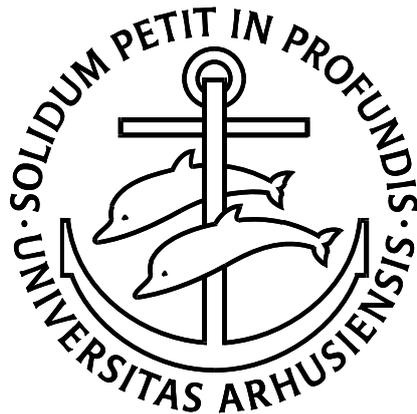


SCHOOL OF ECONOMICS AND MANAGEMENT

ECONOMICS WORKING PAPER 2009-2

**The Effects of Financial Aid in High School on
Academic and Labor Market Outcomes: A Quasi-
Experimental Study**

Maria Knoth Humlum and Rune Majlund Vejlin



UNIVERSITY OF AARHUS

BUILDING 1322 - 8000 AARHUS C - DENMARK ☎ +45 8942 1133fa

The Effects of Financial Aid in High School on Academic and Labor Market Outcomes: A Quasi-Experimental Study*

Maria Knoth Humlum[†] Rune Majlund Vejlin[‡]

Abstract

We investigate the effects of financial aid on student employment and academic outcomes in high school. We exploit administrative differences in the amount of financial aid received based on timing of birth to identify the causal effects of interest. Specifically, individuals born early in a quarter receive less financial aid than comparable individuals born late in the previous quarter. We find that receiving less aid induces individuals to work more during high school. However, we do not find any evidence that receiving less financial aid and thereby working more is associated with any adverse outcomes, such as a lower high school grade point average.

JEL Classification: I28, J22, J24

Keywords: student grants, high school employment, regression discontinuity

*Maria Knoth Humlum is grateful for financial support from the Danish Strategic Research Council Project: ‘Intergenerational Transmission of Human Capital’. We have benefited from constructive comments from participants at the DGPE 2008 workshop and seminar participants at Aarhus University and AKF. In addition, we would like to thank Helena Skyt Nielsen and Michael Svarer for their helpful suggestions. The usual disclaimer applies.

[†]School of Economics and Management, Aarhus University, Building 1322, Bartholins Allé 10, DK-8000 Aarhus C, Denmark. Email: mhumlum@econ.au.dk.

[‡]School of Economics and Management, Aarhus University, Building 1322, Bartholins Allé 10, DK-8000 Aarhus C, Denmark. Email: rvejlin@econ.au.dk.

1 Introduction

Understanding the effects of financial aid on the behavior of students - both in and outside school - is essential for designing effective financial aid policies. At the college level, it is common for students to receive some form of financial aid. The literature on the effects of financial aid in college has focused mainly on college enrollment and college completion, see e.g. Dynarski (2003) and DesJardins (2002). While college enrollment and performance in college is obviously important, perhaps even more so is high school completion and performance in high school. Carneiro and Heckman (2003) find that parental income does not limit college enrollment directly, only indirectly through the impact that parental income has on a child's stock of human capital at the time the decision of college enrollment is made. Cunha et al. (2006) and Cunha and Heckman (2006) further emphasize this point and suggest that the technology of human capital accumulation is characterized by self-productivity and dynamic complementarity of human capital investments. As a consequence, high school performance will be an important determinant of college enrollment and performance. Recently, in the US, a conditional cash transfer program labeled 'Opportunity NYC' was initiated in some of the poor neighborhoods in New York that included a component that aimed to induce youths to complete a high school education, see Morais de Sa e Silva (2008). While the idea of giving money to high school students for attending school constitutes something of a novelty in the US, in the Scandinavian countries it has been common for years to provide financial aid at the upper secondary education level in the form of student grants. The aim of this paper is to estimate the effects of these grants on high school students' academic and labor market outcomes.

The identification of causal effects is an important issue in the literature on the effects of financial aid, see Dynarski (2003). Financial aid is often correlated with observed and unobserved characteristics, such as parental income and ability, and therefore the causal effects of financial aid can be hard to determine. The primary contribution of this paper is to provide valid evidence on the causal relation between student grants and students' academic and labor market outcomes. Contrary to earlier studies, we consider the effects of financial aid at the high school level. The novel feature of our analysis is that the rules governing the payout of the student grants have

created exogenous variation in student grants based on time of birth. Specifically, individuals who are born early in a quarter receive a lower grant in the first year of eligibility than comparable individuals born late in the previous quarter. The mean difference in the grant received by the control and the treatment groups in the year of the 18th birthday is about \$560 which is a sizable amount, also when it is measured as a percentage of the total grant received during high school. We use a regression discontinuity design to identify the causal effect of student grants.

The literature on financial aid focuses on education at the college level, probably because in many countries, and in particular in the US, financial aid for students at other levels of education is uncommon. In Denmark, as well as in other Scandinavian countries, all high school students are entitled to public subsidies in the form of monthly student grants starting from around the time they turn 18. Since Danish high school students to some extent can be compared with first-year US college students, the findings of this paper could potentially be of relevance to this group as well. Individuals attending high school in Denmark are typically 16-19 years old, but only students aged 18 and above are eligible for student grants. These ages correspond roughly to the age of typical US college students in the first years of college. Another similarity is that the majority of Danish high school students live with their parents which is also the case for about 43 percent of all first-year college students in the US.¹

Most studies that investigate the effects of financial aid in college have focused on the effects on college enrollment (e.g., Dynarski, 2003, van der Klaauw, 2002, Nielsen et al., 2008), or college completion (Stinebrickner and Stinebrickner, 2007, DesJardins et al., 2002, Arendt, 2008). A \$1,000 increase in financial aid during college has been shown to be associated with an increase in college enrollment in the range of a couple of percentage points (see, e.g. Dynarski, 2003, Nielsen et al., 2008). Dynarski (2003) points out that the amount of aid received is typically correlated with various background characteristics (observed and unobserved) that also affect college attendance. Thus, to identify the causal effect of aid, exogenous variation in aid is essential. She considers a shift in a US aid policy that only affects some students. She finds that the change in aid policy reduced the probability of attending college, and a \$1,000 increase in aid increases the probability of college attendance by about 3.6 percent. In a Danish setting, Nielsen et al. (2008) investigate

¹71 percent of all first-year college students in the US were aged 19 or below in 1995-1996, King (2002).

the effects of student grants on college enrollment using variation in student grants stemming from a big reform in Denmark in 1988. They find that a \$1,000 increase in aid increases the probability of college attendance by about 1.4 percent. As they mention, this estimate is lower than those generally found in the literature, but they attribute this mainly to the presence of other large subsidies in Denmark. Using a regression discontinuity design, van der Klaauw (2002) estimates the effect of financial aid offers on a student's decision to enroll in college. The enrollment elasticity with respect to college grants is 0.86 for those who applied for financial aid, and 0.13 for those who were ineligible for financial aid. He also finds that OLS estimates were biased and very sensitive to the choice of included covariates.

The effect of financial aid on the decision to drop out of college has also been the subject of investigation. Stinebrickner and Stinebrickner (2007) examine the effect of credit constraints on the decision to drop out of college. Their sample consists of students from low-income families at a particular US college, and yet they find that the main part of the attrition during college is motivated by other issues. DesJardins et al. (2002) use duration-type models to look at students' college drop-out decisions. They focus on how the features of the financial aid package may affect students' decisions and conclude that changing loans to scholarships has a negative effect on the probability of dropping out. In a Danish context, Arendt (2008) uses the same reform as Nielsen et al. (2008) and estimates the effect of the reform on time-to-drop-out and time-to-completion using discrete duration models. He finds that higher student grants decrease drop-out rates, but he finds no effects on completion rates.

Based on the fact that financial aid has been shown to have effects on students' behavior in college, we also expect the financial aid that students receive in high school to affect their decision process. If a student receives less financial aid, one obvious way for the student to compensate for this lower income is to increase his labor supply. Therefore, we would expect to see direct effects of financial aid on the labor market behavior of students. Given that grants affect labor market behavior, the literature on the effects of high school employment on academic performance suggests that there might also be effects of grants on academic performance. There is some disagreement about the size and the direction of the effects of employment during high school on

academic performance; generally, though, the literature suggests that the effects are small and negative (see survey in Rothstein, 2007). High school employment may be beneficial to students if it provides them with valuable experiences in the labor market and knowledge of the world of work. Employment could also enhance skills of students which are valued in the education system such as discipline. On the other hand, high school employment may be detrimental to students if it has adverse effects on students' performance in school, e.g. by crowding out study time.

In the empirical analysis, we use information on the entire Danish 1979-1986 birth cohorts from the Danish registers. The data set includes detailed information on educational paths, academic outcomes, the grant received, parental background, and the exact date of birth of the students. We define measures of employment based on monthly indicators of labor market participation and yearly wage measures. We find that decreasing the grant by approximately \$560 increases the probability of working by two-three percentage points. It increases wage income by about \$125. In addition, although we find no significant effects on high school GPA and other measures of academic performance such as high school completion and college enrollment, the fact that our estimates are very precise implies that we can reject that a lowering of the grant is associated with any adverse effects on these outcomes.

The structure of the paper is as follows. In section 2, the high school student grant system in Denmark and the specific feature of the system that we will use as part of our identification strategy are described. In section 3, we describe our empirical approach. Section 4 describes the data used, and in section 5 we present a graphical analysis of the regression discontinuity and our estimation results. In section 6, we perform a few robustness checks to validate our results. Section 7 concludes.

