subsidies already being substantial.

Keywords: college attendance, educational subsidies, reform, kink regression.

JEL classification: I22, J24.

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1 Introduction

An increase in educational subsidies is expected to increase college enrollment. Despite that substantial educational subsidies are already in place, previous empirical work suggests the elasticity is large. Empirical studies for the U.S. find that the magnitude of the effect is such that a \$1000 increase in the annual educational subsidy increases enrollment by roughly 3-5 percentage points (see e.g. Dynarski, 2003; Leslie and Brinkman, 1988; Manski and Wise, 1983; McPherson and Shapiro, 1991). The purpose of this paper is to investigate the responsiveness of the demand for college to changes in student aid by exploiting some useful exogenous variation in Danish data.

The crucial issue in studies of the effect of financial aid on enrollment is to separately identify the effect of aid from that of other observed and unobserved variables such as parental background. First, student aid is most often means tested against parental income and other socioeconomic variables. Second, responsiveness of demand for college to changes in student aid is likely to vary with parental background for many reasons. In particular, one might expect college enrollment to be particularly responsive to educational subsidies among families who are borrowing constrained (see e.g. Cameron and Taber, 2004). In this paper we exploit the combination of a kinked aid scheme and a reform of the student aid scheme to identify the effect of direct costs on college enrollment accounting for borrowing constraints.

We exploit two data characteristics to identify the effect of educational subsidy separately from the effect of parental income. First, we use the nonlinear relationship between the income of the parents and the amount of aid for which students are eligible. Second, we use information about an extensive reform of the educational grant system which was implemented for the cohort starting at university September 1st, 1988. We show that, theoretically, the first characteristic is sufficient to identify the effect of interest, although, we find that the variation caused by the reform is necessary for identification in practice.

The reform consisted of two major changes: it reduced the age limit above which

you could receive grants independently of parental income and – most importantly - it raised the levels of grants by more than 25% for all students above 19 years of age. The exact amount and relative change in aid varied a lot depending on the income of the parents. After the reform, educational subsidies universally covered almost all students throughout their college education at a generous level.¹

Children with poor parents (henceforth: poor kids) are often found to be more responsive to educational subsidies than the children of rich parents (henceforth: rich kids). This finding may work through (at least) three different channels. (1) Poor kids are likely to be borrowing constrained, therefore, they would be more affected by an increase in subsidies than rich kids. (2) Poor kids may receive lower schooling contingent transfers from their parents. Public transfers likely crowd out some schooling contingent transfers from parents. That would lead to a lower college enrollment response of rich kids to a reduction in public transfers than that for poor kids. (3) Poor kids receive less non-labor income over their life time than the rich kids. That may be due to lower non-contingent transfers from their family when they are young or due to less bequests. This implies that the marginal utility of income is higher for poor kids than for rich kids who have higher non-labor income. As a result, they are more sensitive to a reduction in costs. As is common in most of the literature, we implicitly assume that children and their parents form a dynasty with common interests, and hence we are not able to separately identify the effect of (2). Our efforts are focused on accounting appropriately for channel (1), namely, the effect of borrowing constraints.

We adopt the low liquid asset measure, which was suggested by Zeldes (1989) and successfully applied by Leth-Petersen (2004), to control for borrowing constraints. They argue that the ratio of liquid assets to income is a powerful way to identify the households who potentially face a binding credit constraint.

We use Danish register-based data for the cohorts graduating from high school in 1985 to 1990. In this way we have data both before and after the reform. We rank individuals according to the index of parental income which determines eligibility for

¹We use the terms grant, stipend and student aid interchangeably.

student aid, and match post-reform individuals with pre-reform individuals at the same place in the income distribution.² The fact that the relationship between this measure of parental income - and hence the rank in the distribution of parental income - and the stipend is nonlinear helps us to identify the relationship of interest. We show formally that the relationship between family income and college enrollment should jump at the kink point which allows us to identify the effect of the reform. Basically, we analyze whether the family income/college enrollment relationship changed systematically differently around the kink points.

We find that the subsidy did increase college enrollment, but at a lower rate than in previous work, which is mainly for the US. The point estimates indicate that a \$1,000 increase in the yearly stipend increases college enrollment by 1-3 percentage points. The confidence band rules out estimates as large as found in the previous literature. This is partly caused by the fact that total subsidies were and still are larger in Denmark than it has ever been in the US. To analyze whether the small effect is also partly due to the fact that part of the students are borrowing constrained whereas others are not, we introduce the low liquid asset indicator. We find some evidence that borrowing constraints deter enrollment. The reported results are conditional on an assumption that the supply is completely flexible. If a supply constraint has been binding, a demand increase should show up in 'prices' which (in the Danish case) would be entry requirements in terms of GPA. We argue that the demand response did not affect prices significantly.

To gain further insight and improve the interpretation of the reduced form results, we set up and estimate a simple structural model. Simulating various policy counterfactuals the subsidy level is found to have an important effect on college enrollment. Borrowing constraints do not appear to be important in Denmark at this time, but this is in large due to the substantial subsidy already in place.

The remainder of this paper is organized as follows: Section 2 surveys the literature.

²This avoids making restrictive untestable assumptions about income growth for the parents over the six year period. We are implicitly making the assumption that (counterfactual) eligibility for student aid for the post-reform individuals had they graduated from high school before the reform, is determined by their place in the income distribution.

Section 3 describes the reform while section 4 documents the data. Section 5 presents the empirical analysis and section 6 concludes the paper.

2 Existing Literature

The literature attempting to identify the effect of costs on college attendance is long and diverse. In an older review of the literature, Leslie and Brinkman (1988) conclude that the estimated effect on college attendance of a \$1000 dollar aid increase ranges from three to five percentage points. The results from the earlier studies are, however, likely to be flawed by poor identification, as aid is correlated with numerous observable and unobservable variables.

Comparing enrollment rates across states, Kane (1995) finds that a \$1000 dollar difference in costs of public 2-year college is associated with a 19-29% difference in enrollment rates (8-16 percentage points) among 18-19 year-olds. Conditioning on 2-year tuition, the effect of tuition in public 4-year college is positive, although insignificant, indicating that the marginal price determining whether or not people attend college at all is public 2-year college tuition and not 4-year college tuition. The major weakness of this cross-state comparison is that identification is based upon differences between states which have been fairly stable over time, making it difficult to separate the effect of tuition from other fixed between-state differences. Exploiting within-state differences in public tuition increases since 1980, the same study estimates a 3.5 percentage point drop in public undergraduate enrollment following a \$1000 dollar increase in public 2-year tuition. Furthermore, the gap in enrollment between high- and low-income youth grew the most in the states with the largest tuition increases, hinting at the presence of borrowing constraints. Surprisingly, the study finds no significant effect of implementing the federal means-tested Pell Grant programme in the mid-seventies, leading to the hypothesis of supply constraints muting the effect of the supposedly increased college demand on actual enrollment. This finding is consistent with the influential study of Hansen (1983). Later, Turner (1998) rationalized this missing effect by large aid budget

institutions “undoing” the targeting of the federal grants.

McPherson and Schapiro (1991) exploit the availability of 11 years of observations and find a significant effect of changes of the Pell Grant. A \$1000 dollar increase in net costs (1978-1979 dollars) is estimated to reduce the enrollment of low-income students by 6.8 percentage points.

Exploiting annual micro-data from the CPS, Seftor and Turner (2002) examine the effect of changes in the Pell Grant programme on mature students, and find this group to be more responsive to changes than traditional college-goers. More severe borrowing constraints for the mature students might justify this.

The elimination of the Social Security Student Benefit Program, which subsidized students of deceased, disabled, or retired parents, provides Dynarski (2003) with a source of exogenous variation in schooling costs. The effect of a \$1000 dollar subsidy on the enrollment probability is estimated to be 3.6 percentage points using a “difference-in-differences” identification strategy.

The previously mentioned results mainly apply to students from low-income families, in that the youth subsidized by the Pell programme and to a large extent also those subsidized by the Social Security Student Benefit Program comes from low-income families. Dynarski (2000) studies the Georgia HOPE programme, which mainly affected middle- and upper-income students because any federal (means-tested) grants were deducted from the HOPE stipends. This programme allowed free attendance at Georgia’s public colleges for state residents with at least a B average in high school. Using out-of-state as well as in-state control groups, Dynarski estimates that a \$1000 dollar subsidy increase raise enrollment rates of middle- and upper-income students by 4-6 percentage points.

