Estimating the Effect of Student Aid on College Enrollment: Evidence from a Government Grant Policy Reform[1]

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Abstract

In this paper, we estimate the response of college enrollment 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 only deters college enrollment to a minor extent. We interpret this as a consequence of the fact that the Danish aid scheme was already fairly generous before the reform.

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

JEL classification: I22, J24.

1 Introduction

An increase in educational subsidies is expected to increase college enrollment. Despite the fact that substantial educational subsidies are already in place, previous empirical work suggests that the elasticity is large. Empirical studies for the U.S. find that the magnitude of the effect is such that a $1,000 increase in the annual educational subsidy increases enrollment by roughly 3-5 percentage points (see e.g. Dynarski, 2003; Leslie and Brinkman, 1988; 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 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 show how a combination of a kinked aid scheme and a reform of the student aid scheme can be exploited to identify the effect of direct costs on college enrollment accounting for borrowing constraints. We show that, theoretically, the first characteristic is sufficient to identify the effect of interest although in practice the variation induced by the reform improves identification a lot.

The reform of the Danish student aid scheme was implemented for the college cohort starting September 1988 and consisted of two major changes: it eliminated the means-testing and it raised the level of grants by more than 25% for all students above 19 years of age. The change in subsidy varied across parental background. After the reform, educational subsidies universally covered all students throughout their college education at a generous level.[1]

Children with poor parents (henceforth: poor kids) are often expected 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 more

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

likely to be borrowing constrained; therefore, if increased subsidies reduce the extent of these constraints, poor kids may be more responsive to a policy change. (2) Poor kids may receive lower schooling contingent transfers from their parents. If public transfers crowd out some schooling contingent parental transfers, a policy change may have a smaller impact on rich kids. (3) Poor kids receive less non-labor income transfers from their parents over their life cycle than rich kids. This 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, poor kids 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) and (3). Our efforts are focused on accounting appropriately for channel (1), namely, the effect of borrowing constraints.

We use Danish register-based data for the cohorts graduating from high school in 1985 to 1990. In this way we have observations both before and after the reform. We rank individuals according to the measure of parental income which determines eligibility for 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 kinked 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 stipend separately from the income-enrollment relationship. However, the variation induced by the reform improves identification a lot.

In a more direct approach, we adopt the low liquid asset measure, which was suggested by Zeldes (1989) and successfully applied by Leth-Petersen (2006), 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 find that the subsidy does increase college enrollment, but to a smaller extent compared to previous work on US data. The point estimates indicate that a $1,000 increase in the yearly stipend increases college enrollment by 1.35 percentage points. This somewhat

[2] This 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.

smaller effect might partly be caused by the fact that total subsidies were and still are larger in Denmark than they have ever been in the US. We find some evidence that borrowing constraints may 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 be important for college enrollment. Borrowing constraints do not appear to be important in Denmark at this time, but this is in large part due to the substantial subsidy already in place.

The remainder of this paper is organized as follows: Section 2 surveys the literature. 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. Consistent with the old review by Leslie and Brinkman (1988), most of the studies find that the effect on college attendance of a $1,000 dollar aid increase ranges from three to five percentage points. However, the results from many of the earlier studies are likely to be flawed by poor identification as aid is correlated with numerous observable and unobservable variables. In the present paper, we carefully consider identification issues while accounting for borrowing constraints by use of a new and promising approach.

Comparing enrollment rates across states, Kane (1995) finds that a $1,000 dollar difference in costs of public 2-year college is associated with a 8-16 percentage points difference in enrollment rates among 18-19 year-olds. 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. The major weakness of this study is that identification is based upon differences between states which have been fairly stable over time, making it difficult to separate the effects of interest from other fixed inter-state differences.

McPherson and Schapiro (1991) find a significant effect of changes of the Pell Grant as a

$1,000 dollar increase in net costs (1978-1979 dollars) is estimated to reduce the enrollment of low-income students by 6.8 percentage points. Seftor and Turner (2002) use the annual micro-data from the CPS to examine the effect of changes in the Pell Grant program on mature students. They find this group to be more responsive to changes than traditional college-goers. More severe borrowing constraints for the mature students might justify this. Dynarski (2003) estimates the effect of a $1,000 dollar subsidy on enrollment to be 3.6 percentage points. She uses the exogenous variation in schooling costs stemming from the elimination of the Social Security Student Benefit Program, which subsidized students of deceased, disabled, or retired parents.

The previously mentioned results mainly apply to students from low-income families, in that the youth subsidized by the Pell program and to a large extent also those subsidized by the Social Security Student Benefit Program are from low-income families. Dynarski (2000) studies the Georgia HOPE program, which mainly affected middle- and upper-income students because any federal (means-tested) grants were deducted from the HOPE stipends. This program 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 finds that a $1,000 dollar subsidy increase raises enrollment rates of middle- and upper-income students by 4-6 percentage points. Thus the estimated effects of a $1,000 subsidy are in the same ball park for disadvantaged and more advantaged students.

There is a growing literature on borrowing constraints with no clear consensus on their importance. Cameron and Heckman (1998, 2001) look at the effect of income on college completion. They find that after controlling for AFQT, the effects of family income are quite small. They argue that short-run credit constraints do not seem to be a major component determining schooling levels. Carneiro and Heckman (2002) follow up on this work that credit constraints do not appear to be important, but claim that 8% of the population of the United States may be subject to short-run credit constraints. Belley and Lochner (2007) extend this approach to consider both the NLSY79 and the NLSY97. They find much stronger income effects for the 1997 data. They argue that it is hard to reconcile these facts without appealing to credit constraints. Whereas these studies interpret estimated differences across income groups as the outcome of borrowing constraints, we pursue a more direct approach in which we identify potentially borrowing constrained and compare their behavior to non-constrained.

Keane and Wolpin (2001) estimate a discrete dynamic programming model of schooling, work, and savings. They find existence of borrowing constraints, but also that relaxing them would have very little effect on college enrollment. Stinebrickner and Stinebrickner (2009) study this issue by asking students enrolled at Berea college about whether they would accept a fair market loan, and then they look at the relationship between the answer to this question and subsequent drop out behavior. While there is a relationship, they conclude that credit constraints are not a very important factor. Some studies exploit variation in family income stemming from variation in variables such as union status, job loss and number of siblings to estimate the effect of being (likely) borrowing constrained on schooling as these factors do not appear to be correlated with schooling (Shea, 2000; Brown, Scholz, and Seshadri, 2007). Results vary.

Results from some of the previous studies may be contaminated by supply side effects or general equilibrium effects as the educational policies giving identification affect large parts of the population. 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 counter-intuitive results. We briefly take the supply side into account. General equilibrium effects on labor market outcomes are analyzed by Heckman, Lochner and Taber (1998), who find that taking the tax financing of the college grant and the effect on relative wages into account 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 an identification strategy which does not rely on the existence of a classic control group not exposed to policy changes as many of the previous studies do. Second, we introduce a new approach to deal with heterogenous responses due to credit constraints. We exploit detailed information on the asset portfolios of the parents to directly identify possible liquidity constraints among potential students. After the empirical analyses, we discuss whether supply side effects and general equilibrium effects may have flawed our conclusions.

3 The Reform

To identify the effect of educational subsidies on enrollment into college, we exploit a reform of the Danish Government Grant Policy which took place in 1988 and which in aggregate numbers more than doubled the amount of study grants awarded. This discontinuous jump in the amount awarded and the number of recipients are shown in Figure 1. However, data points around the reform year are missing. In Denmark, the available college educations can be grouped into three categories: short-cycle, medium-cycle and long-cycle higher education programs. Table 1 gives a brief overview of the three. In this paper, we focus on enrollment into any college education. Exploiting the differences between the three cycles of education is left as a natural, although not immediately straightforward, extension of the paper. The reform may of course also have influenced, for instance, consumption while in college, timing of enrollment, duration to completion and the extent of work while studying.[3] In this paper, we focus on the effect on college enrollment.