2 The Grant System for High School Students in Denmark

This section gives a brief introduction to the grant system in Denmark with particular emphasis on the system and the payout scheme as it was in 1996 to 2004, which is the period that we focus on in the empirical analysis. High school students receive financial aid in the form of a monthly grant when they turn 18. In Denmark there are two major branches of upper secondary education:

vocational and high school educations.² Here, the focus will be on the high school educations, although the grant system is basically same for the vocational educations. However, it turns out to be convenient to restrict attention to high school students in order to obtain homogenous outcome measures.

2.1 The Grant System

In Denmark high school students receive student grants from the State Education Fund from the quarter after they turn 18. The main purpose of these grants is to ensure that it is the interests and abilities of young people that determine educational choices and not their economic background. Expenses to the State Education Fund made up about a third of the expenses in the Danish Ministry of Education in 2005 (Statistics Denmark, 2008).

One of the major changes in the grant system occurred in 1996 when a basic grant was introduced to all 18-year-old students. Prior to the reform the grant received by 18-year-olds was means-tested against parental income, and many 18-year-olds received no grant at all. Grants to 19-year-olds were not means-tested prior to the reform, but after the reform 18-year-olds and 19-year-olds were treated the same way in the student grant system. Everybody now receives a basic grant and a means-tested supplement. The reform in 1996 meant that the number of recipients of grants at the upper secondary education level increased substantially. Another major change occurred in the spring of 2004 when the basic grant was reduced by about 30 %. However, on this occasion, the means-tested supplement was increased such that individuals with adjusted parental income below a certain level were unaffected by the reform.³ We select our sample such that these reforms do not bias our results.

In order to limit the extent of work during school, a limit is set on a student's own income, implying that he will have to pay back the student grant in case his income exceeds this limit. The limits are the same for high school students and students in higher education. In 2001, the limit

²We use the term 'vocational education' to cover the traditional vocational educations such as craftsmen etc. The term high school education encompasses all of the standard high school educations. Upper secondary education encompasses both of these terms.

³'Adjusted parental income' is a term used in the Danish grant system, and it depends primarily on parental income and the number of siblings below the age of 18.

was approximately 5,226 DKK (about \$630) in months where the student received a grant and 13,050 DKK (about \$1,570) in months where no grant was received. These limits are so high that the number of high school students affected is relatively small. The size of the basic monthly grant was in the order of 1,252 DKK in 2000 which corresponds to about \$155. In addition, individuals with adjusted parental income below a certain level received a supplement. Individuals who were living away from their parents received a higher basic rate.⁴

2.2 The Payout Scheme

The rules governing payout of grants create differential incentives for work and study among high school students. According to these rules an individual is entitled to student grants from the quarter following the quarter in which he turns 18. Student grants are paid out monthly. They are paid out in advance; i.e. the grant for June will be paid out on the last banking day of May. In practice this implies that an individual born April 1st must wait for about three months before getting his first payout - the grant for July - while an individual who is born March 31st will not have to wait at all, and will receive grants for April, May, and June. Given that some individuals are faced with a 'dry' period of up to three months after their 18th birthday, depending on the date of birth, before beginning to receive the monthly grant, these individuals have a greater incentive to look for alternative financing of their consumption than individuals who do not have to wait for their first monthly grant. For high school students, the two primary sources of financing besides the student grants are likely to be parental transfers and labor market work. If individuals are more likely to hold on to a job once they are employed and have incurred search costs etc., this will have permanent effects on high school employment, but we would expect the difference between the treatment and control group to become smaller over time. The differential incentives are expected to be mitigated by the fact that many students to some extent receive indirect financial support from their parents during this period, e.g. in the form of free housing and food. The monetary loss incurred from being born early in the quarter is about \$465 ($3 \cdot \155) for a student receiving

⁴To receive the higher basic rate, an individual would have to meet certain requirements, and therefore not all individuals who did not live with their parents received the higher basic rate.

the basic rate. It is higher for students receiving the means-tested supplement.⁵

3 Empirical Approach

Our interest is in the following reduced form model for individual i :

$$y_i = \alpha + \beta_G \cdot G_i + u_i \tag{1}$$

where β_G is the parameter of interest and measures the effect of the student grant, G , on a given outcome, y . The primary outcomes that we consider are measures of labor market participation during high school and high school grade point average. u_i is an error term. The estimated coefficient on grants can only be given a causal interpretation if the estimation procedure takes into account that the amount of financial aid received is often correlated with various observed and unobserved background characteristics. Plausibly, some of these characteristics will also matter for the outcome, and the causal effect of G on y cannot be straightforwardly identified. It is unlikely that we would be able to control for the bias even with a large set of covariates. To solve this problem, we take advantage of a particular source of exogenous variation in G to identify the causal effect. Specifically, individuals born early in a quarter (the treatment group) receive less financial aid than individuals born late in a quarter (the control group).

There are several methods that can be used to identify the effects of interest in our case, but because the probability of receiving treatment is discontinuous in date of birth, a regression discontinuity approach is particularly well-suited for our estimation problem.

3.1 The Regression Discontinuity Design

The idea behind identification using a regression discontinuity (RD) design is similar to the idea of a controlled experiment where we can choose one group to receive the treatment (the treatment group) and refrain from giving the treatment to another similar group (the control group). In

⁵A similar payout scheme is used for the child benefits given to parents by the Danish state. Therefore, we need to assume that the effects we measure are in fact caused by the grant payout scheme and not by other administrative features in the Danish benefit system. We address this issue in the robustness section.

this case, the effect of treatment can easily be identified as the difference between the outcomes of the treatment group and the control group. Essentially, the estimation problem is that we only observe $y_i = T_i y_{1i} + (1 - T_i) y_{0i}$ for individual i , but not the potential outcomes y_{1i} and y_{0i} , the outcome in the case with treatment and no treatment, respectively. T_i is the treatment variable for individual i . When an RD design is implemented correctly, it allows the researcher to estimate parameters from observational data that would usually require access to experimental data.⁶

In our setting, T_i is an indicator for whether or not individual i is born early in a quarter. Let x_i be the assignment variable which is a count variable that counts the number of days from the first day of a quarter (x_0) to the date of birth.⁷ In principal, we have repeated regression discontinuities which are joined together under the assumption that the treatment effect is the same at each discontinuity. In a sharp RD design, the treatment variable is a deterministic function of the assignment variable. Since individuals in our case are allocated to treatment and control groups based on their date of birth and since there is no uncertainty with respect to who receives treatment, we use a sharp RD design with the following treatment assignment function:

$$T_i = f(x_i) = 1[x_i \geq x_0] \tag{2}$$

where the discontinuity point is labeled x_0 . The fundamental assumption in an RD design is that the limits $T^+ = \lim_{x \rightarrow x_0^+} E[T_i | x_i = x]$ and $T^- = \lim_{x \rightarrow x_0^-} E[T_i | x_i = x]$ exist, and are not equal, $x^+ \neq x^-$, see Hahn, Todd, and van der Klaauw (2001).

3.2 Parametric Estimation

By definition, the assignment variable, x_i , has no common support in the control and treatment groups. A parametric RD specification is one way to deal with this problem. Consider the following parametric regression model:

⁶Imbens and Lemieux (2008) give an excellent overview of the workings of the RD design.

⁷The discontinuity point is assumed to be $x_0 = 0$, and x_i will be less than zero for observations in the end of a quarter and greater than or equal to zero for observations in the beginning of a quarter. Specifically, and to be discussed in more detail later, x_0 will correspond to April 1st, July 1st, and October 1st.

$$y_i = \alpha + \beta \cdot T_i + \delta \cdot x_i + \gamma \cdot Z_i + \varepsilon_i \quad (3)$$

where y_i , x_i , and T_i are defined above. β captures the causal effect of the student grant, the vertical discontinuity at x_0 . Z_i is a vector of background characteristics, such as gender, high school characteristics, parental characteristics etc. We expect the coefficient on x_i , δ , to be roughly zero as date of birth is not expected to have substantial effects on the outcome in the relatively short time spans we will consider. If necessary, the effect of the assignment variable on the outcome variable can be modeled in a less restrictive way using splines and/or polynomials (see, e.g., Lemieux and Milligan, 2008). The key identification assumption is that no matter how the function is modeled, it should be a smooth continuous function of the assignment variable. Control variables, Z_i , can be included in the specification to control for random variation that might otherwise bias results. If the RD design is correct and the sample is of a sufficient size, we would not expect the inclusion of covariates to matter since they should be the same for the control and treatment groups. Generally, the above model should identify the treatment effect at x_0 with or without covariates, as long as the window around the discontinuity point is not too wide. However, to extrapolate to x_i far away from the discontinuity point, it might be necessary to include covariates. The main concern with parametric estimation is its sensitivity to specification errors in modeling the relationship between y_i and x_i . The next subsection addresses this issue by discussing nonparametric estimation in an RD context.

3.3 Nonparametric Estimation

Hahn, Todd, and van der Klaauw (2001) show conditions under which the treatment effect β is non-parametrically identified. When the treatment effect β is assumed to be constant across individuals, the only assumption needed for nonparametric identification is a smoothness assumption:

$$(A1) \ E[y_{0i}|x_i = x] \text{ is continuous in } x \text{ at } x_0$$

Thus, the outcome (in the no treatment case) is assumed to be a smooth function of the assignment variable around the discontinuity point. In other words, in the absence of treatment, individuals close to the discontinuity point (on either side) are similar. If this were not the case, a difference in

the outcome between the two sides of the continuity point could not credibly be attributed solely to the change in treatment status. Under the smoothness assumption, β is identified by

$$\beta = \lim_{x \rightarrow x_0^+} E[y_i | x_i = x] - \lim_{x \rightarrow x_0^-} E[y_i | x_i = x] = y^+ - y^- \quad (4)$$

Thus, in the case where x has no effect on y , RD reduces to a comparison of means for the treatment and control groups. In our setting, Assumption (A1) implies that in the absence of treatment there is no jump in the outcome variable for those born late in a quarter compared to those born early in a quarter. We believe this to be a valid assumption.