When focus shifts to the relationship between a single educational institution’s grant and tuition scheme and enrollment into the institution, the results are mixed. Linsenmeir, Rosen and Rouse (2006) find a small, insignificant effect on low-income youth of converting subsidized loans into pure grants, although there might be a significant effect on minorities. Shin and Milton (2006) argue that the observed empirical anom-

ally that tuition and enrollment have risen together over a time span is due to omitted variables. They perform their analysis at the college level, including competitive institutions tuition level and the wage premium as control variables, and find the enrollment changes during 1998-2002 to be unaffected by tuition and financial aid. Van der Klaauw (2002) exploits discontinuities in the relation between a university's financial aid offer and a continuous ability measure. Based on the regression discontinuities, Van der Klaauw reveals an enrollment elasticity of 8.6% for applicants who also filed for federal aid, whereas the elasticity for non-filers is estimated to be 1.3%. This difference in responsiveness is taken as an indication of borrowing constraints.

In a European context, Baumgartner and Steiner (2006) implement a difference-in-differences estimator in a discrete-time hazard rate model to evaluate the effect of the German student aid reform of 2001, which both increased the amount of subsidy received and the coverage of the programme. Based on a fairly small sample, they find small and insignificant effects.

Changes of educational policy often affect a significant part of the population. Therefore, it is crucial to consider the supply side of the sector and not only the demand side, and in addition, general equilibrium effects might flaw the results derived from a partial analysis. Supply side considerations are rarely discussed explicitly in the studies, although e.g. Turner (1998) and Kane (1995) point to supply side effects as causing otherwise non-intuitive results. General equilibrium effects are analyzed by Heckman, Lochner and Taber (1998), who find that taking into account the tax financing of the college grant and the effect on relative wages, may reduce the effect of tuition on enrollment by a factor 10.

Our contribution is twofold. First, since the reform of the student aid scheme is universal we apply a identification strategy which does not rely on the existence of a classic control group not exposed to policy changes as most of the previous studies do. Second, we introduce a new approach to deal with heterogeneous responses due to credit constraints. We exploit detailed information on the asset portfolios of the parents to directly identify liquidity constraints among potential students.

3 The Reform

To identify the effect of educational subsidies on enrollment into college, we exploit the reform of the Danish Government Grant Policy which took place in 1988. In Denmark, three types of college education are available: short-cycle, medium-cycle and long-cycle higher education programmes. Table 1 gives a brief overview of the three. In this paper, we focus on enrollment into any college education, although we do also perform some estimations that distinguish the three different cycles of education. The reform may of course also have influenced, for instance, consumption during studies, timing of entry, duration to completion and the extent of work while studying. In this paper, we focus on the effect on college enrollment.

All colleges are public institutions and free of charge. Student grants and loans are universal in the sense that they are given to all students admitted to recognized educational institutions independently of their qualifications. Grants are means-tested for students below a certain age limit, whereas students above the age limit receive grants independently of their parents' income. For the present research project, we have access to the means-testing algorithm, and the exact income measures needed to check for eligibility.

Before the reform the subsidy was means-tested based on the following index, X , for all individuals below a certain age limit:

$$\begin{aligned} X = & \text{Mother's Income} + \text{Father's Income} \\ & - a \times \text{Number of Siblings} + f(\text{Parent's Wealth}), \end{aligned} \tag{1}$$

where Income is taxable income, Number of Siblings denotes the number of siblings below the age limit who are undertaking education, a is annually adjusted to account for inflation, and $f()$ is a nonlinear function of parents' wealth which also varies over time. If parents are divorced, we assume that the individual is associated with the parent with the lowest taxable income. Until 1987, the subsidy was means-tested for

individuals below the age limit 22. That is, before age 22 the amount of the subsidy depended on the index X , but all students age 22 and higher were eligible for the full subsidy. As a first step before the major reform of the student aid system, the subsidy was means-tested only for individuals below the age limit 20 in 1987.³

The actual reform was announced in 1987 and it was implemented for the cohort starting at college September 1st, 1988.⁴ After the reform, educational subsidies universally covered almost all students throughout their college education. The level of economic support was high enough to suffice for living. The reform consisted of two major changes: First, it reduced the age limit above which you could receive grants independently of X to 19 years. This meant that only the very few students who were born after August 1st and followed the fastest possible way through the educational system were means-tested for the first one or two quarters of their studies.⁵ Second, and most importantly, the reform raised the levels of grants by more than 25% for all students above 19 years of age. The increase was largest for those who were not eligible for stipend before the reform. Those with the largest parental incomes - and therefore the largest values of X - went from no grant at all (as long as they were under the age of 22) to 48,968 DDK per year (2001 prices), which roughly compares to \$8,000 per year.

In Figure 1, we sketch the influence of the reform on the aid scheme. The main content of the reform was to universally increase the level of the stipend. The lower line (beginning at s_1) represents the initial stipend while the higher (at level s_2) represents the post reform stipend. One can see that prior to the reform individuals with $X < x_1$ were eligible for the maximum benefit of s_1 . This benefit started to phase out at level

³The 1987 change was first negotiated in February 1986 and it was passed in June 1986 in due time to influence the decisions of the 1986 cohort.

⁴According to the Parliament's yearbooks the law was first proposed in Parliament on 18/11-1986, whereas it was finally agreed upon on 23/4-1987 (see the Parliament's yearbook 1985-1986 and the Parliament's yearbook 1986-1987). Hence the 1987-cohort of high school graduates were the first ones to know about the reform when they made their career decisions.

⁵We exclude individuals who do not turn 19 in the year they graduate from high school. This group is small and they are likely to have a systematically different behaviour.

x_1 until level x_2 . Students from families with $X > x_2$ received no stipend. After the reform everyone was eligible for the higher level. As a result, one can see that the net effect of the reform was larger for individuals for whom the pre-reform means-test was binding (i.e. $X > x_1$) but that the reform affected everyone.

4 Data

4.1 Data source

We use a register-based data set covering 10% of the Danish population in the period 1984-2004. To this data set, we add information about educational event histories of individuals enrolled at educational institutions in the period 1973-2004. For the main part of the empirical analysis, we select a subsample consisting of high school students graduating in 1985-1990. We use only individuals who graduate at “normal” ages, which we define as 19-20 years. This data set contains information about the individuals, their parental background and grade point average from high school (GPA).

We augment the data with a prediction of the amount of grant which each individual would be eligible for, if they entered college immediately after high school graduation. We apply the algorithm which the authorities have used to compute grants for the students (see Section 3).

In order to account for potential borrowing constraints, we add information about the parents’ liquid assets: the amount of assets held in cash, stocks, bonds, mortgage deeds and other assets.

4.2 Data description

In Table 2 we present enrollment rates at college for each of the high school graduation cohorts 1985-1990 by year. One can see that delaying college enrollment in Denmark is common. The table illustrates that roughly one half of the individuals who enroll within a five year period do that within the first year after graduation (32.6% out of 64.1% for

all cohorts). In the empirical analysis, we focus on accumulated enrollment one year after high school graduation so that pre-reform cohorts' enrollment decision occur prior to the announcement of the reform. For the same reason, we disregard the 1986 and 1987 cohorts in the analysis. The reform of 1988 was announced in time for cohort 1987 to adjust their behavior, and it was preceded by a change in the age of eligibility for full grant independently of parental income (or more precisely: independently of the variable X which was defined above) which was announced in time for cohort 1986 to adjust their behavior.

Table 2 indicates that the reform of 1988 has influenced enrollment since enrollment of cohorts graduating in years 1988 and 1990 were systematically higher one year after high school graduation and onwards. However, the level was larger in 1990 than earlier on.

In Table 3, we present summary statistics for the micro data set by graduation year. We exclude the 7-8% of individuals for whom information about the parents is unavailable. They are no different than the rest of the sample in terms of high school graduation age and college enrollment rates. Table 3 shows that the average high school graduation age is stable around 19.4 years. Average GPA is unobserved for 20–30% of the sample, and therefore we create an indicator for missing GPA and include that in the estimations. If observed, the average GPA is just above 8 and slightly increasing over the cohorts, which probably indicates grade inflation.⁶

In Table 3, we report two low liquid asset indicators, D1 and D2. Liquid assets include all non-housing assets⁷, that is cash, shares, bonds, mortgage deeds, shares in ships and other assets.⁸ The basic indicator, D1, takes the value one if parents' liquid assets falls short of one months' income, whereas the extreme indicator, D2, takes

⁶The Danish grade scale is as follows: 0, 3, 5, 6, 7, 8, 9, 10, 11, and 13. The grades 6 and above are passed, and a medium performance is graded 8.

⁷Until a credit market reform in 1992, it was not possible to use the proceeds from mortgage loans for other purposes than financing real property.