All colleges are public institutions and free of charge (no tuition fee). Student grants are universal in the sense that they are given to all students admitted to recognized educational institutions independently of their qualifications. Up to a maximum of 60 months, student grants may be spent flexibly over the whole life-time. There is an upper limit on the amount of wage income allowed. The grant program is well known and the application procedure is simple, and therefore the take-up rate is close to 100%.[4] 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 income measure, X, for all individuals below a certain age limit:

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

where Income is taxable income, Number of Siblings denotes the number of siblings below the age limit, a is annually adjusted to account for inflation, and $f()$ is a nonlinear function of parents' wealth, which also varies over time.[5] The income measure, X, is based on the standard income measure denoted "social income" which was used for means-testing of all

[3] Joensen (2008) and Arendt (2008) have found a negative effect of grants on dropout in Danish data. Arendt (2008) explores this specific reform and he finds no effect of grant on completion.

[4] According to Danish Educational Support Agency (2000), 93% of students at institutions for higher education received a state education grant for at least one month during their education. The residual consists of older students who either earn high wages or are eligible for more favorable public income transfers.

[5] In 1985, the index was given by: X = Mother's Income + Father's Income $-16,600\times$Number of Siblings$+f$(Parents' Wealth). The number of siblings included all siblings below 18 plus siblings between 18-21 if they undertook a college education. Let W indicate parents' wealth, then $f(W) = .1\times(W-447,700)\times 1[W>447,700] + .15\times(W-895,400)\times 1[W>895,400] + .25\times(W-1,492,100)\times 1[W>1,492,100]$. In

sorts of income transfers in Denmark at that point in time. In the rest of this paper, we typically refer to X as social income, but we also refer to it as income. We disregard individuals with divorced parents. Until 1987, the subsidy was means-tested for individuals below the age limit 22. That is, for those younger than 22, the subsidy depended on the social income measure, X, but all students aged 22 and older 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.[6]

The actual reform was announced in 1987, and it was implemented for the cohort starting college after September 1st, 1988.[7] After the reform, educational subsidies universally covered 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 of the means-testing to 19 years. This meant that only the very few students who were born after August 1st and, who followed the fastest possible way through the educational system were means-tested for the first one or two quarters of their studies.[8] Second, 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 DKK per year (2001 prices), which roughly compared to $6,000 per year.[9] As a consequence, the reform actually induces students of wealthy parents to start earlier. If this effect dominates other costs or benefits determining the timing of enrollment, it represents a problem in our analysis as we assume that the reform only influences enrollment as such. Detailed descriptive statistics indicate that students of wealthy parents are not particularly induced to start early, and thus we cautiously conclude that other factors dominate this decision. However, we do perform a range of robustness tests to address this concern.

1985, a value of X below DKK 156,400 resulted in eligibility for the maximum stipend, while a value of X above DKK 256,200 resulted in no stipend.

[6] The 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.

[7] According to the Parliament's yearbooks, the law was first proposed in Parliament on November 18th, 1986, whereas it was finally agreed upon on April 23rd, 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.

[8] We 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 behavior. Means-testing of students above 18 years was finally abolished in 1996.

[9] The average exchange rate during 2001 was 8.32 USD to one DKK.

In Figure 2, we sketch the influence of the reform on the aid scheme. The main purpose 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 $x_1$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.

To exploit the variation provided by the reform, we predict the amount of stipend individuals who are observed post-reform would be eligible for had they graduated before the 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 position in the income distribution. Hence, we rank individuals according to the measure of social income, X_i, which determines eligibility for student aid, and we 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 position in the income distribution at that time.[10] Basically, we are 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. As a consequence, we estimate the effect of the intention to treat rather than the actual treatment. Though, we do not expect behavior determining the income of the parents to be affected considerably by the reform.

Let S_i be the stipend individual i would be eligible for if enrolling at high school graduation. We define the variable, S_i^{pre}, to be the stipend for which the individual would be eligible in 1985.[11] Thus, for an individual from a pre-reform cohort, $S_i^{pre} = S_i$. We have the

[10] As the "social income" was only used for means-testing of income transfers until 1988, it does not exist in the registers after that. In 1987/88, two major tax reforms took place, so the income measures available are therefore not completely comparable. As a measure for X, we compute the best possible post-reform analogue corrected for number of siblings (but not for wealth). We need to assume that the ranking in the social income distribution is unaffected by the fact that the two measures are not completely consistent.

[11] We take four years 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 lump the cycles together, so there are

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_i for the pre-reform cohort, call it x_i^{pre}.
4. Calculate S_i^{pre} as the subsidy corresponding to $X = x_i^{pre}$ using the 1985 rule.

In 1985, 19% of high school graduates would have been eligible for the maximum stipend (because $X < x_1$), whereas 43% of high school graduates would have been eligible for no stipend at all (because $X > x_2$). The former group benefited the least from the reform (the change in average yearly stipend was 11,486 DKK in 2001 prices), whereas the latter group benefited the most (31,756 DKK), while the average change was in between (23,061 DKK).[12] The above-mentioned procedure means that we fix the percentile limits in order to compute the counterfactual pre-reform stipend, S_i^{pre}.

Given the design of the grant scheme and the reform, the basic task we face is to separate the effect of stipend from parental income and cohort effects. Figure 2 illustrates that we have three different sources of variation in the stipend which could potentially identify the effect of interest. Before the reform, the grant varied within each cohort across X. Since the former is a function of the latter, using this source of variation for identification would, generally, require some strong functional form assumptions. The reform provides variation over time. We are, however, excluding two high school graduation cohorts (1987 and 1988) to avoid announcement effects, and we therefore end up with a considerable time span between the treatment group (post-reform high school graduates) and the control group (pre-reform graduates). Hence, directly comparing post-reform individuals to similar pre-reform individuals is obviously not a tempting strategy due to inter cohort variation. Ideally, one would like a control group not exposed to the reform to facilitate a difference-in-difference strategy to prevent cohort or year effects from driving the results. Unforunately for us, the universal nature of the grant system and the reform preclude such a control since all potential

no natural measure for total stipend during education.

[12] The maximum change in *average yearly stipend* over a four-year period is less than the maximum *yearly stipend* (48,968 DKK). This is due to the fact that the pre-reform means-testing ended at age 22, which would be surpassed by all students within a four-year period.

students are affected by the reform. Although we have no group that is unaffected by the reform, we still have variation in the stipend change provided by the reform. As seen in Figure 2, the change in stipend varies across individuals, and this is the source of variation we will exploit for identification.

4 A Simple Model

In this section, we set up a simple model of the college going decision. The model is formulated to provide a straightforward link between the identification of the model and the variation in the data. The model modifies the model in Cameron and Taber (2004).

4.1 Basic Model

Individuals derive utility from consumption and tastes for non-pecuniary aspects of schooling. These non-pecuniary 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, v_S represents non-pecuniary tastes for schooling level S, δ is the subjective rate of time preference. Note that we have abstracted from uncertainty in the model.

We restrict the analysis to the binary choice of whether to go to college or not. Individuals choose schooling 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 approximated the U.S., income is zero while in school so students must borrow while in school.

However, this model is not appropriate in 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 the Cameron-Taber model in a straightforward way. Following them, we assume that the borrowing rate while in school is R. However, if students save while in school, they do so at the market 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 individuals who are potentially constrained, we assume that R is so 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 $f_i < (\delta W_1 + f_i)(1-\delta)$ denotes the cutoff for whether the individual is constrained or not. If it holds, then the 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. Note that f_i will consist, partly, of the educational subsidy received during college. Hence, the subsidy influences the schooling decision through two channels. Directly, by affecting the return to college and, indirectly, by potentially altering the borrowing constraint status of the individual. Whether the subsidy is going to be pivotal for the constraint status of the individual depends on the resources otherwise available to the student during college.