In the case of heterogeneous treatment effects, two additional assumptions are needed for nonparametric identification of β . First, a conditional independence assumption:

(A2) T_i is independent of β_i , conditional on x_i near x_0

and secondly, a functional form restriction:

(A3) $E[y_{1i} - y_{0i} | x_i = x]$, considered a function of x , is continuous at x_0

The conditional independence assumption rules out that individuals somehow select themselves into treatment based on the gains or losses that they expect from being treated. With heterogeneous treatment effects, we can identify $E[\beta_i | x_i = x_0]$ by

$$E[\beta_i | x_i = x_0] = \lim_{x \rightarrow x_0^+} E[y_i | x_i = x] - \lim_{x \rightarrow x_0^-} E[y_i | x_i = x] = y^+ - y^- \quad (5)$$

Given that we can consistently estimate y^+ and y^- , we can consistently estimate β or $E[\beta_i | x_i = x_0]$ depending on our assumptions. Both assumption (A2) and (A3) are plausible in our setting since self-selection with respect to being born early in a quarter versus being born late in a quarter is highly unlikely. We will, however, address this point in the robustness section. In addition, the expected gain from treatment is likely to be similar on either side of the discontinuity point.

Hahn, Todd and van der Klaauw (2001) consider two nonparametric estimators: A one-sided kernel regression estimator and a local linear regression (LLR) estimator. The one-sided kernel regression estimator is asymptotically biased, and the bias converges to zero at a slower rate at the boundary points. This poses a particular problem for estimation in an RD setting where the

estimation is done at the boundary points. LLR has better boundary properties and a smaller bias than the kernel-based estimator. The choice of kernel and bandwidth is important in LLR. We use the triangle kernel as it has nice properties for LLR in an RD setting as it is boundary optimal, see Cheng et al. (1997).

$$K(x) = (1 - |x|)1[|x| \leq 1]$$

As an assistance for choosing a sensible bandwidth, one can apply the method of cross-validation described in Imbens and Lemieux (2008) and applied in Ludwig and Miller (2005). As the approach is not without its faults, the final choice of bandwidth will often depend on both the results from the cross-validation exercise and visual inspection. We will discuss the choice of bandwidth in more detail in section 6.5.

3.4 The Treatment Effect

The estimated causal effect will be an average treatment effect (ATE) at the discontinuity point, $x = x_0$. If the common effect assumption holds, we can further extrapolate to values of x further away from x_0 . Generally, the RD design has excellent internal validity. However, there are a couple of potential problems in our application. First, it might be that high school enrollment is influenced by the differences in the size of the total grant received. Second, in theory, manipulation of the day of birth is possible, e.g. for Caesarean born children. Third, other public transfers might be based on similar rules, e.g. using date of birth on each side of a quarter to determine payout of benefits. These issues will be addressed in the robustness section, and we find no support for any of these hypotheses.

We therefore conclude that the RD design has excellent internal validity. In our application, we believe that the RD design has external validity as well since there is no obvious reason why the treatment effect would be different for individuals with a value of x different from x_0 .

4 The Data

In the empirical analysis, we use administrative data collected by Statistics Denmark with information on the entire 1979-1986 birth cohorts in Denmark. The administrative data are collected from many different data sources, and we have a very extensive data set at hand for our analyses. First, we have detailed information on the characteristics of the 1979-1986 cohorts, including their exact date of birth, the amount of grant received, high school grade point average (GPA), and extensive information on their parents and their education and income. Secondly, detailed event history data on education provides us with information about the exact time of enrollment and completion of high school, and the type of high school education. Finally, we have information on monthly labor market participation (LMP), which tells us exactly in which months the individuals in our sample have been working. In addition, we observe wage income on a yearly basis. However, this wage measure is not ideal for our analysis since it measures income in the calendar year and not the school year. Suppose that we used wage income in the year of high school graduation, this measure would also include income earned after graduation. Since many high school graduates in Denmark work full-time in the year following graduation, the income earned during this period will dominate and make the measure less precise. The labor market related outcomes that we will consider in the analysis are therefore primarily based on information on monthly LMP. The data is based on monthly reports from firms to the tax authorities about the individuals who received income from the firms. The data is not particularly detailed, but it is extremely precise, and we therefore have very reliable information about whether a high school student worked in a given month or not. The measures of LMP are described in more detail in section 4.3. Our primary measure of academic performance is the high school grade point average (GPA). In addition, we also consider the following outcomes: the choice of high level math, the high school drop out probability, and the probability of enrolling in higher education within two years of high school graduation.

4.1 Sample Selection

In order to avoid that our estimation period coincides with reforms of the student grant system, we focus on the 399,121 individuals born between December 2nd 1979 and January 30th 1986. The maximum window at either side of the discontinuity point that we will consider is 30 days. Table 1 gives an overview of the sample selection process. We focus on individuals who attended a three-year high school and who turned 18 during the first or second year of high school. The three-year high school educations are the most common in Denmark, and focusing only on these types provides us with a more homogenous sample. Our identification strategy requires that individuals turn 18 during high school, and to be able to define post-treatment outcomes for all individuals in our sample, we disregard those individuals who turn 18 during the third year of high school. In addition, we disregard individuals who drop out of high school prior to or in the quarter in which they turn 18.⁸ As mentioned, the maximum number of days at either side of the continuity point that we will consider is 30. Thus, we drop individuals from the sample that are not born in the last 30 days or the first 30 days of a quarter.

The fundamental assumption in a regression discontinuity design is that observations just to the left and just to the right of the discontinuity point are inherently similar. However, in our preliminary data analysis we realized that this was indeed not the case for the individuals in our sample who were born in December and January. For example, those born in January are much more likely to turn 18 during their first year of high school than those born in December. We contribute this occurrence to the norms and rules regarding the age at school entry. During this period, enrollment in primary school more or less followed the year of birth. Thus, individuals who are born in December of one year will enroll a year before those born in January the following year. To avoid a potential school entry bias in our regression discontinuity estimate, we simply drop individuals who are born in December or January from the analyses. This leaves us with 66,400 observations. For the analysis of the effects of financial aid on high school GPA, the number of observations is slightly lower (60,738) as high school GPA is only observed for about 90 percent of the sample. For some individuals GPA is missing because they never complete the high school

⁸In order to obtain symmetry, we also disregard individuals from the control group who drop out during the quarter following the quarter in which they turn 18.

education. But for the majority of the missing observations, we do not know exactly why the GPA is missing. We address this issue in section 6 where we establish that a missing GPA is not systematically related to time of birth.

4.2 Descriptive Statistics

We will refer to individuals born within the first 30 days of a quarter, i.e. the first 30 days of April, July, and October, as the treatment group ($T_i = 1$). Correspondingly, individuals born within the last 30 days of a quarter, i.e. the last 30 days of March, June, and September, will be referred to as the control group ($T_i = 0$). Table 2 shows the means of outcome variables and control variables in the entire sample, and for the control and treatment groups separately.⁹ On average, the treatment group receives about 4,500 DKK (about \$560) less in financial aid than the control group in the year they turn 18. The treatment and control group means also vary across labor market outcomes, but not for high school GPA.¹⁰ Labor market participation is higher on average for the treatment group than for the control group. Comparing the means of the individual and parental characteristics, we find that the treatment and control groups are very similar.

4.3 Labor Market Participation

We will motivate our measures of labor market participation using figure 1. The graph shows mean monthly labor market participation for the control and treatment groups from one year before the time of the 18th birthday to one year after. The vertical line marks the month when the individuals in the control group turn 18. Individuals in the treatment group will turn 18 one month later, e.g. for the March-April groups, the vertical line marks the month of March. The construction of the measures of labor market participation requires that one pays specific attention to a couple of things that are evident in this graph. First, based on this descriptive graph, there does not

⁹In addition to the control variables summarized in table 2, we also include indicator variables for year of birth, year of high school enrollment, region of high school, and age of parents at birth. There are no statistically significant differences in the means of the treatment and control groups based on any of these variables.

¹⁰The high school grade point average is mainly computed based on grades given in courses taken in the final year of high school. Both the grades for the year's work and the grades obtained at exams are included. The grading scale used in Denmark during this time period is a grading scale with the following grades: '00', '03', '5', '6', '7', '8', '9', '10', '11', and '13'. Grades above 6 are passing grades.

appear to be long-term effects of a reduced grant, and we should therefore focus our attention on labor market measures around the months following the 18th birthday. Secondly, there is a large downward change in labor market participation around the 18th birthday caused by the institutional settings in Denmark that provide a favorable setting for the hiring of workers below the age of 18 and, of course, the fact that individuals become eligible for student grants when they turn 18. Specifically, the minimum wage an employer has to pay increases substantially when a worker turns 18. Since we only observe monthly labor market participation, we need to be careful that the constructed measures of labor market participation are not capturing these effects.

The graph shows a pattern of initial similarity in the labor market participation in the treatment and control groups. Briefly prior to the month of the 18th birthday, the paths of the treatment and control groups diverge and stay different for a while after which they converge again. Clearly, there is no reason to expect continued divergence in the treatment and control group work measures after the month of the 18th birthday. In fact, ignoring the payout scheme, we would even expect the control group to have higher labor market participation in the months following the 18th birthday since they have had more time to search for a new job. Thus, our estimate will be a conservative estimate of the true treatment effect.