⁸For shares in ships we only have access to the net value of assets placed in shares in ships, that is the value of assets minus liabilities. We use this net value as a replacement for the value of liquid assets.

the value one if the parents' liquid assets falls short of two months' income. Roughly 50% of the sample have liquid assets below one months income, and roughly 60% have liquid assets below two months income. Those are the parents we regard as potentially borrowing constrained. However, for parents who are self-employed, the amount of liquid assets is not registered, therefore, we separately account for this in the empirical analysis by two alternative approaches: Whenever we use the low liquid asset indicator, we include an indicator for whether one's parents were self-employed in the year before graduation or we exclude the self-employed from the analysis.

In Table 4, we present the average composition of the parents' portfolio by high school graduation cohort. Cash dominates the portfolio and takes up roughly half of the portfolio in most years, whereas other assets (such as yachts, cars, campers and other taxable assets) is the second largest element. In Table 5, we present the average composition of the parents' portfolio by the two low liquid asset indicators. It is seen that the potentially borrowing constrained individuals - with $D1=1$ or $D2=1$ - hold a much higher proportion of their wealth in cash. The parents who have liquid assets of less than one months' income, and thereby falls short of the basic split, hold as much as 84% of their wealth in cash. The parents who have liquid assets of less than two months' income, and thereby falls short of the extreme split, hold 74% of their wealth in cash. The least constrained group with $D2=0$ hold only 30% in cash, 36% in other assets and roughly 11% in mortgage deeds, 12% in bonds and 12% in shares. It seems reasonable to us that a group with this portfolio composition would not be borrowing constrained.

5 Empirical Results

To exploit the variation provided by the reform, we need to predict whether individuals who are observed post-reform would be eligible for student aid had they graduated pre-reform. To avoid restrictive untestable assumptions about income growth for the parents over the six year period, we make the assumption that (counterfactual) eligibility for

student aid for the post-reform individuals had they graduated from high school before the reform, is determined by their place in the income distribution. Therefore, we rank individuals according to the measure of parental income, X , which determines eligibility for student aid, and predict the amount of aid an individual would have been eligible for before the reform by assuming that his parents would have been at the same point in the income distribution at that time. Thus, we are able to predict the magnitude of the “treatment” provided by the reform for each individual. In a sense we are just controlling for the distribution of income and not allowing it to determine the effect of the reform. Using this approach, we implicitly assume that the placement in the income distribution is unaffected by the reform.

Specifically we define the variable, S_i to be the proportion of the maximum stipend for which the individual would be eligible in 1985⁹. Thus, for an individual from a pre-reform cohort, S_i is the stipend that individual would have received if he or she had gone to college. That is, we have the same administrative data used to determine the subsidy so we know this variable exactly. For an individual post reform, we use the following procedure:

1. Calculate X_i
2. Determine the quantile of X_i for the current cohort
3. Obtain the corresponding quantile of X for the pre-reform cohort, call it x_i^{pre} .
4. Calculate S_i as the subsidy corresponding to $X = x_i^{pre}$ using the 1985 rule.

Since it is defined as the proportion of the maximum stipend it varies from zero to one. This is the key variable in our analysis.

⁹We take one year of grant as our measure of stipend. Due to the age limit, the total stipend received during college is in general not just a scaling of the one-year stipend. Hence, the planning horizon of the individual could be of relevance. In our present analysis, however, we treat the different lengths of education alike, so there are no natural measure for total stipend during education. We are going to look into this issue and in general distinguish between the different educational types and lengths.

5.1 Standard difference-in-difference estimation

The design of the grant scheme and the reform of the grant system provides us with some different sources of variation as is clear from Figure 1. Before the reform, the grant varied within each cohort across X . However, this variation is clearly not exogenous as the level of grant was tightly linked to the financial situation of the parents, which for many reasons are likely to be correlated with the individuals educational behavior. The reform of the grant system is, however, exogenous in the sense that those graduating from high school before the reform most likely are very similar to those graduating after the reform. The reform provides variation over time, and since we are going to exclude the two high school graduation cohorts to avoid announcement effects, we end up with a considerable time span between the treatment group (post-reform high school graduates) and the control group (pre-reform graduates). Ideally one would like a control group not exposed to the reform to facilitate a difference-in-difference strategy to prevent macro or year effects from driving the results. However, the universal nature of the grant system and the reform preclude such a control, since all potential students are affected by the reform. Although we have no group who are unaffected by the reform, we still have variation in the treatment provided by the reform. As seen in Figure 1, the magnitude of the treatment is tied to the grant which an individual would have been eligible for before the reform. Therefore, we compare individuals based on the amount of grant eligible for before the reform, and then expect those who would have received lower grants to respond more strongly to the reform, since they experience larger treatments.

That is we can estimate the model

$$\Pr(C_i = 1) = \Phi(\beta_0 + \beta_1 S_i + \beta_2 (S_i \times R_i) + \beta_3' G_i + \beta_4 X_i + \beta_5' Z_i), \quad (2)$$

where C_i is a dummy variable indicating college enrollment within one year of graduation, R_i is a dummy variable determining whether the individual was a member of a post-reform cohort, G_i is a full array of cohort dummy variables, X_i is the index of par-

ent’s income that determine the subsidy, Z_i is a vector of other socioeconomic variables that were controlled for, and Φ is the c.d.f. of a standard Normal random variable. The key variable in the analysis is β_2 which should be negative as the individuals receiving the smallest subsidies ex-ante were the largest beneficiaries of the reform (see Figure 1).

Table 6 summarizes the results from probit models based on equation (2) for the sample of 19-20 year old high school graduates from the cohorts 1985, 1988-90. In specification (1), the interaction between the reform dummy and the predicted pre-reform grant identifies the difference in responsiveness to the reform between those who got a large treatment and those who got a small treatment (i.e. β_2 in equation 2). The difference between the smallest and largest treatment amounts to approximately \$4,000 dollars per year. Since the predicted grant is normalized to have a maximum of one, we find that a change in grants of \$4,000 dollars per year leads to a change in enrollment of 6-7 percentage points. This effect is small compared to the previous literature, and the confidence band rules out estimates as large as found in the previous literature.¹⁰ This is partly caused by the fact that total subsidies were and still are larger in Denmark than it has ever been in the US.

Both the pre-reform grant and the magnitude of the treatment induced by the reform are tightly linked to the financial situation of the parents. In specification (1) in Table 6, we include the income percentile of the parents in the probit regression, and we see a 8.6 percentage point higher enrollment for the individuals at the top of the income distribution compared to the individual at the very bottom of the distribution. Hence, we allowed children of “rich” parents to have a different, presumably higher, enrollment rate than children of “poor” parents. However, we assumed that they, for a given change in the stipend, respond to the same extent. In other word, we allowed the rich and poor to differ in levels, but not in their sensitivity to net costs of college.

This is a major issue if one is worried about borrowing constraints, different income

¹⁰The precision of the estimate rules out changes in enrollment higher than 12 percentage points, which amount to 3 percentage points for a \$1,000 dollars grant increase.

effects, and contingent parental transfers. All of these arguments suggest that the responsiveness to changes in net costs might depend on the financial conditions of the family. If this is the case, we might *understate* the true effect of the stipend on the demand for college, since individuals with higher values of X_i benefitted more from the program. To look for these mechanisms and in particular to control for them in identifying the effect of grant changes on enrollment, we allow income to have a different effect after the reform than before the reform.¹¹

5.2 Accounting for borrowing constraints: A regression kink design

We control for the parents' financial situation by including their position in the income distribution. If we believe that, for instance, borrowing constraints or contingent parental transfers have a significant effect on enrollment, then it is crucial to allow the response to the subsidy to vary with income. However, allowing for this hinges on being able to separate the effect of the subsidy from that of the parents' financial conditions. In our case, pre-reform stipend (and the treatment following the reform) and parents' income are strongly correlated, as the former is a function of the latter. Generally, this would force identification to come from an assumption about the functional relationship between income and enrollment. However, the means-testing algorithm provide us with a kink in the relationship between parents' income and the grant eligible for. This kink could be exploited for non-parametric identification.