To see the main prediction from the model, consider two individuals with exactly the same college lifetime income, W_1, but that one individual is borrowing constrained while the other is not. For the non-borrowing constrained student

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

while for the borrowing constrained student

$$\frac{\partial V_{1i}}{\partial f_i} = \frac{1}{f_i}.$$

Recall from above that the constraint binds when $f_i < (\delta W_1 + f_i)(1-\delta)$ in which case borrowing constrained students will be more sensitive to changes in f_i than will those that are not constrained. Furthermore, it is important to keep in mind that f_i consists of both the subsidy and of other resources given to the parents while in school. This means that even among families that are borrowing constrained, children from wealthier families will have higher values of f_i and thus lower values of $\frac{\partial V_{1i}}{\partial f_i}$. Thus the theory yields the following predictions:

1. All else equal, students from borrowing constrained families will be more sensitive to the subsidy than students who are not constrained.

2. All else equal, among students from borrowing constrained households, those with lower household resources will be more sensitive to the subsidy than those with more resources, as long as wealthier families give larger transfers to their children.

5 Empirical Approach

The goal of our work is to evaluate the effects of the reform. As should be clear from the model, a major complication of the work will be incorporating borrowing constraints. We showed in the empirical section that individuals who are borrowing constrained will be particularly sensitive to changes in schooling subsidies. In this paper, we evaluate the reform using both a difference-in-differences type of approach and a structural model. We demonstrate the close relationship between the two, and in particular, show that identification in the structural model is very closely linked to the difference-in-differences model. With this goal in mind, we begin with a very simple model and add parts until we reach the structural model.

From a glance at Figure 2, a natural simple estimation strategy appears. Prior to the reform some students were receiving large subsidies, but after the reform subsidies were the same across all groups. This has the simple prediction that we would expect to see a much larger effect of the reform on those individuals who were previously receiving low subsidies than those who were receiving high subsidies.

There are a number of different ways to implement such a difference-in-differences model, but we find the following most convenient. It is easier to think about identification of this model if the social income measure, X_i, was binary. Assuming a probit model, we could estimate the model

$$\Pr(C_i = 1) = \Phi(\beta_0 + \beta_1 S_i + \beta_2 R_i + \beta_3 X_i). \tag{2}$$

where C_i is a dummy variable indicating college enrollment, Φ is the c.d.f. of a Standard Normal random variable, S_i is the subsidy for which individual i is eligible, and R_i a reform dummy. In practice, we actually split R_i into a set of sub-dummies, one for each graduation cohort, but we suppress this for now. Before the reform, S_i would take two values depending on whether the income measure (X_i) was high or low. After the reform, it would take a third and higher value but be constant across individuals. The key parameter is β_1 which represents the response of college enrollment to a subsidy. Prior to the reform S_i was a function of X_i, so with only pre-reform data we can not separately identify β_1 from β_3. Furthermore, the reform dummy (R_i) is like a time dummy in a standard difference-in-differences framework. Thus β_1, is identified purely from the extent to which those who received a larger increase in stipend due to the reform responded more strongly than those who experienced the smallest increase.

In practice, X_i is not binary, and we have other variables on which to condition. Our base specification is

$$\Pr(C_i = 1) = \Phi(\beta_0 + \beta_1 S_i + \beta_2 R_i + \beta_3 S_i^{pre} + \beta_4 X_i + \beta_5' Z_i). \tag{3}$$

where S_i^{pre} is the pre-reform subsidy described in section 3, and Z_i is a column vector of other covariates such as age, gender, GPA, and indicator variables for the education of the parents. Recall that S_i^{pre} is just a deterministic function of X_i, so we now control for S_i^{pre} and a linear term X_i rather than just for X_i. To see why controlling for S_i^{pre} is essential for the method, consider a specification that only controlled for the linear term:

$$\Pr(C_i = 1) = \Phi(\beta_0 + \beta_1 S_i + \beta_2 R_i + \beta_3 X_i + \beta_4' Z_i).$$

Suppose we ran this probit prior to the reform. Since S_i is a nonlinear function of X_i, β_1 would be identified in this specification. This violates the spirit of the difference-in-differences approach. However, note that for cohorts making decisions prior to the reform, $S_i = S_i^{pre}$, so one could not separately identify β_1 from β_3 using only pre-reform data. Thus, identification of (3) requires cohorts making decisions both after and before the reform.

There is a major problem with this approach. Specification (3) imposes a number of assumptions. In addition to the linear index probit model, we make two implicit assumptions that are at odds with our model above. First, we make the standard difference-in-differences assumption that there is no interaction between social income (X_i) and the reform (R_i). Second, we assume that the effect of the subsidy (β_1) does not vary across individuals. The problem is that the model indicates that borrowing constrained individuals will tend to be more sensitive to subsidies than non-borrowing constrained individuals. Furthermore, within borrowing constrained families, those with lower family resources will tend to be more responsive. Going back to specification (2), ideally we would like to interact both S_i and R_i with X_i, but this is not feasible since that model is already saturated. We need to use a different methodology that can address this problem.

Our first approach is apparent from Figure 2. The form of the subsidy is far from smooth at the two kink points. By contrast, one would expect the relationship between X_i and borrowing constraints to be smooth near the kink points. In practice, one can implement this idea just by allowing an interaction between the reform and a smooth function of X_i. That is, one can estimate a model such as

$$\Pr(C_i = 1) = \Phi(\beta_0 + \beta_1 S_i + \beta_2 R_i + \beta_3 g_1(X_i) + \beta_4 S_i^{pre} + \beta_5 (R_i \times g_2(X_i)) + \beta_6' Z_i). \quad (4)$$

where g_1 and g_2 are smooth functions. In the appendix, we formalize this form of semi-parametric identification, showing that as long as g is continuously differential, the model is identified because S_i^{pre} is not smooth. We implement this idea by using a polynomial model for g.[13]

Our second approach is to make use of an indicator for having low liquid assets, D_i. As will be discussed later, we assume that those individuals who have high liquid assets are not borrowing constrained, while those with low liquid assets are potentially constrained. We can then look at the effects of the program on the non-constrained as well as examine the

[13] One could formalize this as a sieve estimator as the degree of the polynomial would increase with the sample size. We do not do this but rather just treat it as a flexible functional form.

importance of borrowing constraints by looking at the difference in the program on the two different groups. To see the intuition for identification, again consider the binary income case. We could estimate the fully interacted model

$$\begin{aligned}\Pr(C_i = 1) = \Phi(&\beta_0 + \beta_1 S_i + \beta_2 R_i + \beta_3 X_i + \beta_4 D_i + \beta_5 (D_i \times S_i) \\ &+\beta_6 (D_i \times R_i) + \beta_7 (D_i \times X_i))\end{aligned} \tag{5}$$

We have eight different groups and eight different parameters, so the model is just identified and the source of the identification is the same as in the simple model. That is, it is crucial that there is no interaction between time (R_i) and Income (X_i). In practice, we have a continuous model, and the base specification we use is the following:

$$\begin{aligned}\Pr(C_i = 1) =&\Phi(\beta_0 + \beta_1 S_i + \beta_2 R_i + \beta_3 X_i + \beta_4 S_i^{pre} + \beta_5 D_i + \beta_6 (D_i \times S_i) \\ &+\beta_7 (D_i \times R_i) + \beta_8 (D_i \times X_i) + \beta_9' Z_i).\end{aligned} \tag{6}$$