We will consider a couple of measures of labor market participation during high school which are all based on the monthly labor market participation data. Let *labor market participation during the third year of high school* be an indicator variable with the value 1 if an individual is observed to be working in one or more months during the third year of high school. The school year is assumed to run from August to June. Similarly, let *labor market participation during the 12 months after the 18th birthday* be an indicator variable with the value 1 if an individual is observed to be working in one or more months during the 12 months after the 18th birthday. In relation to the discussion above, we actually use the 12 months from the month following the month of the 18th birthday of the treatment group. In addition, corresponding to the above two measures, we define the number of months worked during the third year of high school and the number of months worked during the 12 months after the 18th birthday. To some extent, this allows us to look at both the extensive and the intensive margin.

5 Empirical Results

We will start by presenting a graphical analysis of the discontinuity. Subsequently, we employ the two empirical approaches described in section 3 to estimate the effect of student grants on two types of outcomes: employment as measured by labor market participation and the number of months worked in a given period and academic performance as measured by high school GPA. In addition, we consider some additional outcomes for completeness.

5.1 Graphical Analysis

The graphs shown in figures 2-3 all share some common features. The assignment variable is grouped into two-day bins and takes values from 1 to 30, corresponding to 30 days before the beginning of a quarter and 30 days after. The plots show the average outcome of each bin. In addition, the plots are overlaid with fitted values and 95 percent confidence intervals from a linear regression that is allowed to vary with treatment status. The vertical line is placed at the discontinuity point, which in actual dates corresponds to April 1st, July 1st, or October 1st.

To illustrate the discontinuity in student grants caused by the payout scheme, we plot the amount of grant received in the first year in which the student is eligible for student grants against the assignment variable in figure 2. The graph shows a clear discontinuity in the yearly grant received of about 4,500 DKK (\$560). This amount is higher than three times the basic grant since many individuals receive a higher grant because they receive the means-tested supplement or because they do not live with their parents.

We now turn to the same type of plots for the outcome variables and investigate whether the basic plots suggest an effect of student grants on the measures of work or high school GPA. These plots are shown in figure 3. Based on these plots, we expect to find no significant effect on high school GPA, but a positive effect on labor market participation and number of months worked, although stronger for the measure of work during the 12 months after the 18th birthday than the measure for work during the third year of high school. This is also what we would expect given the picture in figure 1.

Often researchers are also interested in a plot that shows the density of the assignment variable

to determine whether there is a discontinuity in the density of the assignment variable, see Imbens and Lemieux, 2008. In our case, the assignment variable is date of birth which is not typically considered a covariate that individuals try to manipulate. Particularly because we are looking at a relatively narrow window and determining simply whether individuals were born in the beginning of or in the end of a quarter, manipulation seems unlikely, although, theoretically, women can choose to have Caesarean births and time them, e.g. in order to receive child benefits at an earlier date.¹¹ Buckles and Hungerman (2008) find that season of birth affects later outcomes such as health and labor market status. Controlling for family background characteristics they can explain up to half of the variation in later outcomes. In our setting, we would not expect this to be an issue since we compare children month-by-month and not e.g. quarter by quarter. If there was selection issues with respect to time of birth in our application, we would expect to find differences between the treatment and control groups in pre-high school variables, such as differences in the choice of high school tracks, parental income, parental education etc. Table 2 suggests that there are almost no significant differences between the two groups based on these measures. In addition, a plot of the number of births by date of birth does not suggest any systematic decrease in the number of births around the beginning of a quarter.

5.2 Regression Discontinuity Estimation

The estimation results are presented by outcome.

5.2.1 The Effects of Grants on Work Outcomes

Aggregate Measures of Labor Market Participation Table 3 presents the main results from the RD estimation of the effect of grants on labor market participation measures in the third year of high school. The first column shows the results using a 30-day window on either side of the discontinuity point. Both estimates from a standard linear regression and nonparametric estimates obtained by local linear regression are reported. Both types of estimates are significant

¹¹Recall that the child benefits in Denmark follow the same basic payout scheme as the student grants such that families with children born late in a quarter will receive child benefits earlier than families with children born early in the following quarter.

at the 5 percent level although relatively small in size. For the linear specification, the estimated treatment effect on labor market participation in the third year and number of months worked is 0.015 and 0.170, respectively. The nonparametric estimates are slightly larger, but in the same order of magnitude.

Since individuals who receive more than the minimum grant incur a greater loss in student grants from being born early in a quarter, we would expect any effects to be larger for this group. Column 2) shows the results from a regression including only those who received more than the minimum grant. As expected, the estimated treatment effects are now larger (with the exception of one), but they are still of a relatively small magnitude, and for the linear specification, they are not statistically significant at the 5 percent level. The fact that the effects are no longer significant could be due to the sample size being considerably smaller for this subsample, and furthermore that the individuals who receive more than the basic grant are typically also disadvantaged compared to students who only receive the basic grant. Thus, it may be harder for them to adjust their labor market behavior given a poor network or family conditions. However, based on the nonparametric estimates, the effects for this group are a little bit larger and still significant.

Including control variables does not result in any major changes, except for an (expected) increase in r-squared. By definition the RD design is meant to compare individuals who are the same, and control variables should not be necessary in a clean design setup. We conclude that even for the 30-day window, any problems with bias are negligible.

Considering (in column 4) a 15-day window makes it more likely that individuals are exactly the same, thereby reducing the need for control variables. Also, the treatment effect is expected to be bigger as we compare individuals closer and closer to the discontinuity point. This is because individuals who are born 30 days before the beginning of a quarter have to wait one month for their student grant payout, while individuals born 30 days after the beginning of a quarter have to wait for two months. As we move closer to the discontinuity point, the difference in waiting times, and therefore the expected difference in the outcome, increases. In fact, we find slightly larger estimates of the treatment effect for this specification. The estimated effects are very similar to the nonparametric estimates on the entire sample using a bandwidth of 15, which implies that

the kernel weighting function used does not really have a big impact on the results compared to a uniform weighting function.

Overall, the estimates suggest an increase in labor market participation during the third year of high school in the order of 1.5 to 2.8 percentage points, which is relatively small. However, one should keep in mind that average labor market participation is around 80 percent in this sample. For the number of months worked, the estimated treatment effect is about 0.2 months, which is also relatively small.

In table 4, the estimated treatment effects are shown for the outcome measures based on labor market participation in the 12 months after the 18th birthday. The estimates paint a similar picture as the results described above. Labor market participation increases by about two to three percentage points, and number of months worked increase with about 0.2-0.3 months. The estimated effects are generally larger than for the measures based on labor market participation in the third year of high school, but this is in accordance with our expectations based on figure 1, which clearly suggested larger effects for the months immediately following the 18th birthday. If one is willing to make a linear extrapolation based on these results, the implication is that receiving a \$1000 lower grant changes the probability of working in a given year by 2.7-5.5 percentage points. The income elasticity of labor market participation is about -0.2 to -0.3, where income is the study grant and wage income. At the intensive margin, i.e. number of months worked, the elasticity is about -0.15 to -0.25.

Whether or not our estimates are reasonable based on existing studies is difficult to say since the literature provides few benchmarks. Eissa and Hoynes (2004) reports income elasticities of labor market participation of -0.039 for women and -0.007 for men. Triest (1992) concludes that married women's labor market participation is likely to be more responsive to taxes than their hours worked as his estimated labor supply elasticities are higher when using all women as opposed to only working women.

Monthly Measures of Labor Market Participation In figure 4, we have depicted the treatment effect on labor market participation in each month. The setup of this figure corresponds to that of figure 1. The treatment effects and the 95 percent confidence intervals are obtained by

a linear regression using the 30-day window. From the graph, we see that (ignoring the month labeled '1') the treatment effects on monthly labor market participation after the 18th birthday are about 0.3-0.4 in the first couple of months and slowly decline and are equal to zero at approximately 9-10 months after the 18th birthday. Thus, the graph suggests that the effects of the lower grant caused by the payout scheme are of a transitory nature.

Wage Income Obviously, the advantage of the monthly measures of labor market participation is that we can fully control the time period in which our outcome is measured. The limitation is that it is not that informative with respect to whether those who are already working also work more hours. We can measure an effect on the extensive margin (work or not work), but we cannot say much about the intensive margin (number of hours worked). In table 5, results from estimations using wage income in the year of the 18th birthday are shown. The estimations in panel A include all observations with observed wages, while the estimations in panel B include only observations with observed strictly positive wages. Especially for the individuals born in September and October, wage income in the year of the 18th birthday is likely to be an underestimate of the true effect since wages are only measured on a yearly basis. We prefer using wage income in the year of the 18th birthday to using later wage income measures. Many students will complete high school and e.g. work full time, which will bring a lot of noise into the estimates. We find positive and mostly significant treatment effects in all specifications. In panel A, we find higher estimated effects for columns 2) and 4) as expected. However, these results suggest an increase in wage income in the order of 1,000 DKK (\$125), which is less than 25 percent of the average difference in grants received by the control and treatment groups. Thus, it appears that although students try to compensate for the lower grant received, they are not even close to compensating for the full amount. Since the estimate measures mean income, this effect could be due to the fact that more students work.

To estimate an effect on the intensive margin, we need to make an additional assumption. Specifically, the treated individuals that are induced to work, get jobs that are similar to the jobs held by those who already work in terms of the wage and the number of hours worked. Almost all student jobs pay about the same hourly wage, but they may require a different number of hours

worked per week. However, most jobs are probably in range of 5-15 hours a week. Given the above assumption, we can estimate the effect on the intensive margin. The estimated wage income effect is about 600 DKK in specification 1). Even if one has doubts regarding the identifying assumption, this effect is too large to be explained entirely by differences in the type of jobs held. Thus, we have established that the student grant affects the extensive as well as the intensive margin of work.