Consider the following general specification in which Y_i represents a generic dependent variable,

$$Y_i = g(X_i, S_i) + u_i$$

where X_i is a continuous variable and S_i is the treatment variable which we treat as continuous here. The main issue for identification is that S_i is completely determined

¹¹Whenever parents' income or financial situation is mentioned, the empirical counterpart we have in mind is the index determining the subsidy.

by X_i (i.e. $S_i = S(X_i)$). The standard problem one faces in this type of analysis is that u_i is correlated with X_i (and thus S_i). However, suppose that there is a kink in the function S at some value x^* , but not in the function $g(X_i, \cdot)$ or the function $E(u_i | X_i)$. To simplify, we assume that $g(X_i, \cdot)$ and $E(u_i | X_i)$ are continuous differentiable. Define d_0 and d_1 such that

$$d_0 \equiv \lim_{x \uparrow x^*} \frac{\partial S(x)}{\partial x}$$

$$d_1 \equiv \lim_{x \downarrow x^*} \frac{\partial S(x)}{\partial x}$$

Then

$$\begin{aligned} & \frac{\lim_{x \downarrow x^*} \frac{\partial E(Y_i | X_i = x)}{\partial x} - \lim_{x \uparrow x^*} \frac{\partial E(Y_i | X_i = x)}{\partial x}}{d_1 - d_0} \\ = & \frac{\lim_{x \downarrow x^*} \left(\frac{\partial g(x, S(x))}{\partial x} + \frac{\partial g(x, S(x))}{\partial s} \frac{\partial S(x)}{\partial x} + \frac{\partial E(u_i | X_i = x)}{\partial x} \right)}{d_1 - d_0} \\ & \frac{\lim_{x \uparrow x^*} \left(\frac{\partial g(x, S(x))}{\partial x} + \frac{\partial g(x, S(x))}{\partial s} \frac{\partial S(x)}{\partial x} + \frac{\partial E(u_i | X_i = x)}{\partial x} \right)}{d_1 - d_0} \\ = & \frac{\frac{\partial g(x^*, S(x^*))}{\partial x} + \frac{\partial g(x^*, S(x^*))}{\partial s} d_1 + \frac{\partial E(u_i | X_i = x^*)}{\partial x}}{d_1 - d_0} \\ & \frac{-\frac{\partial g(x^*, S(x^*))}{\partial x} - \frac{\partial g(x^*, S(x^*))}{\partial s} d_0 - \frac{\partial E(u_i | X_i = x^*)}{\partial x}}{d_1 - d_0} \\ = & \frac{\partial g(x^*, S(x^*))}{\partial s} \end{aligned}$$

Hence, this treatment effect is nonparametrically identified at the kink points. In practice, one must use more parameterized models to obtain reasonable precision. But still, the difference in sensitivity to income changes between those just to the left of the kink point and those just to the right of it, can provide identification. Thus, theoretically, the treatment effect is identified even when we allow the effect of parental

income to be different after the reform.¹² In practice, data will determine whether we have sufficient power to separate S_i from parental income X_i .

In Figure 3, we will show kernel regressions of enrollment on the income percentile of the parents cohort by cohort together with a kernel regression of the 1985 stipend on income percentile.¹³ Before discussing what one actually sees in the data, first consider what one would expect to see if borrowing constraints were important. In Figure 2 we have superimposed the college enrollment rates onto Figure 1. The two dashed lines present the patterns one would expect if borrowing constraints were important. Consider first the pre-reform patterns shown as the lower of the two dashed lines. In the area to the left of x_1 , all individuals receive the maximum subsidy so we would expect enrollment rates to increase with income due to the borrowing constraints (where in this figure we have assumed the relationship is linear). In the segment between x_1 and x_2 there are two counteracting forces. As before we have borrowing constraints but balancing that is the fact that the subsidy is phasing out which should lead to a more negative slope. We have drawn the figure so the second effect dominates and the slope is negative which it does not have to be. What is crucial is the slope in this segment should be less steep than in the segment with $X < x_1$. Finally, when $X > x_2$ the subsidy is zero, so again all that should matter is the borrowing constraints thus this should again be increasing. In particular the slope should jump right around the kink point x_2 . The higher of the dotted lines represents the post-reform response. This should have three properties: a) it should be smooth since there are no kink points in the subsidy, b) it should be higher than in the previous line because the subsidy has increase for everyone, c) if borrowing constraints are important, the slope of the line should be less steep because the subsidy should have alleviated borrowing constraints lowering the relationship between income and enrollment. Thus if borrowing constraints are important we should see a bigger impact of the program when we compare individuals

¹²This assumes that the kink points are known and that they are exogenous to income formation. In the empirical analysis, we check the latter assumption in line with Blundell, Duncan and Meghir (1998).

¹³We use the LOWESS command in Stata to create this figure.

with very low income to those with $X \approx x_1$ and we should also see a bigger impact of the program when we compare individuals with $X \approx x_2$ to those with high income.

In Figure 3 we present kernel estimates of the analogous to the predictions in Figure 2. One can see no evidence of many of the patterns we predicted in Figure 2. The three fattest lines are the post-reform regressions. In the two cohorts 1988 and 1990, the increase with income percentile is relatively abrupt and steep after roughly the 60th percentile, but for the cohort 1989, the gap between the cohort and the pre-reform cohort is large around the first kink but then the gap declines.

To implement this in our probit model, we account for this problem by allowing the slope on Parent's Income to vary with the reform

$$\Pr(C_i = 1) = \Phi(\beta_0 + \beta_1 S_i + \beta_2 (S_i \times R_i) + \beta_3' G_i + \beta_4 X_i + \beta_5' Z_i + \beta_6 (X_i \times R_i)). \quad (3)$$

Clearly this is not nonparametric, but we will see that even this restricted model was too imprecise to draw broad conclusions.

The probit regression in specification (2) in Table 6 is based on equation (3). The coefficients of interest change dramatically, but we also see a considerable cut in precision. This indicate that we lack the power in our data to separate the effect of stipend from that of parents' income. The upper bound of the confidence interval is 0.158 which corresponds to an effect that an increase in \$1000 subsidy should increase college enrollment by about 4 percentage points. One can also see that the interaction between Income Percentile and Reform is not significant at conventional levels and goes in the opposite of the predicted direction.

In specification (3) and (4) we exclude youth with at least one self-employed parent. This leave the qualitative conclusions unchanged although the magnitudes of the effects are smaller. In specification (2) and (4), we allowed borrowing constraints to be picked up by the measure of parents' income. One could argue that a better measure also would take the parents assets and the degree of liquidity of these assets into account.

That we will pursue in the next subsection.

5.3 Accounting for borrowing constraints: Using low liquid assets

We adopt a measure of borrowing constraints developed by Zeldes (1989) and used in this Danish data by Leth-Petersen (2004). We get a powerful test of the effect of borrowing constraints by adopting a sample split that divides the sample into a group who are definitely *not* borrowing constrained versus a residual group who may be borrowing constrained. The assumption is that households who have high liquid assets relative to income are definitely *not* borrowing constrained, whereas households who have low liquid assets relative to income are potentially borrowing constrained. It is implicitly assumed that the low liquid assets households currently face a binding constraint because adverse income or consumption shocks have forced them to run down liquid assets in the past. As a measure of borrowing constraints, this is to be preferred over the measures that are usually applied: parents' income, parents' education or race (Cameron and Taber, 2004; Carneiro and Heckman, 2001), because it more accurately identifies households who are potentially constrained.

We construct a basic and an extreme indicator for being potentially borrowing constrained: The basic indicator takes the value one if parents' liquid assets falls short of one months' income, whereas the extreme indicator takes the value one if the parents' liquid assets falls short of two months' income. For self-employed the amount of liquid assets is not registered. Therefore, whenever we account for low liquid assets, we either include an indicator for whether the parents were self-employed the year before graduation or we exclude individuals with self-employed parents.

In Table 7, we present the results of an estimation of the probit model where we control for low liquid assets and interact the low liquid asset indicator with the reform indicator to check whether potentially borrowing constrained individuals reacted differently to the reform than others. We still include the measure of parental income (the

position in the income distribution) though we presume that conditional on the indicator of liquid assets the effect of parents income is constant across the reform. Thus the equation now takes the form

$$\Pr(C_i = 1) = \Phi(\beta_0 + \beta_1 S_i + \beta_2 (S_i \times R_i) + \beta_3' G_i + \beta_4 X_i + \beta_5' Z_i + \beta_6 (D_i \times R_i) + \beta_7 D_i), \quad (4)$$

where D_i is an indicator of being borrowing constrained (either $D1$ or $D2$).

In specification (1) in Table 7, we see that the basic low liquid asset indicator, $D1$, has a negative coefficient, which may be interpreted as evidence of binding borrowing constraints. We would expect the reform to reduce the extent of borrowing constraints, and this is also what the coefficient to the interaction term between $D1$ and the reform indicator shows. However, precision is low and the coefficients are not significant at conventional significance levels. In specification (2) we use the extreme low liquid asset indicator, $D2$, which has a significantly negative effect on enrollment, which disappears after the reform, although the latter result is not significant at normal levels. The indicator for having at least one parent who are self-employed seem to have a similar effect as $D1 = 0$ or $D2 = 0$, which may indicate that a self-employed business is like a type of a liquid asset.¹⁴ Therefore, to achieve higher precision, in specification (3) we construct a combined measure of being borrowing constrained which takes the value one for individuals who have (extremely) low liquid assets, i.e. $D2 = 1$, or no self-employed parent. The coefficient to this liquid assets indicator is -.053 and the effect disappears after the reform - and both effects are significant at normal levels. That the effect disappears after the reform reassures us that the asset indicator in fact captures ability to meet short run financial requirements, and not a relationship due to differences in preferences of cognitive skills. In specification (4) and (5), we exclude the self-employed, and find results that are similar to specification (1) and (2).