Before discussing the structural model, consider specification (5) in terms of the model we exposed in section 4. One insight from the model was that the borrowing constraint should bind more tightly for poorer families. This means that for borrowing constrained people the effect of the subsidy should vary with family income. However, for non-borrowing constrained it should not. Ideally we would want three treatment effects; one for those who are not borrowing constrained, a second for the high income borrowing constrained, and a third for the low income borrowing constrained. However, specification (5) does not allow for this. It allows for two "treatment effects," β_1 for individuals who are not borrowing constrained, and $\beta_1 + \beta_6$ for those who are. We would like to add a third possibility by including a $S_i \times D_i \times X_i$ interaction into the model. That would require nine different parameters, but we only have eight separate groups in the binary case, so the model would not be identified. However, we can identify the model under one additional assumption. As mentioned above, the key assumption justifying the difference-in-differences model is that the time trend does not vary with X_i. It seems completely reasonable to us to also assume that the time trend does not vary with D_i. In that case, in the difference-in-differences framework we could think of estimating the model

$$\begin{aligned}\Pr(C_i =1) = \Phi\,(&\beta_0 + \beta_1 S_i + \beta_2 R_i + \beta_3 X_i + \beta_4 D_i + \beta_5 (D_i \times S_i) \\ &+\beta_6 (D_i \times S_i \times R_i) + \beta_7 (D_i \times X_i)).\end{aligned} \tag{7}$$

Again, we have eight parameters and eight unknowns, and it is straightforward to show that this model is identified. Identification of this specification will turn out to be analogous to identification of the structural model.

The problem with specification (7) is that while it provides a clear description of the data, it is difficult to interpret the parameters. For example, the parameters themselves have little economic meaning, so it is difficult to say whether they are large or small. By using a structural model, we can present parameters that are interpretable and we can use the model to simulate policy counter-factuals. Of course, in doing so we are making stronger assumptions in that we are taking the model as true, so this represents the standard tradeoff of stronger assumptions but a more powerful model.

Next, we describe the econometric implementation of the structural model presented in section 4. To keep things simple, we specify the first period income as

$$f_i = S_i + \gamma_0 + \gamma_1 X_i,$$

where we allow students to have some other assets while young which can depend on family resources proxied by X_i. We assume that these assets are not schooling dependent; that is, the family will provide the resources to the children whether they go to college or not. 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. Given knowledge of W_{1i}, W_{0i}, f_i, and δ, one can calculate V_{0i} and V_{1i}.

We next assume that we can write the difference in non-pecuniary tastes across schooling levels as

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

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

$$\Phi\left(\frac{1}{\sigma_\varepsilon}\left[V_{1i} - V_{oi}\right] + T_i'\theta\right).$$

The difference-in-differences model (7) above was specified in accordance with our structural model which is very similar, but we would replace the three parameters β_1,β_5, and β_6 with the three parameters $\sigma_\varepsilon, \gamma_0$ and γ_1. The rest of the parameters would be analogous to the taste parameters θ so that we would estimate

$$\Pr(C_i = 1) = \Phi\left(\theta_0 + \frac{1}{\sigma_\varepsilon}[V_{1i} - V_{oi}] + \theta_1 R_i + \theta_2 X_i + \theta_3 D_i + \theta_4[D_i \times X_i]\right). \qquad (8)$$

Thus, identification of the structural model comes in virtually the same way as that for the difference-in-differences model. The advantage is that this model is easier to interpret and one can use the model for policy simulation.

In practice, we have continuous variables and we estimate a model that is analogous to (8), so we estimate the model

$$\Pr(C_i = 1) = \Phi\left(\theta_0 + \frac{1}{\sigma_\varepsilon}[V_{1i} - V_{0i}] + \theta_1 R_i + \theta_2 X_i + \theta_3 S_i^{pre} + \theta_4 D_i + \theta_5[D_i \times X_i] + \theta_6' Z_i\right). \qquad (9)$$

In the empirical analysis, we use the social income percentile instead of the money measure of social income, X_i, in order to discard the effect of real income growth over time.

6 Data

6.1 Data Source

We use a register-based data set covering the entire Danish population in the period 1983-2005. To this data set, we add information about educational event histories of individuals enrolled at educational institutions in the period 1973-2005. For the main part of the empirical analysis, we select a subsample consisting of high school students graduating in 1985-1990, before which period we do not observe assets. We use only individuals who graduate at "normal" ages, which we define as 19-20 years.[14] Furthermore, in order to get a homogenous sample of individuals with observed GPA, we select individuals who graduate from the ordinary high school track.[15] Finally, we need to exclude individuals where information about the parents is missing. The sample selection process is summarized in Table 2, where we also show means of some key variables for the excluded groups. This table shows that our sample tends to be slightly positively selected with respect to college enrollment one year after graduation.

[14] We do this to get a homogenous sample and to avoid interaction with the pre-reform means-testing age of 22 years. More than 80% of the students graduate at age 19-20 years.

[15] About 60% of the high school students attend the traditional academic track. The rest of the high school students attend the business track, technical track, or another high school equivalent education. For the latter group, we do not observe GPA.

We augment the pre-reform 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), and exclude individuals with divorced parents at the time of graduation as it is complicated to asses their eligibility status.

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. For individuals with self-employed parents at the time of high school graduation, accurate information about liquid assets over the observation period is not available. We choose to treat them as non-constrained as they are likely to have access to liquidity through their business. Our results are robust to this choice.

The resulting data set contains basic information about the individuals, grade point average from high school (GPA), their parental background, including the income measure needed for means-testing and the amount of liquid assets held at time of high school graduation.

6.2 Data Description

In Table 3, we present enrollment rates at college for each of the high school graduation cohorts 1983-1990 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 so within the first year of high school graduation (39% out of 73% for all cohorts). In the empirical analysis, we focus on accumulated enrollment one year after high school graduation so that the pre-reform cohorts' enrollment decisions occur prior to the announcement of the reform. It is important to keep this restriction of the analysis in mind. Strictly speaking, we cannot distinguish whether higher college enrollment within one year of graduation is due to a higher general enrollment rate or people enrolling earlier. However, this is what the data allows, and it is nevertheless a relevant policy parameter that we identify as policy makers are interested in inducing enrollment early after high school graduation.[16]

In the empirical analysis, we disregard the 1986 and 1987 cohorts. The reform of 1988

[16] If we ignore the issue of borrowing constraints the data allow us to assess the effect on accumulated enrollment two and three years after high school graduation as well. Doing this we get similar estimates, indicating that the focus on accumulated enrollment one year after high school graduation does not limit the interpretability of the analysis considerably.

was announced in time for the 1987 cohort 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 income variable X which was defined above) which was announced in time for cohort 1986 to adjust their behavior.

Table 3 indicates that the reform of 1988 has influenced enrollment since enrollment of cohorts graduating in years 1988-90 was systematically higher one year after high school graduation and onwards. However, we notice that mean enrollment varies substantially across the post-reform cohorts as well, indicating that it might be important to allow enrollment to vary flexibly across cohorts. In the empirical analysis, we tried to impose placebo reforms taking place either before or after the actual reform, and this experiment confirmed that the variation due to the actual reform is different from other cohort variation.

In Table 4, we present summary statistics by graduation year. Table 4 shows that the average high school graduation age is stable around 19.3 years, and that the average GPA is just above 8 and slightly increasing over the cohorts, which probably indicates grade inflation.[17]

To implement our model, we need to identify which students are potentially borrowing constrained. We adopt a measure of borrowing constraints developed by Zeldes (1989) and used on Danish data by Leth-Petersen (2006). 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, 2002), because it more accurately identifies households who are potentially constrained.

We construct a basic and an extreme indicator for being potentially borrowing con-

[17] At the relevant point in time, the Danish grade scale was 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.

strained: The basic indicator, D_1, takes the value one if parents' liquid assets fall short of one month's income, whereas the extreme indicator, D_2, takes the value one if the parents' liquid assets fall short of two months' income. However, for parents who are self-employed, the amount of liquid assets is not registered and we set $D_1 = 0$ and $D_2 = 0$.