5.2.2 The Effects of Grants on Academic Outcomes

High School GPA In table 6, the main results from the RD estimation of the effect of grants on high school GPA are reported. For the linear specification using the 30-day window, the estimated treatment effect is small (0.023) and insignificant even at the 10 percent level. The size of the effect corresponds to about 1/50 of a standard deviation of high school GPA. Column 2) shows the results from a regression including only those who received more than the minimum grant. As expected, the estimated treatment effect is now larger, but still of a very small magnitude and only borderline significant. Including control variables only results in a slightly smaller estimated treatment effect and a considerable increase in r-squared. Considering (in column 4) a 15-day window, the estimated treatment effect is still small and insignificant. The nonparametric estimates are even smaller and still insignificant.

In summary, the effect of grants on GPA, if any, is of a relatively small size. For the linear specification with the 30-day window, the 95 percent confidence interval ranges from about -0.006 to 0.052. Thus, although insignificant, our estimate is very precise, and we can reject that there are any adverse effects of the lower grant on GPA. If anything, it seems that there are small positive effects on GPA. There could be numerous explanations for this. It might be the case that working more gives the student skills such as discipline, work ethics etc. Students who work on weekends do not have as much time to party and therefore they spend Friday or Saturday evening at home studying. Students who work low-skilled and low-paid jobs at McDonald's might realize that getting a higher education is important for them and they might therefore be motivated to study harder.

Other Academic Outcomes To check whether the lower grant affects other academic outcomes, table 7 shows similar results for a number of different academic outcomes. First, the choice of high level math in high school is investigated.¹² Supposedly, high level math is a relatively time-intensive course and a determinant of future academic performance, Joensen and Nielsen (2009). Again we find essentially a zero treatment effect, but if anything, the treatment effect is actually positive. Second, the lower grant could also affect the decision to drop out of high school, but again we find a zero treatment effect, or at least not a positive effect on the probability of dropping out. Third, we consider the effect of the lower grant on enrollment in higher education. Due to data limitations, we focus on the 1980-1983 birth cohorts and consider only enrollment within two years of high school completion. For all four academic outcomes (including GPA), we find a treatment effect that is essentially zero. If anything, the treatment effect actually tends to be positive in the sense that academic outcomes are improved rather than deteriorated.

6 Robustness of Results

In this section, we consider various robustness checks of our main results.

6.1 Are GPAs Systematically Missing?

One might speculate that missing GPA is in some way related to date of birth, thereby invalidating the identification strategy. In order to confirm that GPA is not systematically missing with respect to the treatment indicator, we estimate a probit model where the dependent variable is an indicator for missing GPA and the independent variables are the entire range of individual and parental characteristics and the treatment indicator. Both estimations with and without those individuals who drop out of high school yield no significant effects of the treatment indicator on the probability of having a missing GPA. The estimated coefficient of the treatment indicator is positive. This is true whether or not background characteristics are included. However, the associated p-value is never below 0.36. We conclude that there is no reason to be worried about the fact that GPA is

¹²The sample for the choice of high level math is reduced as this is only relevant for the traditional language and math and science tracks.

missing for about 10 percent of our sample with respect to our results.

6.2 Does Time of Birth Affect High School enrollment?

If individuals are able to anticipate the size of the grant that they will receive in high school and take this information into account when they make their high school enrollment decision, then our identification strategy would be weakened. If this were an issue in our analysis, we would expect to observe differences in high school enrollment by time of birth. To ensure that this is not an issue in our setup, we perform the same analysis as in section 5.2, but we now consider high school enrollment as the outcome. The estimated treatment effect is -0.005 for both the 30-day window and the 15-day window using a linear specification. Given the very small standard errors, 0.004 and 0.006, respectively, we can essentially rule out that there are any effects on high school enrollment.¹³ This reassures us that our identification strategy is in fact valid.

6.3 Alternative Rules that Discriminate by Time of Birth?

If other kinds of benefits are governed by the same set of payout rules (which is the case for child benefits), one might be worried that the estimated treatment effects are confounded by this. As shown in the data section, there are almost no statistically significant differences between the treatment and control groups in terms for pre-grant variables. If the differences in e.g. child benefits were driving our results, we would expect to find difference between the control and treatment groups in pre-grant variables and also high school enrollment rates; i.e. children whose parents receive higher child benefits are going to be more likely to enroll in high school educations. Thus, it seems reasonable to assume that we are in fact measuring the effects of the grant payout scheme.

¹³For high school enrollment, we consider a sample similar to our main sample, but obviously we no longer condition on high school enrolment. High school enrolment is defined as enrolment in a 3-year high school education.

6.4 Heterogeneous Treatment Effects

In this subsection, we investigate whether there are differential treatment effects by parents' education level, income, gender of the high school student, whether or not the student attended 10th grade, and work behavior prior to the 18th birthday.¹⁴ The presence of heterogeneous treatment effects may explain why we find significant effects of grants on labor market behavior but not on academic outcomes. It might be the case that only students with a favorable socioeconomic background are changing labor market status. Since this group has better support from their families, they might be able to work more and keep their GPA unaffected. We measure socioeconomic background by parental income and education. Another possibility is that only high-ability students change labor market behavior. This group of students are doing well in school, and they may not be affected much by working more in terms of lower academic performance. We do not have access to GPA from lower secondary school, but we proxy this by whether or not the student attended 10th grade. It could also be the case that only students who already have jobs are changing behavior. This is measured by the number of months worked in a six month period one year prior to the 18th birthday. Table 8 reports the results.

With respect to the socioeconomic indicators, parental education and income, there is not much evidence of heterogeneous treatment effects. An exception is the individuals with high-income fathers. Their treatment effect is significantly lower than those with low-income fathers. In fact, the treatment effect is essentially zero for this group. This suggests that parents subsidize their children's consumption to some extent. However, if this is indeed the case, we would also expect to find this pattern for those with high-income mothers. With respect to the treatment effect on high school GPA, there is no evidence of heterogeneous treatment effects, even at the 10 percent level. In general, the results do not support the hypothesis that the effect is only present for students with favorable socioeconomic characteristics.

There is no evidence of differential treatment effects across gender. Looking at the estimates for those who attended 10th grade and those who did not, we do not find much support for the story that only high-ability students are affected. Although the estimated effect on number of months

¹⁴10th grade is an optional grade at the lower secondary level. To some extent, it is an indicator of individual ability since high ability individuals tend to enroll directly in high school after 9th grade.

worked is smaller for students who attended 10th grade, it is not statistically different from the estimate for students who did not attend 10th grade. Also, there is no evidence of differential treatment effects on GPA. Finally, the students' labor market behavior prior to the 18th birthday does not seem to matter. To sum up, we find little evidence of strong heterogeneous treatment effects. The estimated effects appear to be present for all subgroups of students that we consider.

6.5 Choice of Bandwidth

The choice of bandwidth is potentially extremely important for the nonparametric estimation results, and one should always perform some sensitivity analysis on the choice of bandwidth. Figure 5 illustrates the sensitivity of our main results to the choice of bandwidth for bandwidths in the range of 3 to 30 days. At the low bandwidths, the estimated treatment effects are imprecise, but for the labor market measures the estimated treatment effects are significant at bandwidths of about 7-10 days and above. After that, the estimated treatment effect is relatively constant. For GPA, the estimated treatment effects are essentially zero at all choices of bandwidth. We would expect the estimates to be more imprecise at the low bandwidths, so we consider these results a good indicator that our choice of bandwidth is reasonable. In our main results, we used a bandwidth of 15. This bandwidth gives us enough observations to be able to get informative estimates while we are still only using observations relatively close to the discontinuity point.¹⁵

7 Conclusions

We employed exogenous variation in the size of student grants given to students in high school to identify the causal effects of grants on academic and labor market outcomes. The exogenous variation arises due to administrative rules governing the payout of grants when students turn 18. Students born late in a quarter receive a higher grant than students born early in the subsequent quarter. The exogenous variation is on average \$560 (year 2000 US \$), which is a relatively large amount for students in high school. The findings from both parametric and nonparametric

¹⁵We have also experimented with the cross-validation procedure described in Imbens and Lemieux (2008). But, similarly to Ludwig and Miller (2005), we find that the estimated loss function is extremely flat and therefore the procedure does not seem particularly well-suited for optimal bandwidth selection in our application.

estimates suggest that there is a positive and significant effect on both labor market participation and wage income. About two percent of all students change labor market participation status. This constitutes approximately 10 % of those not working in the 12 months following their 18th birthday. Various labor market participation measures yield the same basic result. There also seems to be an effect on the intensive margin of labor. Students who receive a lower grant have a higher wage income, and since the hourly wage for high school students is not expected to differ a lot across jobs, this suggests that they indeed work more hours.

We also estimated the effects of lowering the student grant on various academic outcomes. There is no adverse effect on GPA, and since the standard errors of the estimates are very small, we can reject that the GPA decreases by more than 0.04 grade points at a 5 percent significance level. A drop of 0.04 grade points is essentially zero since the mean is 8.18 with a standard deviation of 0.956. We have also tested other academic outcomes such as high school drop-out rates, enrollment in higher education, and the probability of choosing high level math. We find no evidence of adverse effects of lowering grants on any of these outcomes.

These results suggest that lowering student grants in high school has an effect on the labor market behavior of students, but there seem to be no adverse effects on various academic outcomes. Since the literature in general has found positive effects of grants in college, from an economic viewpoint, one might argue that the government should redirect grants from high school students to college students.