The point estimates of the effects suggest that borrowing constraints may matter,

¹⁴The indicator for having at least one self-employed parent may also reflect the potential effect of systematically underreported income among the self-employed.

although most coefficients are not statistically significant at conventional levels. The overall effect of the key variable, β_2 , is modest, suggesting a relatively modest effect of the reform. The point estimate of β_2 depends on how we handle the self-employed parents. When kept in the analysis and singled out by an dummy variable, as in specification (1)-(3), we find that a change in grants by \$4,000 dollars a year change enrollment by 6 percentage points. When the self-employed are entirely omitted from the analysis we estimate an effect of 3 percentage points, cf. specification (4) and (5).¹⁵

In Table 8, we present the results from estimation of a conditional logit model, where we distinguish between the three different levels of higher education (see Table 1). We choose to estimate a conditional logit model (or multinomial logit model) rather than an ordered or a nested logit model because the three different levels of education are best describe as nomial variables. We find that the effect of increased subsidy on enrollment seems to be driven by increased enrollment into short cycle higher education, whereas the negative effect of being potentially borrowing constrained seems to be driven by lower enrollment into long cycle higher education by potentially borrowing constrained individuals.

5.4 Analysis of capacity constraints

The effect of a change in demand on the observed quantity depends crucially on the supply side (that is the number of openings for students throughout the country). This is also the case in the market for education. To derive the change in demand from changes in observed college enrollment and attainment, we need to know something about the supply side of the educational market or at least make our results conditional

¹⁵We have performed a number of robustness checks. Firstly, if parents consciously manipulate their income to arrive at the left of the kink points, our results would be biased. Following Blundell, Duncan and Meghir, we run the regressions for individuals far away from the kink points, and the conclusions are unchanged. Secondly, we would expect some selection into college by high school GPA and parental income. Unfortunately, our dataset is not large enough to run regressions separately by GPA intervals. Thirdly, the low liquid asset indicator may pick up the income effect as mentioned in the introduction. To better control for the income effect, we try to include other asset and income variable and to interact those with the reform indicator. Such parameters are not well-determined.

on assumptions about supply. The existing literature have, most often, implicitly, assumed a totally flexible supply of education, thereby equating demand changes to observed changes in quantities. This approach seems reasonable when studying effects on a limited subset of the population, but when whole cohorts are affected, as in our case, the supply of education might no longer adjust fully to match the increased demand.

Generally, if supply is not perfectly elastic, an increase in demand would lead to price increases. In the Danish educational system, however, education is publicly provided, so there are no direct price mechanism in the educational sector to observe. Thus, we need another observable variable to somehow gauge the elasticity or flexibility of the supply. When the demand for a particular education exceeds the study places supplied, the applicants are to a large extent sorted by their high school GPA.¹⁶ When the net return to education increases, we would expect the demand for education to increase for high school graduates with low as well as high GPA. If the supply is totally elastic and follows demand, the composition of those being induced to take further education decide whether the average GPA of enrolled students goes up or down. However, if the supply is fixed we would expect the average GPA of enrolled students to increase, as GPA is the main sorting instrument.

In Figure 4, which is based on a gross data set for a longer time period, we plot the average high school GPA of first-year students for all colleges and for university college. We do not see an unambiguous effect of the reform on the average GPA of enrolled students, but there seem to be a upward trend from 1984 with a slight drop in 1988 and a jump afterwards. This observation might indicate an increased excess demand for education, and, therefore, a wedge between the increase in demand and the increase in actual enrollment. However, the increased average GPA of enrolled students in Figure 4 might just be a result of a time-varying distribution of high school GPA as indicated by Tabel 3. To accommodate this potential problem, we normalize each student's GPA by the average in his or her high school cohort. Still, the figures are vulnerable to

¹⁶A (varying) fraction of the study places are reserved for so called second-quota-applicants, who can supplement their GPA with e.g. work experience, folk high school.

changes in other moments in the distribution of high school GPA. In Figure 5 we plot the averages of these relative GPAs for first-year students. Now the series seem more stationary - though, still with a slight drop in 1988 and a jump up in 1989 - indicating that the increased enrollment following the reform was not to a considerably extent dampened by an inflexible supply. To conclude, potential supply constraints do not seem to have changed the composition of enrolled students with respect to high school GPA. This analysis is, of course, not perfectly capable of identifying the elasticity of the supply, but with Figure 4 in mind, we are more confident in directly linking changes in observed enrollment to changes in demand for education.

5.5 Estimation of Structural Model

In our evidence to this point we have found no strong evidence that borrowing constraints are particularly important and also that the reform does not appear to be particularly effective in promoting education. However, without structure it is hard to interpret the estimates that we have. Does our lack of evidence on borrowing constraints result because they are not important or because we have little precision? By putting some simple structure on the model we can gain some insight to this question. In this section we will discuss the extension of our model into a simple structural framework which extends that of Cameron and Taber (2004).

Individuals derive utility from consumption and tastes for nonpecuniary aspects of schooling. These nonpecuniary tastes could represent the utility or disutility from school itself or preferences for the menu of jobs available at each level of schooling. Assuming agents have log utility over consumption in each period, lifetime utility for schooling level S is given by

$$U_s = \sum_{t=0}^{\infty} \delta^t \log(c_t) + \nu_S$$

where c_t is consumption at time t , ν_S represents nonpecuniary tastes for schooling level

S , δ is the subjective rate of time preference. Note that we have abstracted from uncertainty in the model.

As of this draft we restrict the analysis to the binary choice of whether to go college or not. Individuals choose S out of this set so that

$$S = \arg \max \{U_S \mid S \in \{0, 1\}\},$$

where 1 represents going to college, 0 represents not going to college.

Cameron and Taber (2004) assume that people borrow and lend at rate $\frac{1}{\delta}$ after school, but at rate R while in school. For borrowing constrained students $R > \frac{1}{\delta}$, whereas for non borrowing constrained students $R = \frac{1}{\delta}$. In their model, which approximates the U.S., income is zero while in school so students must borrow while in school.

However, this model is not appropriate for the Danish case. Since students may be able to subsist on the subsidy, they may not have to borrow while in school and may actually save. We augment Cameron Taber model in a straight forward way. Following them, we assume that the borrowing rate while in school is R . However if students save while in school, they do it at the market rate rate $\frac{1}{\delta}$.

Let W_1 and W_0 be the present values of earnings discounted to the time of labor market entry for college goers and non-college goers, respectively, and f is income during college. There are three possibilities in this model depending on the level of f :

- If f is sufficiently low, students will borrow while in school at rate R
- If f is sufficiently high, students will save for the future while in school at rate $1/\delta$
- For intermediate values, the student will neither borrow nor lend, but consume f while in school.

Those who are not borrowing constrained can borrow and lend at rate $1/\delta$. For the individual who are potentially constrained, we assume that R is sufficiently large that

borrowing constrained people will not borrow during college. That is, for tractability, we assume that only the second or third of the possibilities above actually occur.

Letting V_{1i} be the utility from consumption if one goes to college. Then, for those that can borrow at the market rate:

$$V_{1i} = \frac{\log(1-\delta)}{1-\delta} + \left(\frac{1}{1-\delta}\right) \log(\delta W_1 + f_i).$$

For those that are potentially borrowing constrained:

$$V_{1i} = \begin{cases} \log(f_i) + \delta \left[\frac{\log(1-\delta)}{1-\delta} + \left(\frac{1}{1-\delta}\right) \log(W_1) \right] & \text{if } f_i < (\delta W_1 + f_i)(1-\delta) \\ \frac{\log(1-\delta)}{1-\delta} + \left(\frac{1}{1-\delta}\right) \log(\delta W_1 + f_i) & \text{otherwise} \end{cases}.$$

The condition $f_i < (\delta W_1 + f_i)(1-\delta)$ indicates the cutoff for whether the constraint binds. If it is violated, an individual is borrowing constrained in the sense that if she were offered a loan at the market interest rate, she would choose to take it.