In Table 4, we report the two low liquid asset indicators, D1 and D2. Liquid assets include all non-housing assets[18]; that is cash, shares, bonds, mortgage deeds, and other assets. Roughly 30% of the sample have liquid assets below one month's income, and roughly 40% have liquid assets below two months' income. Those are the parents we regard as potentially borrowing constrained.

In Table 5, we present the average composition of the parents' portfolio by high school graduation cohort. The portfolios are dominated by cash and other assets (such as yachts, cars, campers and other taxable assets). In Table 6, 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 lower proportion of their wealth in stocks, bonds and mortgage deeds, and a higher proportion in other assets. The parents who have liquid assets of less than one month's income, and thereby fall short of the basic split, hold as much as 83% of their wealth in other assets. The parents who have liquid assets of less than two months' income, and thereby fall short of the extreme split, hold 72% of their wealth in other assets. The least constrained group with D2=0 hold 34% in cash, only 33% in other assets and roughly 9% in mortgage deeds, 12% in bonds and 13% in shares. It seems reasonable to us that a group with this portfolio composition would not be borrowing constrained. In the empirical analysis, we try both D1 and D2 as measures of being potentially borrowing constrained. We report the results from using the basic split, D1, as this turns out to be more powerful than D2.

7 Empirical Results

As we stated in the introduction, 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. One reason is that aid is typically means-tested, and another reason is borrowing constraints. In the present set-up, a naïve regression

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

of college enrollment on stipend using pre-reform data gives a strong and significant *negative* impact of aid on enrollment.[19]

Before we turn to the results of estimation, we briefly discuss the evolvement of college enrollment one year after graduation for individuals with high vs. low income, and high vs. low liquid assets (according to the basic split, D1). This is shown in Table 7, and the purpose is to check if the pattern is consistent with the common trend assumptions as well as the anticipated findings from the estimations. We see that the trends in enrollment are very similar across groups before the reform. Furthermore, it is seen that the high income individuals experience a higher increase in enrollment one year after graduation than those with low income, and similarly, if we compare 1985 to 1989-90, we see that the individuals who are not borrowing constrained experience a slightly higher increase in enrollment than those who are potentially constrained. This is exactly the variation induced by the reform which we will exploit when we use the difference-in-differences approach.

The remainder of our discussion of the results follows our discussion of our empirical approach in section 5 very closely. The first specification is analogous to the simple difference-in-differences approach described in equation (3). In column (1) no control variables are included, whereas the full specification is presented in column (2). The inclusion of control variables is not vital for the results. Note that we do not present the probit coefficients but rather the average derivates so that the results are interpretable. One can see from the table that the effect of the stipend is statistically significant in both specifications. The stipend is defined as the fraction of the full stipend. That is, an individual would have a value of $S_i = 1$ if he was eligible for the full stipend. Since the full stipend represents approximately \$6,000 per year, the coefficient on stipend in column (2) implies that the effect of a \$1,000 subsidy would be to change enrollment by 1.35%, and the confidence interval around this value is tight. This is a substantially smaller effect than the magnitude typically found in the U.S. To check the common trend assumption behind the difference-in-differences approach, we superimpose a "pseudo reform" on pre-reform and post-reform data, respectively. In these cases, the effect of the stipend becomes very close to zero, and thus these tests do not compromise our conclusions.

Our next goal is to account for borrowing constraints by taking advantage of the kink

[19] Adding a control for family income drives the coefficient to zero, while adding all available controls makes the coefficient significantly positive and of similar size, as we will see later.

in the subsidy amount. In particular, we implement the idea in specification (4) by controlling for smooth functions of family income percentile and interacting it with the reform. We start with the simplest expression by taking these functions (g_1 and g_2) to be linear. The point estimates work as one might expect. The coefficient on the interaction between income percentile and the reform is negative, which implies that the reform has a stronger effect on students from poorer families. Further, this leads the coefficient on the stipend to increase substantially although the overall effect is still quite small compared to the previous literature. However, note that the key interaction term is not statistically significant and the estimate of the main effect (coefficient on Stipend) has become considerably less precise. The coefficient on Stipend is still statistically significant, but the confidence interval now includes considerably higher values. However, the fact that the interaction is not statistically significant and the fact that the point estimate only increases by about 50% suggest that while there might be bias in the first approach, it is not huge. Our confidence interval can still rule out effects of the size typically found in the United States. Since the stipend rule also depends on age, we now interact our linear functions with age. The point estimate falls slightly, but the standard error rises more. Next, we add a quadratic term (interacted with the relevant variable). The point estimates fall somewhat more, but the precision also worsens. We experimented with higher order terms and found even less precise results. We find this exercise useful because the results at least suggest that our basic analysis is not far from off. That is, the fact that the interaction between the income percentile and the reform dummy is not statistically significant suggests that the borrowing constraint problem is not severe. However, nothing here is conclusive given the size of the standard errors using only two terms in the polynomial. Thus, we turn to our second approach.

We first estimate the highly interacted equation (6) in Table 9, column(1). The point estimates go the opposite of what one would expect. The coefficient on the interaction between the stipend and the low asset indicator is actually negative, suggesting that the stipend actually has a smaller effect on those likely to be borrowing constrained rather than a positive effect as one might expect. However, this negative interaction is not statistically significant. Once again, the results suggest relatively small effects of the subsidy (that is relative to the previous literature), and they also suggest that borrowing constraints do not appear to be important.

The specification (7) is presented in column (2) and is closer to what the model predicts.

We find a marginal effect of 0.095 overall for the non-borrowing constrained individuals. The effect for borrowing constrained individuals can be seen from the interaction between the stipend and low assets as well as the interaction between the stipend, low assets, and family income. For low liquid asset/low income families, the effect of the stipend is substantially higher. For example, for a low asset family at the tenth income percentile the effect is

$$0.095 + 0.086 - 0.125 \times 0.1 = 0.181$$

which is substantially larger than the main result. To get a better sense of whether these effects are large or not, we use our structural model.

The structural model is a nonlinear probit model represented in equation (9), and estimation by maximum likelihood is straightforward. We estimate the model assuming that students from families with low liquid asset holdings are potentially borrowing constrained. In doing so, we control for family income, the interaction between family income and low liquid assets, and for graduation cohorts. In this sense, identification is analogous to the difference-in-differences models presented above. The results from the structural model are presented in Table 10. While not guaranteed, one can see that the $1/\sigma_\varepsilon$ parameter is positive and statistically significant. Looking at the form of the probit model above, this results because our prediction of $V_{1i} - V_{0i}$ does predict college enrollment.[20] This is the key parameter in the model, and we find it to be generally robust across different specifications of the model.

The main advantage of estimating the structural model is that we can use it to simulate various policy counterfactuals. In Table 11, 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 11 corresponds to the current subsidy level. Looking at the columns to the right, one can see that relaxing the borrowing constraint would lead to only a slight increase in college going rates. Even for the low income potentially constrained, eliminating the constraint would only increase education levels from 33.3% to 33.7%. However, the subsidy itself is important. Completely eliminating the subsidy would reduce enrollment among all groups by more than 7 percentage points. One

[20] To evaluate the utility of going to college, V_{1i},we compute the present value of earnings of college-goers as the average present value of observed earnings of college-goers rather than trying to get a measure of the casual return to college. A similar comment holds for V_{0i}. This makes the model simple to estimate, but it means that ability bias may be present.

should of course keep in mind that this is a very large subsidy so this is not surprising.[21] This effect is a little smaller than the effects estimated in the reduced form analysis and substantially smaller than the ones estimated in the previous literature. An interesting result is the importance of the borrowing constraint in the absence of the subsidy. In this case, eliminating borrowing constraints would raise education levels for the low income potentially constrained from 25.6% to 27.1%. This is a large effect relative to the enrollment rate and relative to the change for the current subsidy level, but it is still not huge.