References

- [1] Arendt, J. N. 2008. "The impact of public student grants on drop-out and completion of higher education - evidence from a student grant reform". Health Economics Papers 2008:10, University of Southern Denmark.
- [2] Buckles, K. and D. M. Hungerman. 2008. "Season of Birth and Later Outcomes: Old Questions, New Answers". NBER Working Paper No. 14573.

- [3] Cheng, M.-Y., Fan, J., and J. S. Marron. 1997. "On Automatic Boundary Corrections". *Annals of Statistics*, 25(4), pp. 1691-1708.
- [4] Cunha, F. and J. J. Heckman. 2006. "The Technology of Skill Formation". *American Economic Review*, 97(2), pp. 31-47.
- [5] Cunha, F., Heckman, J. J., Lochner, L., and D. V. Masterov. 2006. "Interpreting the Evidence on Life Cycle Skill Formation". In E. A. Hanushek and F. Welch (Eds.), *Handbook of the Economics of Education*, Vol. 1. Elsevier. Amsterdam.
- [6] DesJardins, S. L., Ahlburg, D. A., and B. P. McCall. 2002. "Simulating the Longitudinal Effects of Changes in Financial Aid on Student Departure from College". *Journal of Human Resources*, 37(3), pp. 653-679.
- [7] Dynarski, S. 2003. "Does Aid Matter? Measuring the Effect of Student Aid on College Attendance and Completion". *American Economic Review*, 93(1), pp. 279-288.
- [8] Eissa, N. and H. W. Hoynes. 2004. "Taxes and the labor market participation of married couples: the earned income tax credit ". *Journal of Public Economics*, 88(9-10), pp. 1931-1958.
- [9] Hahn, J., Todd, P., and W. van der Klaauw. 2001. "Identification and estimation of treatment effects with a regression-discontinuity design". *Econometrica*, 69(1), pp. 201-209.
- [10] Imbens, G. W. and T. Lemieux. 2008. "Regression discontinuity designs: A guide to practise". *Journal of Econometrics*, 142(2), pp. 615-635.
- [11] Joensen, J. S. and H. S. Nielsen. 2009. "Is there a Causal Effect of High School Math on Labor Market Outcomes?". Forthcoming in *Journal of Human Resources*.
- [12] King, J. E. 2002. *Crucial Choices: How Students' Financial Decisions Affect Their Academic Success*. American Council of Education. Washington D. C.
- [13] Lemieux, T. and K. Milligan. 2008. "Incentive effects of social assistance: A regression discontinuity approach". *Journal of Econometrics*, 142(2), pp. 807-828.

- [14] Ludwig, J. and D. L. Miller. 2005. "Does Head Start Improve Children's Life Chances? Evidence from a Regression Discontinuity Design". NBER Working Paper No. 11702.
- [15] Ludwig, J. and D. L. Miller. 2007. "Does Head Start Improve Children's Life Chances? Evidence from a Regression Discontinuity Design". *Quarterly Journal of Economics*, 122(1), pp. 159-208.
- [16] Morais de Sa e Silva, M. 2008. "Opportunity NYC: A performance-based conditional cash transfer programme. A quantitative analysis". International Poverty Centre Working Paper No. 49.
- [17] Nielsen, H. S., Sørensen, T., and C. Taber. 2008. "Estimating the Effect of Student Aid on College Enrollment: Evidence from a Government Grant Policy Reform". Economics Working Paper No. 2008-14, University of Aarhus.
- [18] Rothstein, D. S. 2007. "High School Employment and Youths' Academic Achievement. *Journal of Human Resources*, 42(1), pp. 194-213.
- [19] Statistics Denmark. 2008. *Statistical Yearbook 2008*. Statistics Denmark. Copenhagen.
- [20] Stinebrickner, T. R. and R. Stinebrickner. 2007. "The Effect of Credit Constraints on the College Drop-Out Decision: A Direct Approach Using a New Panel Study". NBER Working Paper No. 13340.
- [21] Triest, R. K. 1992. "The effect of income taxation on labor supply in the United States". *Journal of Human Resources*, 25(3), pp. 491-516.
- [22] van der Klaauw, W. 2002. "Estimating the Effect of Financial Aid Offers on College Enrollment: A Regression-Discontinuity Approach". *International Economic Review*, 43(4), pp. 1249-1287.

A Figures

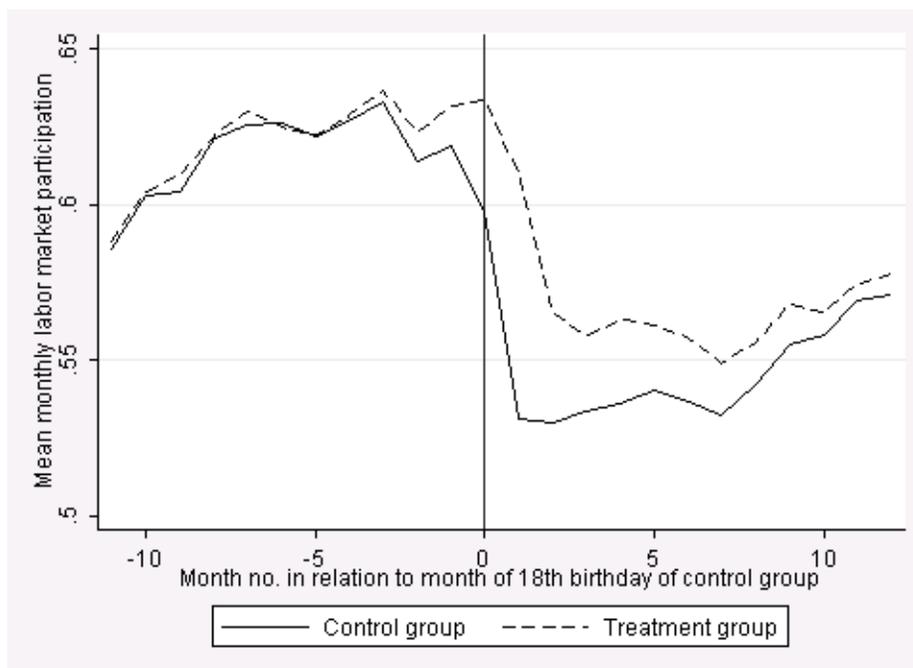


Figure 1: Monthly labor market participation for the treatment and control groups. 1981-1985 cohorts.

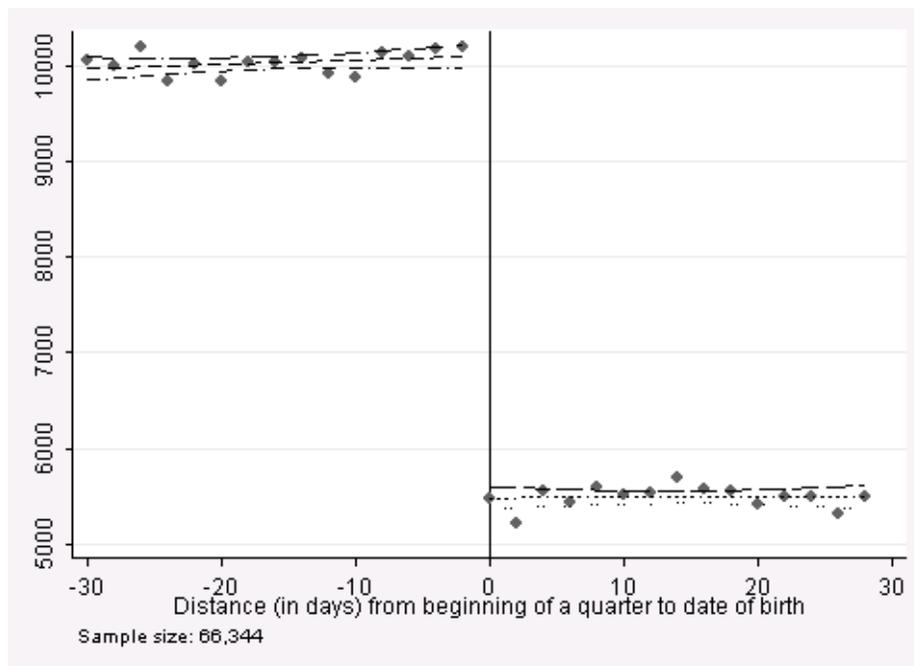


Figure 2: Size of grant (in DKK) received in the year of 18th birthday by time of birth.