Next we describe the econometric model that we have implemented. Let I_i be the income of the family of individual i and let T_i represent time. To keep things simple we specify the first period income as

$$f_i = S(I_i, T_i) + X_i' \beta$$

where $S(I_i, T_i)$ is the stipend for which individual i is eligible and X_i represent other potential factors (such as family income). Let W_{1i} and W_{0i} be our estimate of the present value of earnings of individual i as a high school or college graduate.¹⁷

We next assume that we can write the difference in nonpecuniary tastes across schooling levels as

$$\begin{aligned} \nu_{1i} - \nu_{0i} &= \frac{Z_i' \theta}{\delta} + \varepsilon_i \\ \varepsilon_i &\sim N(0, \sigma_\varepsilon^2) \end{aligned}$$

¹⁷As of this writing, W_{0i} and W_{1i} do not vary across individuals.

where Z_i contains a vast set of controls including the borrowing constraints indicator, parents' income, and time indicators, T_i . Then the probability of going to college is

$$\Phi\left(\frac{1}{\sigma_\varepsilon}[V_{1i} - V_{0i}] + Z_i'\theta\right), \quad (5)$$

where $\Phi(\cdot)$ is the cdf of a standard normal variable.

Thus the model is a nonlinear probit and estimation by maximum likelihood is straight forward. We estimate the model using the low liquid asset holding as an indicator of whether a student is potentially borrowing constrained. In doing so we control for family income, the interaction between family income and low assets, and for graduation cohorts. In this sense, identification is analogous to the difference in differences models presented above. The results are presented in Table 9. By looking at the form of the probit model in equation (5), one can see that the key parameter for the model is the coefficient $1/\sigma_\varepsilon$ which is the coefficient on $V_{1i} - V_{0i}$. While not imposed by the specification, one can see that the parameter is positive and statistically significant as one would expect. Thus it is comforting that the key parameter has the expected sign and that its magnitude is robust across the different specifications.

The main advantage of estimating the structural model is that we can use it to simulate various policy counterfactuals. In Table 10 we present results that conform to two different types of simulations. The first involves relaxing borrowing constraints while the second involves changing the subsidy level. The middle row in Table 10 corresponds to the current subsidy level. Looking in the third and fourth columns one can see that relaxing the borrowing constraint would lead to only a slight increase in college going rates for the constrained, from 33.7% to 34.7%. However the subsidy itself is important. Completely eliminating the subsidy would have a large effect on college enrollment, particularly among the borrowing constrained. One should of course keep in mind that this is a very large subsidy so this is not surprising. Furthermore, we are ignoring general equilibrium effects which Heckman, Lochner, and Taber (1998) show can be very important in this context.

An interesting result is the importance of the borrowing constraint in the absence of the subsidy (which should not disappear with GE effects). Borrowing constraints would lower school enrollment for the students from families with low assets by approximately 25% in this case (0.220 relative to 0.165). Thus borrowing constraints do not appear to be particularly important in Denmark at this time, but this is in large part due to the subsidy.

In Table 11 we show the result of our second exercise which is to calculate how large the subsidy should be to completely eliminate borrowing constraints.¹⁸ One can see that the subsidy is very large, around \$10,000 in U.S. dollars.

6 Conclusion

Empirical studies across time and countries find a strong intergenerational correlation in schooling, and more generally a strong relationship between family background and schooling. To make the educational attainment less dependent of the parental background, educational subsidies which are means-tested against parental income have been introduced all over the world. We devote this paper to study how those subsidies influence the demand for college education.

>From a reduced form analysis taking potential borrowing constraints into account, we find that college enrollment increases with increasing subsidy. A \$1,000 increase in the stipend increases college enrollment by 1-3 percentage points, which is a significantly lower response than found in the earlier literature. One reason might be that large subsidies are already in place. Introducing a simple structural model allows us to simulate different policy counterfactuals. These exercises shows that borrowing constraints do not appear to be particularly important in Denmark at this time, but this is in large part due to the subsidy.

¹⁸Eliminating borrowing constraints in this context means that there would be nobody who would like access to a fair market loan-but couldn't get one.

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Educational category	Length	Examples
Short-cycle higher education	2 years	The fields of study include agriculture, textile and design, food industry, construction, hotel and tourism, computer programming, industrial production, laboratory technician, IT and communication and international marketing.
Mediumcycle higher education	3 to 4 years	Professional programmes like teacher training programmes, programmes in social work, journalism, nursing, engineering etc. and research-based Bachelor programmes.
Long-cycle higher education	5 to 6 years	The master programmes. The programmes qualify students for occupational functions and scientific work.

Table 1: Overview of higher education programmes in Denmark

Table 2
College Enrollment, Year X After Graduation

High school graduation cohort	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5	Nobs
1985	0.144	0.310	0.453	0.535	0.581	0.604	2229
1986	0.138	0.296	0.435	0.519	0.573	0.600	2320
1987	0.129	0.299	0.463	0.558	0.610	0.642	2132
1988	0.121	0.315	0.489	0.588	0.642	0.668	2054
1989	0.127	0.339	0.483	0.566	0.611	0.642	2208
1990	0.167	0.395	0.536	0.618	0.664	0.694	2289
All cohorts	0.138	0.326	0.476	0.563	0.613	0.641	13232

Table 3

Means of Central Variables by High School Graduation Cohort

	High school graduation cohort					
	1985	1986	1987	1988	1989	1990
High school graduation age (years)	19.35	19.43	19.43	19.43	19.43	19.39
GPA	8.01	8.10	8.15	8.19	8.21	8.30
GPA missing (0/1)	0.235	0.256	0.258	0.264	0.309	0.278
Mothers education category 0 (basic school)	0.441	0.421	0.374	0.360	0.353	0.315
Mothers education category 1 (high school or similar)	0.019	0.013	0.014	0.016	0.018	0.021
Mothers education category 2 (vocational education)	0.332	0.360	0.363	0.367	0.369	0.375
Mothers education category 3 (short cycle higher education)	0.034	0.040	0.042	0.043	0.042	0.047
Mothers education category 4 (medium cycle higher education)	0.152	0.147	0.184	0.191	0.192	0.204
Mothers education category 5 (long cycle higher education)	0.022	0.019	0.023	0.024	0.027	0.038
Fathers education category 0 (basic school)	0.370	0.330	0.339	0.316	0.296	0.284
Fathers education category 1 (high school or similar)	0.019	0.019	0.023	0.019	0.024	0.025
Fathers education category 2 (vocational education)	0.353	0.398	0.363	0.370	0.381	0.370
Fathers education category 3 (short cycle higher education)	0.029	0.030	0.037	0.037	0.039	0.038
Fathers education category 4 (medium cycle higher education)	0.135	0.141	0.155	0.156	0.162	0.172
Fathers education category 5 (long cycle higher education)	0.095	0.082	0.083	0.102	0.098	0.111
D1: Parents' liquid assets exceed one month's income (0/1)*	0.472	0.460	0.453	0.541	0.480	0.472
D2: Parents' liquid assets exceed two month's income (0/1)*	0.366	0.359	0.366	0.442	0.388	0.367
At least one of the parents are self-employed (0/1)	0.277	0.274	0.292	0.259	0.292	0.256
Number of observations	2229	2320	2132	2054	2208	2289

* If liquid assets are registered and parents are not self-employed.

Table 4
Portfolio Composition by High School Graduation Cohort

Average proportion of portfolio in	High school graduation cohort					
	1985	1986	1987	1988	1989	1990
Cash	0.469	0.498	0.484	0.445	0.514	0.560
Other	0.293	0.275	0.290	0.301	0.237	0.227
Mortgage deeds	0.091	0.081	0.060	0.072	0.065	0.046
Bonds	0.088	0.075	0.064	0.079	0.081	0.067
Shares	0.058	0.071	0.101	0.099	0.098	0.098
Shares in a ship	0.000	0.000	0.001	0.005	0.005	0.002
Total	1.000	1.000	1.000	1.000	1.000	1.000
Number of students with observed liquid assets of parents	1543	1594	1429	1532	1632	1681
	1985	1986	1987	1988	1989	1990
D1: Parents' liquid assets exceed one month's income (0/1)	0.472	0.460	0.453	0.541	0.480	0.472
D2: Parents' liquid assets exceed two month's income (0/1)	0.366	0.359	0.366	0.442	0.388	0.367
Number of students with observed liquid assets of parents	1543	1594	1429	1532	1632	1681

Table 5

Portfolio Composition by Low Liquid Asset Split: Basic vs. Extreme

	Low liquid asset split			
	Basic		Extreme	
Average proportion of portfolio in	d1=0	d1=1	d2=0	d2=1
Cash	0.844	0.350	0.741	0.303
Other	0.089	0.345	0.158	0.357
Mortgage deeds	0.011	0.093	0.023	0.105
Bonds	0.013	0.102	0.024	0.117
Shares	0.042	0.107	0.053	0.115
Shares in a ship	0.001	0.003	0.001	0.003
Total	1.000	1.000	1.000	1.000
Number of students with observed liquid assets of parents	2786	6620	4146	5260