One can also see in Table 11 that if we were to double the stipend one would see a substantial increase in enrollment for all groups and a complete elimination of the importance of borrowing constraints.

7.1 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). To derive the change in demand from changes in observed college enrollment and attainment, we need to condition our results on assumptions about supply. The existing literature has, 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 is 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.[22] 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 determines whether the average GPA of

[21] The yearly stipend corresponds to 48,968 DKK (2001 prices) or approximately $6,000 US dollars using the exchange rate at that time.

[22] A (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.

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 than our estimation sample, 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 seems to be an 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 Table 4. 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 changes in other moments in the distribution of high school GPA. In Figure 4, 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 these figures in mind, we are more confident in directly linking changes in observed enrollment to changes in demand for education.

8 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 studying 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 1.35 percentage points, which is a somewhat 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 show that borrowing constraints do not appear to be particularly important in Denmark at this time.

A Appendix: A Regression Kink Design

We control for the parents' financial situation by including their position in the social income distribution. If we believe that, for instance, borrowing constraints play a significant role in the schooling decision, it would be misleading to restrict the responsiveness of stipend to be constant across individuals. The variation in stipend over time is a direct function of parental income, and therefore strong functional form assumptions are needed to allow the responsiveness to stipend to vary across income. However, the means-testing algorithm provides us with a kink in the relationship between parents' income and the grant eligible for. This kink could be exploited for non-parametric identification.

We choose a specification which is simpler than model (4), which we implement, but which makes the problem clearer. We consider the following model in which Y_i represents a generic dependent variable,

$$Y_i = \beta_1 S_i + g(X_i) + u_i$$

where X_i is a continuous variable, and S is the formula determining the subsidy which contains a kink at $X_i = x^*$.

Our goal here is to show that this model is semiparametrically identified. That is, we can leave g completely unrestricted (other than imposing smoothness) and show that β_1 is identified. There are many ways one can estimate the model after showing it is identified. The simplest possibility is to just estimate the model as a regression with a flexible functional form. A natural choice would be to use a sieve estimator in which the number of terms approximating g gets large with the sample size. In this paper, we simply experimented with alternative polynomial models.

The main issue for identification is that S_i is completely determined 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 $E(u_i \mid X_i)$. We assume that $g(X_i, \cdot)$ and

$E(u_i \mid X_i)$ are continuous differentiable. Define d_0 and d_1 such that

$$
\begin{aligned}
d_0 &\equiv \lim_{x \uparrow x^*} \frac{\partial S(x)}{\partial x} \\
d_1 &\equiv \lim_{x \downarrow x^*} \frac{\partial S(x)}{\partial x},
\end{aligned}
$$

we assume that

$$d_0 \neq d_1.$$

Then

$$
\begin{aligned}
& \frac{\lim_{x \downarrow x^*} \frac{\partial E(Y_i|X_i=x)}{\partial x} - \lim_{x \uparrow x^*} \frac{\partial E(Y_i|X_i=x)}{\partial x}}{d_1 - d_0} \\
= & \frac{\lim_{x \downarrow x^*} \left(\beta_1 \frac{\partial S(x)}{\partial x} + \frac{\partial g(x)}{\partial x} + \frac{\partial E(u_i|X_i=x)}{\partial x}\right)}{d_1 - d_0} \\
& - \frac{\lim_{x \uparrow x^*} \left(\beta_1 \frac{\partial S(x)}{\partial x} + \frac{\partial g(x)}{\partial x} + \frac{\partial E(u_i|X_i=x)}{\partial x}\right)}{d_1 - d_0} \\
= & \beta_1
\end{aligned}
$$

Hence, this parameter is identified. In practice, one must use more parameterized models to obtain reasonable precision. Extending this idea to specification (4) in the text is straightforward.

This identification argument resembles the regression discontinuity idea. Instead of a discontinuity in the level of the stipend-income function, we have a discontinuity in the slope of the function. Rothstein and Rouse (2007) apply a similar approach in estimating the effect of student debt on post-graduation behavior.

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Table 1

Overview of Higher Education Programs in Denmark

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.
Medium-cycle higher education	3 to 4 years	Professional programs like teacher training programs, programs in social work, journalism, nursing, engineering etc. and research-based Bachelor programs.
Long-cycle higher education	5 to 6 years	The master programs. The programs qualify students for occupational functions and scientific work.

Table 2

Overview of the Sample Selection Process

Sub sample	Observations	Male	Age	College enrollment after one year
All (prior to selection)	206,465	0.417	19.37	0.339
GPA registered	165,547	0.405	19.28	0.388
GPA missing	40,918	0.465	19.70	0.140
Graduation age 17	43	0.465	17.00	0.512
Graduation age 18	13,209	0.356	18.00	0.443
Graduation age 19	113,096	0.411	19.00	0.370
Graduation age 20	70,835	0.435	20.00	0.279
Graduation age 21	9,282	0.441	21.00	0.269
Parents' income registered	156,693	0.417	19.36	0.344
Parents' income missing	49,772	0.416	19.40	0.325
Parents' education registered	181,919	0.416	19.36	0.340
Parents' education missing	24,546	0.422	19.43	0.335
Final sample	108,933	0.409	19.33	0.390

Table 3

College Enrollment, Year X after Graduation

High school graduation cohort	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5	Nobs
1983	0.206	0.407	0.548	0.636	0.683	0.709	12,710
1984	0.192	0.379	0.528	0.612	0.667	0.695	14,028
1985	0.160	0.363	0.522	0.617	0.664	0.692	14,686
1986	0.153	0.358	0.528	0.621	0.672	0.700	14,328
1987	0.144	0.365	0.542	0.649	0.703	0.732	13,008
1988	0.144	0.368	0.559	0.667	0.721	0.753	12,798
1989	0.159	0.431	0.612	0.701	0.752	0.781	13,119
1990	0.188	0.451	0.617	0.710	0.759	0.788	14,256
All cohorts	0.168	0.390	0.557	0.652	0.702	0.730	108,933

Table 4

Means of Central Variables by High School Graduation Cohort

	High School Graduation Cohort					
	1985	1986	1987	1988	1989	1990
High school graduation age (years)	19.29	19.33	19.35	19.35	19.33	19.31
Male	0.411	0.397	0.413	0.409	0.408	0.408
GPA	8.01	8.06	8.10	8.15	8.20	8.23
Mother's education category						
Cat. 0 (basic school)	0.359	0.336	0.312	0.298	0.277	0.247
Cat. 1 (high school or similar)	0.016	0.016	0.017	0.018	0.019	0.021
Cat. 2 (vocational education)	0.366	0.382	0.382	0.386	0.385	0.397
Cat. 3 (short cycle higher education)	0.043	0.042	0.046	0.045	0.049	0.054
Cat. 4 (medium cycle higher education)	0.192	0.198	0.216	0.222	0.235	0.244
Cat. 5 (long cycle higher education)	0.025	0.026	0.028	0.031	0.035	0.038
Father's education category						
Cat. 0 (basic school)	0.250	0.243	0.233	0.222	0.218	0.204
Cat. 1 (high school or similar)	0.019	0.018	0.021	0.022	0.025	0.026
Cat. 2 (vocational education)	0.400	0.403	0.389	0.390	0.380	0.375
Cat. 3 (short cycle higher education)	0.043	0.044	0.049	0.048	0.049	0.051
Cat. 4 (medium cycle higher education)	0.176	0.178	0.189	0.191	0.196	0.207
Cat. 5 (long cycle higher education)	0.111	0.114	0.119	0.127	0.132	0.137
Living in a large municipality	0.339	0.338	0.334	0.335	0.336	0.334
Living in one of the four largest municipalities	0.313	0.306	0.300	0.290	0.294	0.281
D1: Parents' liquid assets < one month's inc.*	0.280	0.298	0.284	0.189	0.211	0.248
D2: Parents' liquid assets < two month's inc.*	0.391	0.399	0.385	0.296	0.332	0.368
At least one of the parents are self-employed	0.215	0.219	0.209	0.227	0.226	0.211
Number of observations	14,686	14,328	13,008	12,798	13,119	14,256