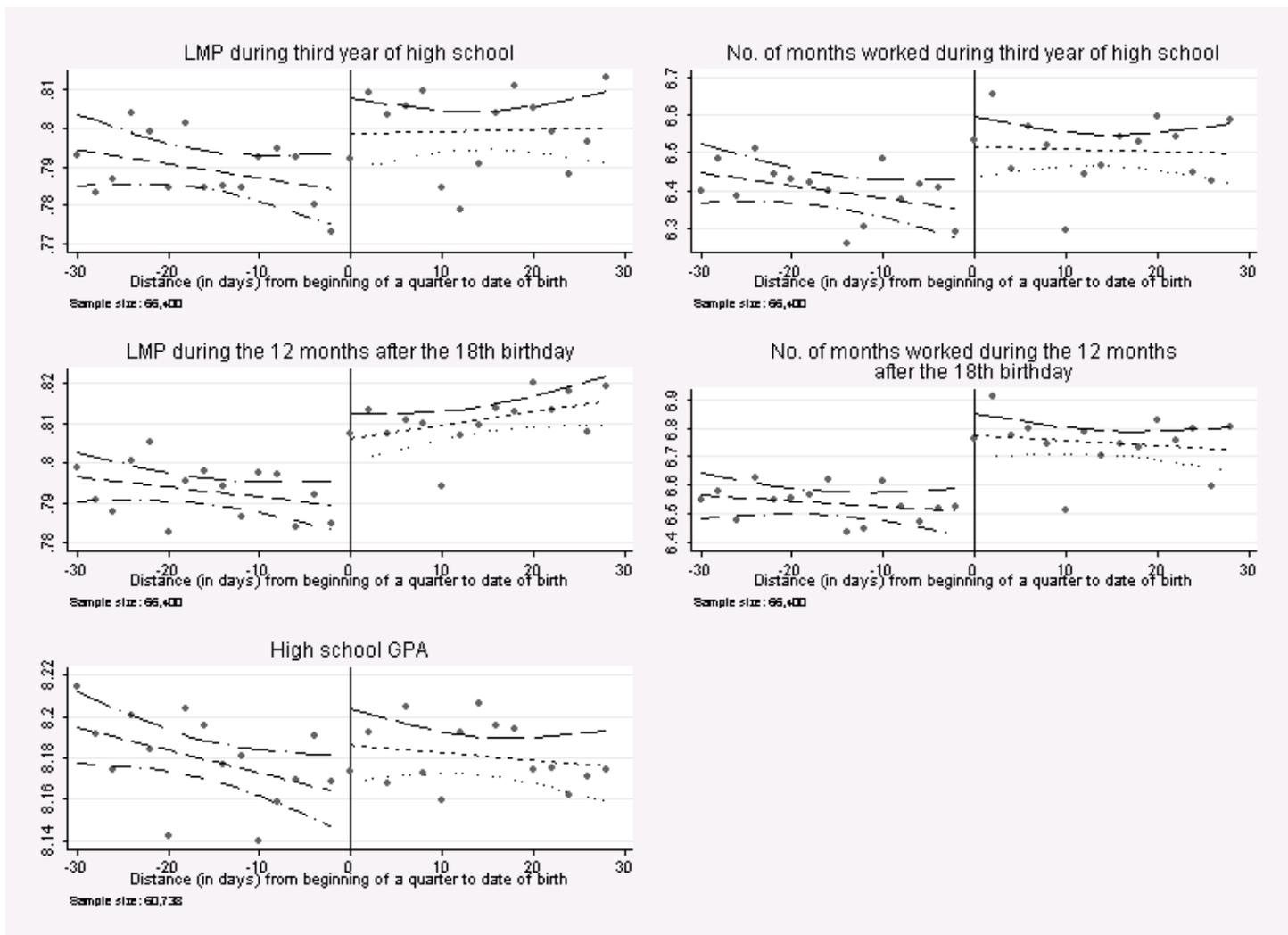


Figure 3: Main outcomes by time of birth. Predicted values from linear regressions and 95 percent confidence bands.

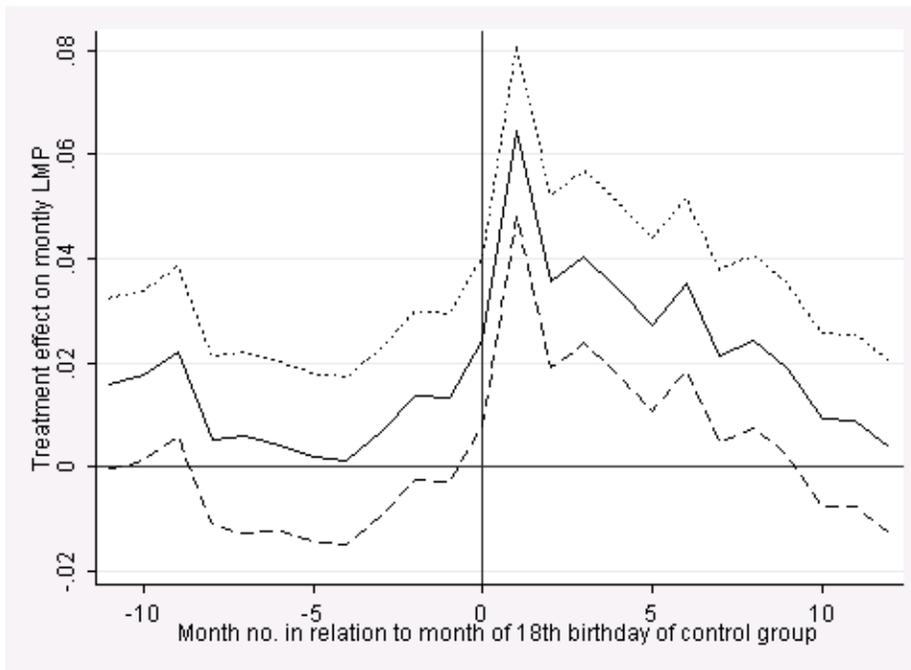


Figure 4: Treatment effects on monthly labor market participation from linear estimation. 1981-1985 cohorts.

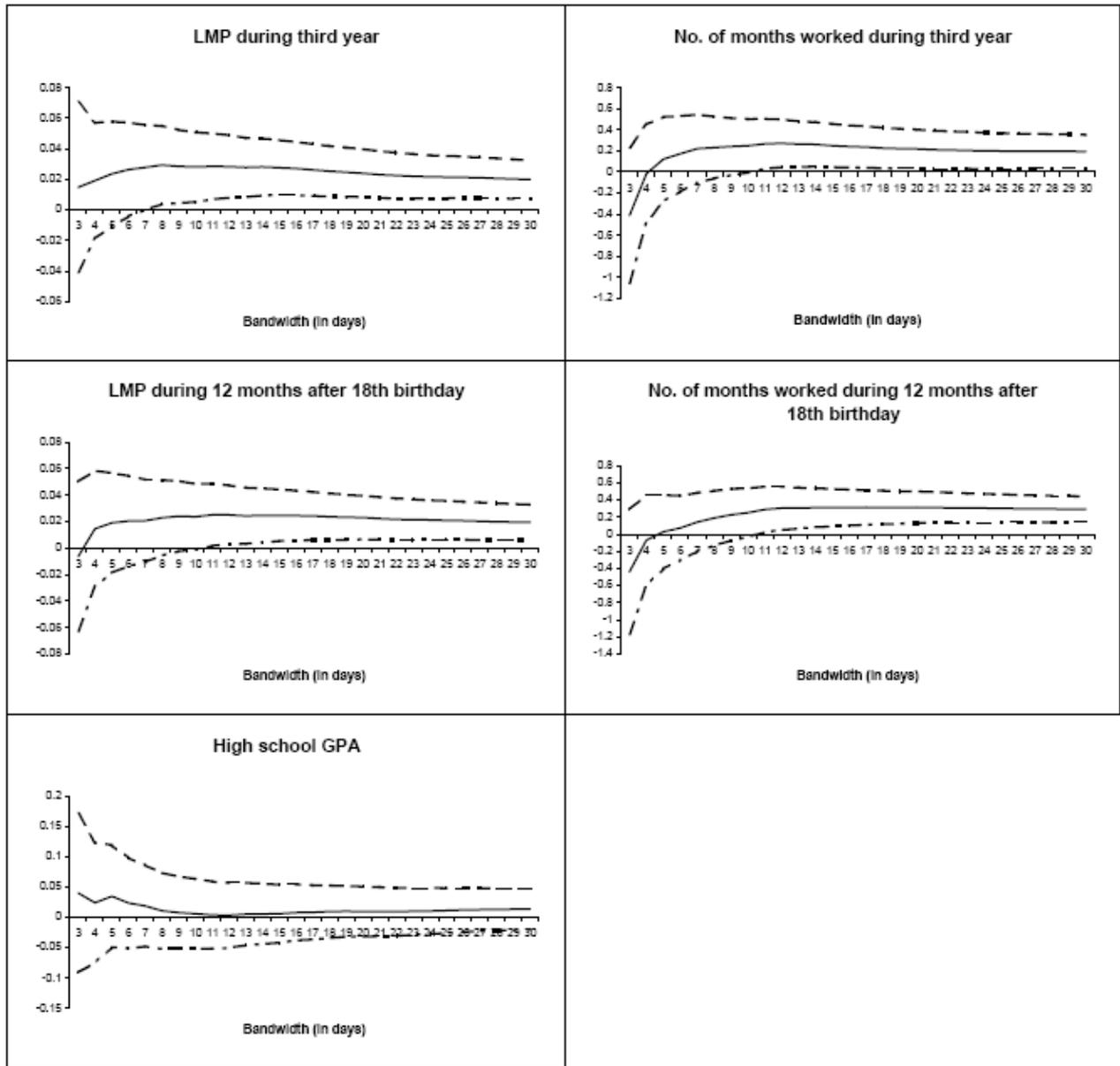


Figure 5: Sensitivity analysis of bandwidth choice for the main outcome measures. LLR estimates and 95 percent confidence intervals.

B Tables

Table 1: SEQUENTIAL OVERVIEW OF SAMPLE SELECTION

Description of sample selection	Number of individuals	Percentage of total
Born between December 2nd 1979 and January 30th 1986	391,121	100.0
Enrolled in a three-year high school education	176,082	45.0
Turned 18 during the first or second year of high school	141,860	36.3
Did not drop out prior to or in the quarter in which they turned 18 ^a	129,879	33.2
Born in the first 30 days or the last 30 days of a quarter	87,909	22.5
Not born in December or January	66,400	17.0
Observed GPA	60,738	15.5

Notes:

a) To obtain comparable treatment and control groups, we also disregard the controls who dropped out in the quarter of their control group's 18th birthday.