Table 6
 Probit Model for College Enrollment, Cohorts 1985, 1988-1990
 Marginal Effects on College Enrollment
 (Standard Errors in Parenthese)

	(1)	(2)	(3)	(4)
High school graduation age=20 (0/1)	0.016 (0.012)	0.016 (0.012)	0.015 (0.014)	0.015 (0.014)
Proportion of max stipend (1985 regime)	0.107 (0.055)	0.051 (0.089)	0.099 (0.065)	0.066 (0.104)
Proportion of max treatment	0.100 (0.043)	0.021 (0.107)	0.073 (0.051)	0.027 (0.126)
Income Percentile (X)	0.092 (0.047)	0.029 (0.091)	0.140 (0.057)	0.101 (0.112)
Reform \times Income percentile		0.085 (0.105)		0.052 (0.128)
GPA	0.173 (0.006)	0.173 (0.006)	0.176 (0.008)	0.176 (0.008)
GPA missing (0/1)	0.875 (0.015)	0.875 (0.015)	0.882 (0.016)	0.882 (0.016)
Male (0/1)	0.081 (0.011)	0.081 (0.011)	0.082 (0.013)	0.082 (0.013)
Indicators for graduation cohort included	yes	yes	yes	yes
Controls for parents' education included	yes	yes	yes	yes
Include self-employed	yes	yes	no	no
Log-likelihood	-4793	-4792	-3492	-3492
Number of observations	8771	8771	6395	6395

Table 7

Probit Model for College Enrollment With Controls for Borrowing Constraints, Cohorts 1985, 1988-1990
 Marginal Effects on College Enrollment
 (Standard Errors in Parenthese)

	(1)	(2)	(3)	(4)	(5)
High school graduation age=20	0.017 (0.012)	0.016 (0.012)	0.016 (0.012)	0.017 (0.014)	0.016 (0.014)
Proportion of max stipend (1985 regime)	0.064 (0.043)	0.063 (0.043)	0.064 (0.043)	0.068 (0.052)	0.065 (0.052)
Reform \times Proportion of max stipend (1985 regime)	-0.059 (0.030)	-0.058 (0.030)	-0.060 (0.030)	-0.033 (0.037)	-0.032 (0.037)
Income Percentile	0.093 (0.055)	0.088 (0.056)	0.088 (0.055)	0.168 (0.069)	0.160 (0.070)
D1: low liquid assets indicator (0/1) (basic)	-0.033 -(0.029)		(0.028)	-0.030	
Reform \times D1	0.044 (0.034)			0.045 (0.034)	
D2: low liquid assets indicator (0/1) (extreme)		-0.051 (0.027)			-0.048 (0.027)
Reform \times D2		0.040 (0.031)			0.039 (0.031)
Self-employed (0/1)	0.054 (0.032)	0.059 (0.029)			
Reform \times self-employed	-0.060 (0.034)	-0.051 (0.031)			
Self-employed=0 or D2=1			-0.053 (0.023)		
Reform \times (self-employed=0 or D2=1)			0.046 (0.028)		
GPA	0.172 (0.007)	0.171 (0.007)	0.171 (0.007)	0.176 (0.008)	0.175 (0.008)
GPA missing (0/1)	0.871 (0.016)	0.870 (0.017)	0.870 (0.017)	0.879 (0.018)	0.877 (0.018)
Male (0/1)	0.079 (0.011)	0.078 (0.011)	0.078 (0.011)	0.079 (0.013)	0.079 (0.013)
Indicators for graduation cohort included	yes	yes	yes	yes	yes
Controls for parents' education included	yes	yes	yes	yes	yes
Include self-employed	yes	yes	yes	no	no
Log-likelihood	-4436	-4434	-4434	-3136	-3135
Number of observations	8118	8118	8118	5742	5742

Table 8
 Conditional Logit Model, Base: No College
 Odds Ratio
 (Standard Errors in Parentheses)

	Short cycle	Medium cycle	Long cycle
High school graduation age	0.799 (0.109)	0.725 (0.071)	1.028 (0.067)
Proportion of max stipend (1985 regime)	1.719 (0.941)	2.245 (0.822)	1.082 (0.257)
Reform \times Proportion of max stipend (1985 regime)	0.357 (0.144)	0.587 (0.149)	0.937 (0.156)
Income Percentile	1.053 (0.689)	3.027 (1.450)	1.286 (0.391)
D2: low liquid assets indicator (0/1) (extreme)	0.884 (0.310)	0.993 (0.218)	0.700 (0.105)
Reform \times D2	1.005 (0.400)	0.882 (0.223)	1.411 (0.241)
Self-employed (0/1)	1.302 (0.461)	1.164 (0.265)	1.408 (0.224)
Reform \times self-employed	1.250 (0.501)	0.750 (0.202)	0.705 (0.128)
GPA	0.897 (0.077)	1.487 (0.081)	2.879 (0.111)
GPA missing (0/1)	0.224 (0.154)	4.935 (2.287)	1547.436 (514.025)
Male (0/1)	0.789 (0.111)	1.204 (0.114)	1.689 (0.102)
Indicators for graduation cohort included		yes	
Controls for parents' education included		yes	
Include self-employed		yes	
Log-likelihood		-6341	
Number of observations		8118	

Table 9
 Estimates From Structural Model
 (Standard Errors in Parentheses)

Variable	(1)	(2)	(3)
$\frac{1}{\sigma_\varepsilon}$	3.398 (1.567)	3.401 (1.568)	3.102 (1.451)
	Parameters determining f_i (i.e. β)		
Intercept	1.602 (1.214)	1.757 (0.772)	3.248 (1.254)
Family Income	1.933 (1.720)	1.707 (1.245)	
	Taste Parameters (i.e. θ)		
Intercept	-8.151 (1.733)	-8.139 (1.722)	-7.714 (1.521)
Family Income	0.383 (0.219)	0.361 (0.167)	0.239 (0.065)
Low Asset	0.025 (0.192)	-0.001 (0.077)	-0.043 (0.049)
Lows Asset \times Family Income	-0.038 (0.247)		
Graduate 1988	-0.312 (0.121)	-0.312 (0.119)	-0.277 (0.110)
Graduate 1988	-0.218 (0.123)	-0.218 (0.122)	-0.180 (0.112)
Graduate 1988	-0.099 (0.126)	-0.099 (0.125)	-0.060 (0.116)
GPA	0.48 (0.018)	0.48 (0.018)	0.48 (0.018)
GPA Missing	3.124 (0.156)	3.124 (0.156)	3.123 (0.155)
Male	0.218 (0.031)	0.218 (0.031)	0.218 (0.031)
Age 19 at HS Graduation	-0.038 (0.032)	-0.038 (0.032)	-0.039 (0.033)
Parent's Education Dummies	Yes	Yes	Yes

Table 10

Simulated College Enrollment From Structural Model

	High Asset	Low Asset		Full Population	
		With Borrowing Constraint	Without Borrowing Constraint	With Borrowing Constraint	Without Borrowing Constraint
No stipend	0.235	0.165	0.220	0.213	0.230
Actual stipend	0.367	0.337	0.347	0.358	0.361
Double stipend	0.509	0.496	0.496	0.505	0.505

Table 11Average Subsidy to make Possible
Borrowing Constraint Non-Binding

	Low Asset	Full Population
Subsidy	2.41	2.45
In US\$	\$10,032	\$10,218

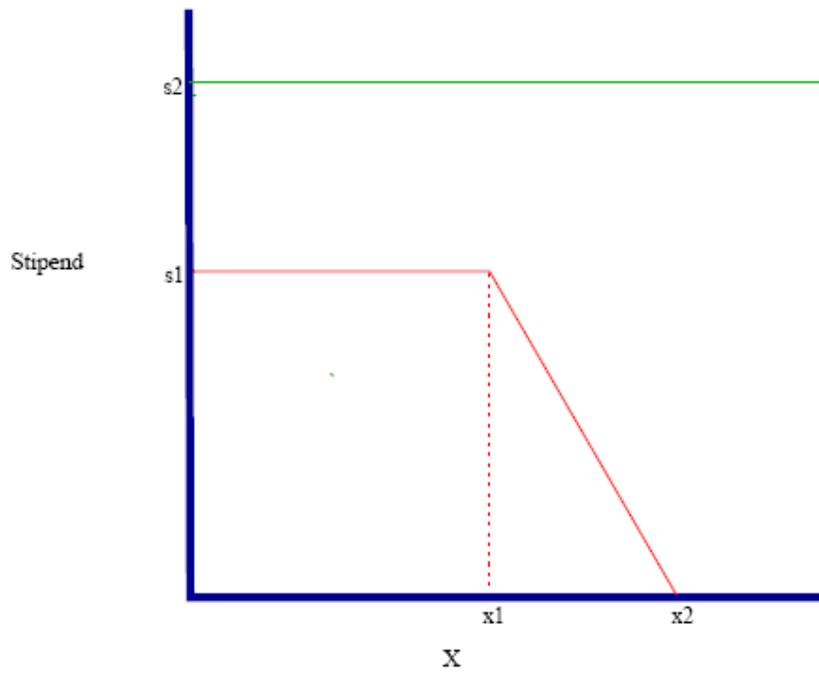


Figure 1: Illustration of the influence of the reform on the stipend.