* If parents are self-employed we impose D=0

Table 5
Portfolio Composition by High School Graduation Cohort

	High school graduation cohort					
	1985	1986	1987	1988	1989	1990
Average proportion of portfolio in						
Cash	0.279	0.264	0.281	0.316	0.273	0.268
Other	0.501	0.524	0.504	0.398	0.455	0.474
Mortgage deeds	0.078	0.068	0.058	0.066	0.059	0.054
Bonds	0.079	0.072	0.065	0.099	0.092	0.083
Shares	0.056	0.073	0.095	0.121	0.121	0.121
Low Liquid Assets Indicators						
D1	0.280	0.298	0.284	0.189	0.211	0.248
D2	0.391	0.399	0.385	0.296	0.332	0.368

Table 6
Portfolio Composition by Low Liquid Asset Split: Basic vs. Extreme

	D1=1	D1=0	D2=1	D2=0
Average proportion of portfolio in				
Cash	0.097	0.338	0.174	0.340
Other	0.832	0.363	0.724	0.334
Mortgage deeds	0.012	0.080	0.024	0.086
Bonds	0.014	0.104	0.024	0.115
Shares	0.045	0.116	0.054	0.125
Observations with observed liquid assets	17,576	56,102	26,626	47,052

Table 7
College Enrollment, Year 1 after High School Graduation

High School	Low Income			High Income		
Graduation Cohort	All	D1=0	D1=1	All	D1=0	D1=1
1983	0.375			0.443		
1984	0.344			0.417		
1985	0.323	0.328	0.320	0.397	0.400	0.392
1986	0.315	0.323	0.310	0.402	0.408	0.396
1987	0.310	0.310	0.311	0.415	0.413	0.416
1988	0.322	0.331	0.297	0.413	0.421	0.375
1989	0.368	0.374	0.354	0.481	0.485	0.463
1990	0.389	0.398	0.372	0.502	0.512	0.469

Table 8

Probit Model for College Enrollment, Cohorts 1985, 1988-1990

Marginal Effects on College Enrollment

(Standard errors in parentheses)

	(1)	(2)	(3)	(4)	(5)
Stipend	0.067*	0.082*	0.143*	0.115*	0.091
	(0.016)	(0.017)	(0.043)	(0.050)	(0.066)
Pre-Reform Stipend	-0.072*	-0.005	-0.036	0.019	0.018
	(0.022)	(0.025)	(0.032)	(0.036)	(0.050)
Income Percentile	0.123*	0.043*	0.067*	0.101*	0.040
	(0.015)	(0.017)	(0.023)	(0.028)	(0.076)
Income Percentile × (Age=20)				-0.051*	-0.018
				(0.017)	(0.063)
Income Percentile Squared					0.041
					(0.050)
Income Percentile Squared × (Age=20)					-0.023
					(0.064)
Income Percentile × Reform Dummy			-0.049	-0.024	0.021
			(0.031)	(0.038)	(0.104)
Income Percentile × (Age=20)				-0.017	0.008
× Reform Dummy				(0.016)	(0.055)
Income Percentile Squared					-0.024
× Reform Dummy					(0.067)
Income Percentile Squared × (Age=20)					-0.040
× Reform Dummy					(0.070)
High School GPA included	no	yes	yes	yes	yes
Sex, age and geographical indicators	no	yes	yes	yes	yes
Indicators for graduation cohort	yes	yes	yes	yes	yes
Controls for parents' education	no	yes	yes	yes	yes
Log-likelihood	-54377	-48,967	-48,965	-48,954	-48,953
Number of observations	81,581	81,581	81,581	81,581	81,581

Note: * indicate significance at a 5%-level.

Table 9

Probit Model for College Enrollment Using Controls for Borrowing Constraints

Cohorts 1985, 1988-1990

Marginal Effects on College Enrollment

(Standard errors in parentheses)

	(1)	(2)
Stipend	0.114*	0.095*
	(0.032)	(0.026)
Income Percentile	0.037	0.033
	(0.022)	(0.021)
Pre-Reform Stipend	-0.011	-0.022
	(0.030)	(0.030)
Low assets (0/1)	0.027	-0.074
	(0.027)	(0.042)
Stipend × Low assets	-0.077	0.086*
	(0.049)	(0.038)
Reform × Low assets	0.049	
	(0.032)	
Stipend × Income Percentile × Low assets		-0.125*
		(0.060)
Income Percentile × Low assets	-0.017	0.087
	(0.020)	(0.062)
High School GPA included	yes	yes
Sex, age and geographical indicators included	yes	yes
Indicators for graduation cohort included	yes	yes
Controls for parents' education included	yes	yes
Log-likelihood	-32,613	-32,613
Number of observations	54,843	54,843

Note: * indicate significance at a 5%-level.

Table 10
Estimates from Structural Model
(Standard errors in parentheses)

$\frac{1}{\sigma_\varepsilon}$	1.881*
	(0.635)
Parameters determining f_i (i.e. γ_0 and γ_1)	
Intercept	6.787
	(3.246)
Income Percentile	1.571
	(4.915)
Taste Parameters (i.e. θ)	
Intercept	-4.889*
	(0.299)
Pre-reform Stipend	-0.031
	(0.078)
Income Percentile	0.110
	(0.076)
Low Assets	0.016
	(0.089)
Lows Assets × Income Percentile	-0.069
	(0.121)
GPA	0.501*
	(0.006)
Male	0.184*
	(0.012)
Age 19 at HS Graduation	0.003
	(0.014)
Parent's Education Dummies	Yes
Geographical Indicators	Yes
Indicators for graduation cohort	Yes
Mean log-likelihood	-0.594678
Number of observations	54,843

Note: * indicate significance at a 5%-level.

Table 11

Policy Simulations from Structural Model

	Enrollment rates			Effect of removing constraints		
	High Assets	Low Assets		Low Assets		
		Low Inc.	High Inc.	All	Low Inc.	High Inc.
No stipend	0.356	0.256	0.395	0.010	0.015	0.003
Actual stipend	0.426	0.333	0.470	0.002	0.004	0.000
Double stipend	0.498	0.407	0.542	0.000	0.000	0.000

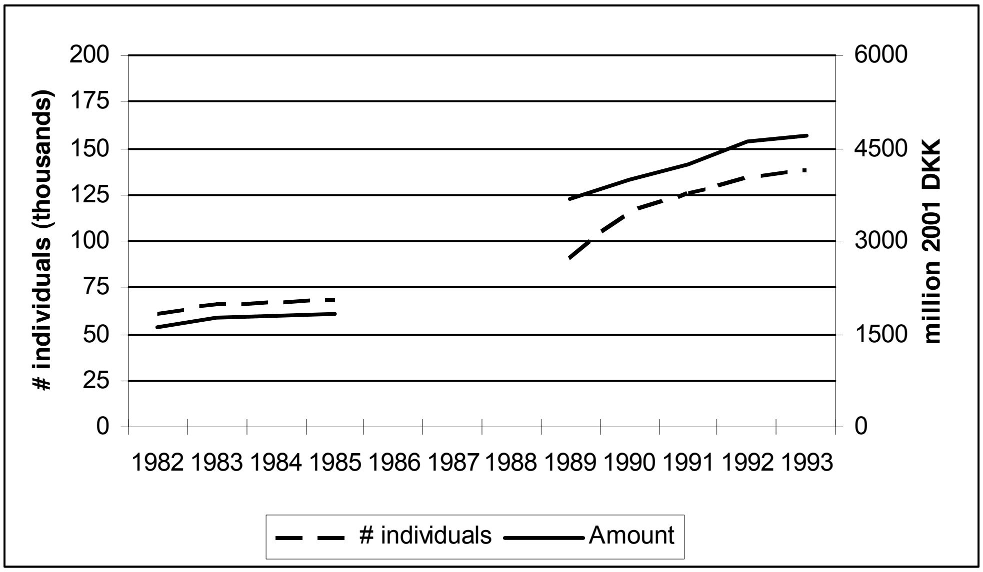

Figure 1: Government Grants Awarded (Source: Statistical Yearbooks 1984-96)

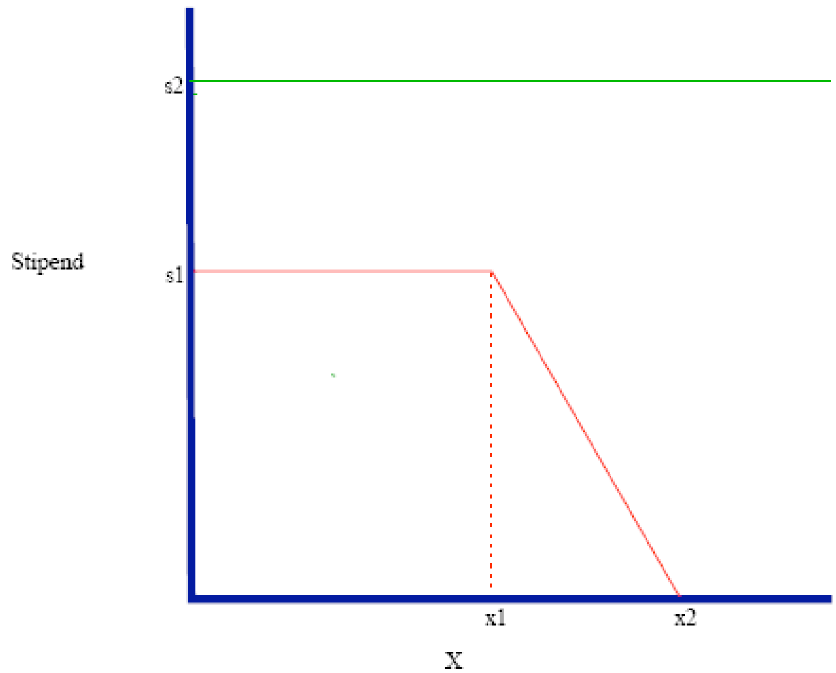

Figure 2: Illustration of the Influence of the Reform on the Stipend.

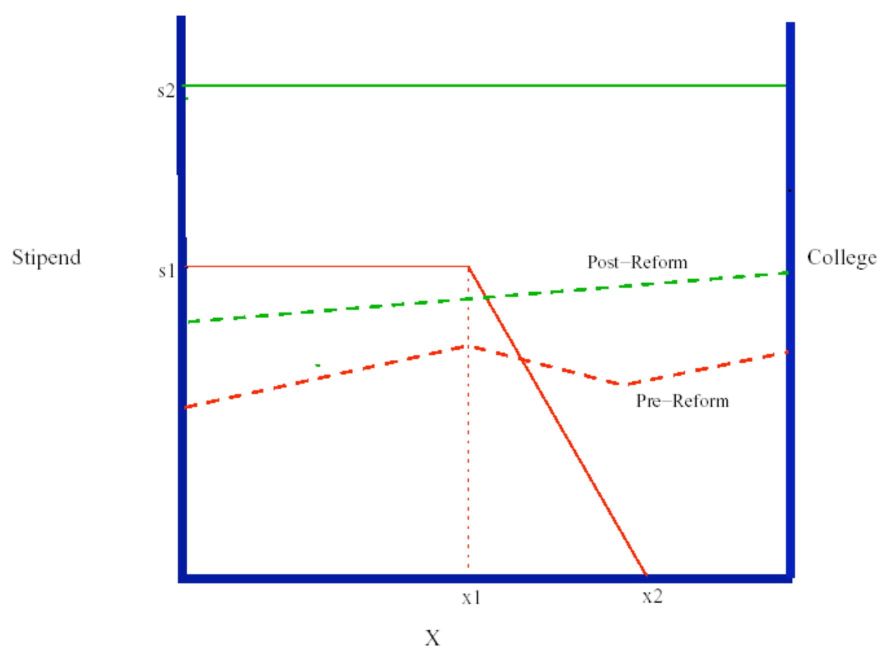

Figure 3: The Stipend Rule and It's Relation to Enrollment.

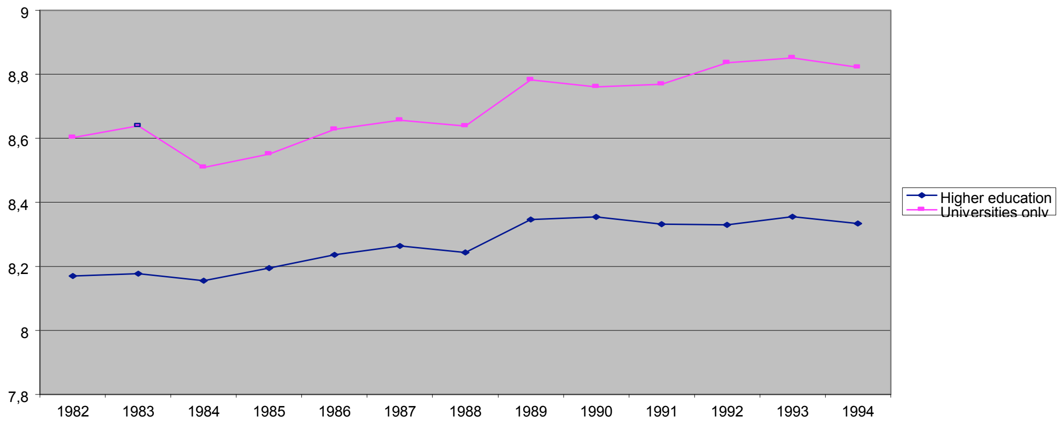

Figure 4: High school GPA of First-Year Students.

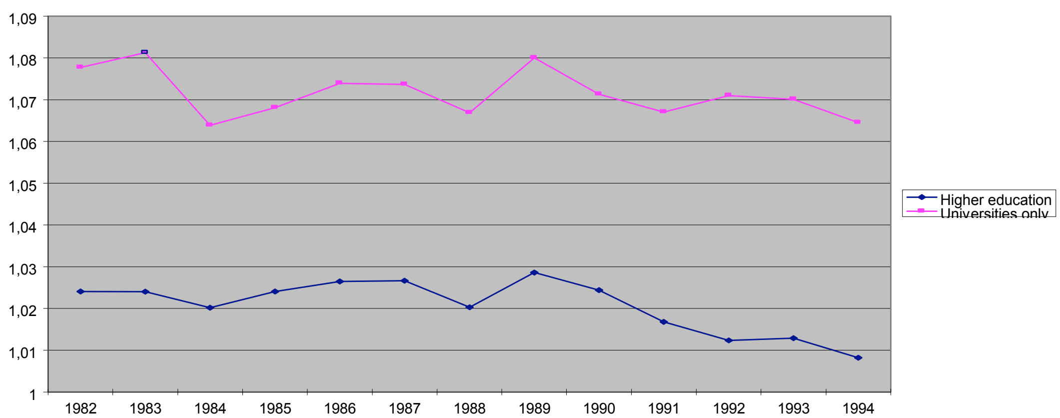

Figure 5: High School GPA of First-Year Students Relative to High School Cohort Average.