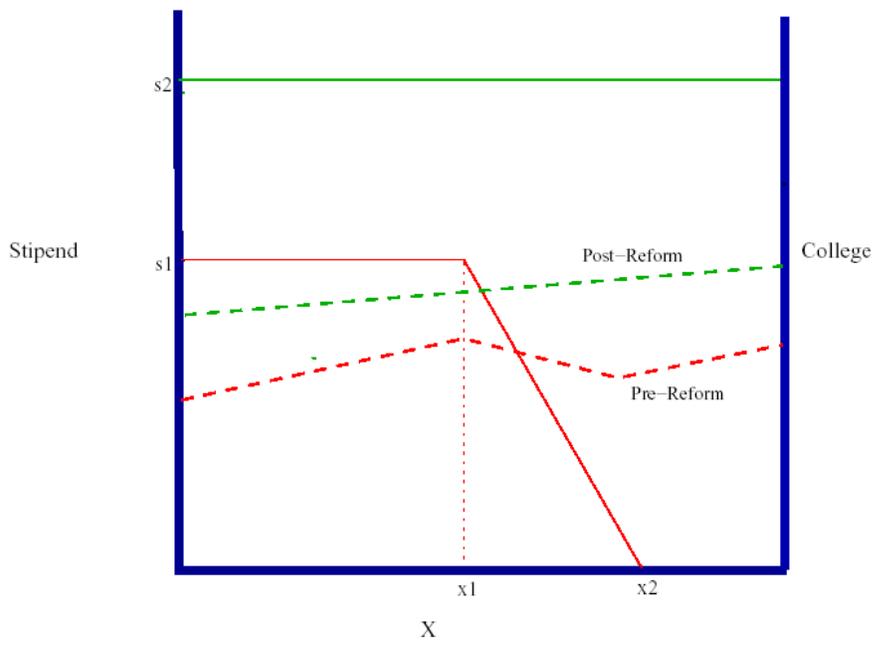


Figure 2: The stipend rule and it's relation to enrollment.

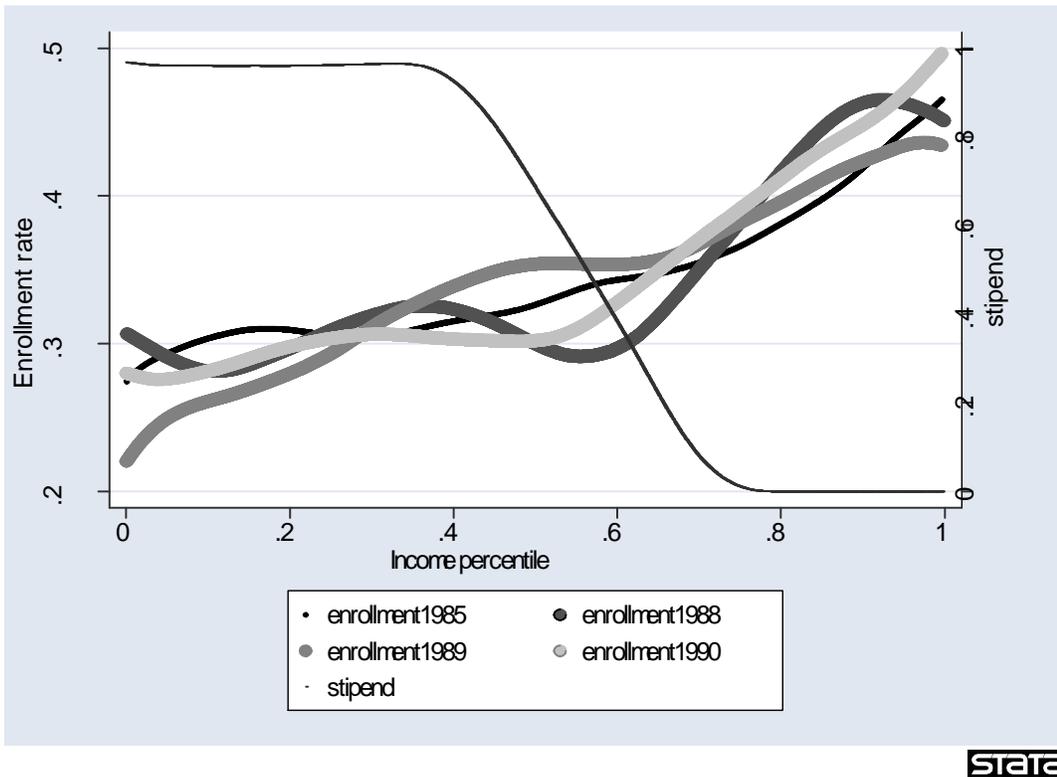


Figure 3: Kernel regression of college enrollment on income percentile.

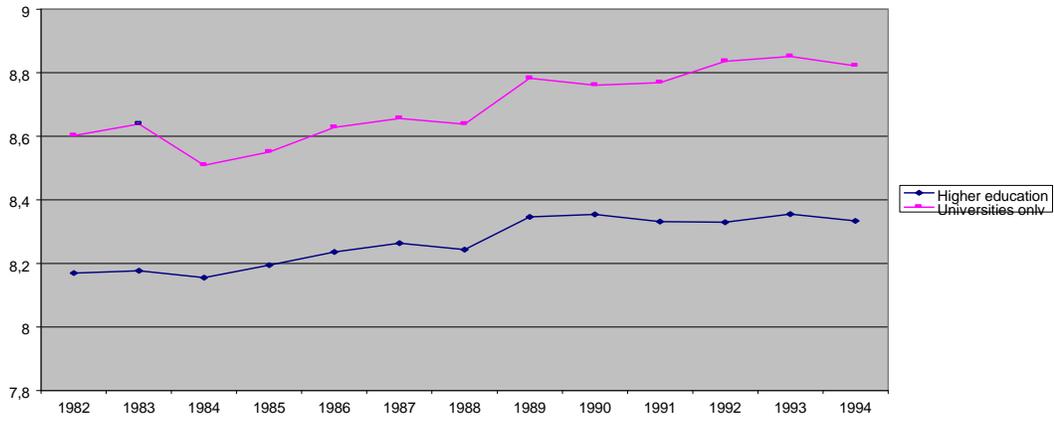


Figure 4: High school GPA of first-year students.

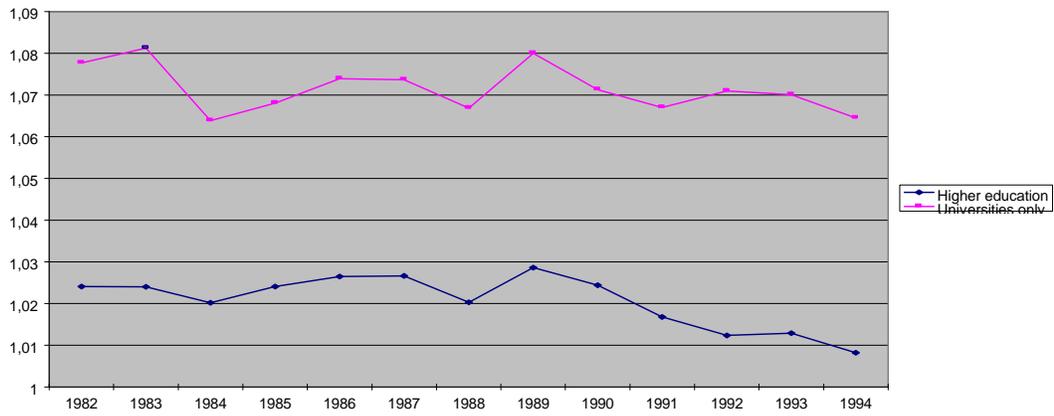


Figure 5: High school GPA of first-year students relative to high school cohort average.