Table 2: COMPARISON OF MEANS IN THE CONTROL AND TREATMENT GROUPS

	All		Control		Treatment	
	Mean	Std. Dev.	Mean	Std.dev.	Mean	Std. Dev.
Yearly grant in year 2000 DKK	7752.174	5385.720	10034.410	5344.993	5487.346***	4375.147
Outcomes						
LMP during 3rd year	0.794		0.789		0.799***	
No. of months worked during 3rd year	6.454	4.515	6.400	4.528	6.508***	4.502
LMP during 12 months after 18th birthday	0.802		0.793		0.811***	
No. of months worked during 12 months after 18th birthday	6.644	4.805	6.537	4.826	6.751***	4.782
Wage income at age 18	22090.7	18865.82	21781.32	19105.68	22398.6***	18619.23
High school GPA	8.180	0.956	8.179	0.956	8.181	0.956
High level math	0.496		0.495		0.496	
High school drop-out	0.075		0.076		0.075	
Enrollment in higher education	0.599		0.597		0.602	
Individual characteristics						
Female	0.551		0.551		0.551	
Turned 18 during 1st year of high school	0.440		0.434		0.446***	
Attended 10th grade	0.593		0.602		0.585***	
<i>High school track</i>						
Math and science track	0.371		0.369		0.373	
Language track	0.280		0.280		0.281	
International track	0.002		0.002		0.002	
Business track	0.259		0.261		0.257	
Technical track	0.087		0.087		0.087	
Parental characteristics						
Mother missing in data	0.002		0.002		0.002	
Father missing in data	0.017		0.018		0.016	
Log of mother's income at age 18	12.020	2.031	12.014	2.033	12.025	2.029
Log of father's income at age 18	12.016	2.995	11.997	3.021	12.035	2.969
Mother's income is missing	0.024		0.024		0.024	
Father's income is missing	0.056		0.057		0.055	
<i>Mother's education level</i>						
Basic	0.259		0.258		0.259	
Vocational	0.325		0.329		0.322	
Higher	0.399		0.396		0.402	
Missing	0.017		0.017		0.017	
<i>Father's education level</i>						
Basic	0.215		0.220		0.211***	
Vocational	0.395		0.393		0.397	
Higher	0.358		0.354		0.363	
Missing	0.031		0.033		0.029***	
Number of children of the mother	2.428	0.961	2.427	0.970	2.429	0.951
Number of observations	66,400		33,071		33,329	

Notes:

- '***' indicates statistically different from the mean of the untreated group at a 1 percent level.
- There are 5,662 observations with missing high school GPA and 56 observations with missing information on the yearly grant.
- Parents' log income has been set equal to zero if missing.
- The sample sizes for the secondary outcomes are in some cases different from the numbers indicated here. See section 5.2 for more details.

Table 3: REGRESSION DISCONTINUITY ESTIMATES OF THE EFFECT OF GRANTS ON WORK DURING THE THIRD YEAR

Estimation approach	1) All	2) Received more than the minimum grant	3) All	4) 15-day window
Linear				
<i>A. LMP during 3rd year</i>				
Treatment effect	0.015** (0.006)	0.016* (0.009)	0.014** (0.006)	0.027*** (0.009)
Control variables	no	no	yes	no
R-squared	0.000	0.000	0.037	0.000
<i>B. Months during 3rd year</i>				
Treatment effect	0.170** (0.070)	0.146 (0.103)	0.154** (0.068)	0.224** (0.099)
Control variables	no	no	yes	no
R-squared	0.000	0.000	0.053	0.000
Nonparametric				
<i>A. LMP during 3rd year</i>				
Treatment effect	0.028*** (0.009)	0.043*** (0.015)		
Bandwidth (in days)	15	15		
<i>B. Months during 3rd year</i>				
Treatment effect	0.252** (0.105)	0.391** (0.164)		
Bandwidth (in days)	15	15		
Observations	66,400	31,121	66,400	33,521

Notes:

- a) Controls include all personal, high school, and parental characteristics.
- b) '***', '**', and '*' indicate significance at the 1, 5, and 10 percent level, respectively.
- c) Standard errors for nonparametric estimates are bootstrapped with 200 replications.

Table 4: REGRESSION DISCONTINUITY ESTIMATES OF THE EFFECT OF GRANTS ON WORK DURING THE NEXT 12 MONTHS

Estimation approach	1) All	2) Received more than the minimum grant	3) All	4) 15-day window
Linear				
<i>A. LMP during next 12 months</i>				
Treatment effect	0.017*** (0.006)	0.024** (0.009)	0.015** (0.006)	0.026*** (0.009)
Control variables	no	no	yes	no
R-squared	0.001	0.001	0.040	0.000
<i>B. Months during next 12 months</i>				
Treatment effect	0.269*** (0.074)	0.240** (0.109)	0.242*** (0.072)	0.340*** (0.105)
Control variables	no	no	yes	no
R-squared	0.001	0.001	0.057	0.001
Nonparametric				
<i>A. LMP during next 12 months</i>				
Treatment effect	0.025** (0.010)	0.044*** (0.015)		
Bandwidth (in days)	15	15		
<i>B. Months during next 12 months</i>				
Treatment effect	0.310*** (0.111)	0.318* (0.172)		
Bandwidth (in days)	15	15		
Observations	66,400	31,121	66,400	33,521

Notes:

- a) Controls include all personal, high school, and parental characteristics.
- b) '***', '**', and '*' indicate significance at the 1, 5, and 10 percent level, respectively.
- c) Standard errors for nonparametric estimates are bootstrapped with 200 replications.

Table 5: REGRESSION DISCONTINUITY ESTIMATES OF THE EFFECT OF GRANTS ON WAGE

	1)	2)	3)	4)
		Received more than the minimum grant		15-day window
Estimation approach	All		All	
Linear				
<i>A. Wage</i>				
Treatment effect	838.984*** (296.187)	949.819** (430.717)	706.852** (284.768)	1202.183*** (419.292)
Control variables	no	no	yes	no
R-squared	0.000	0.001	0.077	0.000
Observations	64,392	29,993	64,392	32,494
<i>B. Strictly Positive Wage</i>				
Treatment effect	640.168** (303.622)	575.816 (443.024)	556.602* (293.503)	1141.123*** (430.095)
Control variables	no	no	yes	no
R-squared	0.000	0.000	0.067	0.000
Observations	54,963	25,279	54,963	27,697

Notes:

- a) Controls include all personal, high school, and parental characteristics.
b) '***', '**', and '*' indicate significance at the 1, 5, and 10 percent level, respectively.
c) The wage is measured in the year of the 18th birthday.
d) In panel B only observations with strictly positive wages are included.

Table 6: REGRESSION DISCONTINUITY ESTIMATES OF THE EFFECT OF GRANTS ON HIGH SCHOOL GRADE POINT AVERAGE

	1)	2)	3)	4)
Estimation approach	All	Received more than the minimum grant	All	15-day window
Linear				
Treatment effect	0.023 (0.015)	0.041* (0.022)	0.015 (0.015)	0.017 (0.022)
Control variables	no	no	yes	no
R-squared	0.000	0.000	0.109	0.000
Nonparametric				
Treatment effect	0.006 (0.024)	0.021 (0.033)		
Bandwidth (in days)	15	15		
Observations	60,738	28,047	60,738	30,640

Notes:

- a) Controls include all personal, high school, and parental characteristics.
- b) '***', '**', and '*' indicate significance at the 1, 5, and 10 percent level, respectively.
- c) Standard errors for nonparametric estimates are bootstrapped with 200 replications.

Table 7: REGRESSION DISCONTINUITY ESTIMATES OF THE EFFECT OF GRANTS ON VARIOUS ACADEMIC OUTCOMES

Estimation approach	1) All	2) Received more than the minimum grant	3) All	4) 15-day window
Linear				
<i>A. High Level Math</i>				
Treatment effect	0.014 (0.010)	0.027* (0.015)	0.010 (0.008)	0.016 (0.014)
Control variables	no	no	yes	no
R-squared	0.000	0.000	0.3781	0.000
Observations	40,279	17,481	40,279	20,363
<i>B. High School Dropout</i>				
Treatment effect	-0.001 (0.004)	-0.013** (0.006)	0.001 (0.004)	0.001 (0.006)
Control variables	no	no	yes	no
R-squared	0.000	0.000	0.058	0.000
Observations	66,400	31,121	66,400	33,521
<i>C. Enrollment in Higher Education within Two Years</i>				
Treatment effect	0.014 (0.010)	0.016 (0.014)	0.016* (0.009)	0.026* (0.014)
Control variables	no	no	yes	no
R-squared	0.000	0.000	0.073	0.000
Observations	40,846	18,918	40,846	20,561

Notes:

- a) Controls include all personal, high school, and parental characteristics.
- b) '***', '**', and '*' indicate significance at the 1, 5, and 10 percent level, respectively.
- c) Only the 1980-1983 birth cohorts are used for the estimations in panel C.

Table 8: SUBGROUP TREATMENT EFFECTS

	LMP during 12 months after 18th birthday Coef./Std.Err	No. of months worked during 12 months after 18th birthday Coef./Std.Err	High school GPA Coef./Std.Err
Father's education level			
Treatment (<i>Basic</i> is left-out category)	0.018** (0.009)	0.301*** (0.102)	0.012 (0.021)
<i>Vocational</i> *Treatment	0.000 (0.008)	-0.014 (0.099)	0.011 (0.021)
<i>Higher</i> *Treatment	-0.002 (0.008)	-0.055 (0.101)	0.004 (0.021)
<i>Missing</i> *Treatment	-0.022 (0.019)	-0.013 (0.225)	-0.045 (0.049)
Mother's education level			
Treatment (<i>Basic</i> is left-out category)	0.012 (0.008)	0.235** (0.097)	0.044** (0.020)
<i>Vocational</i> *Treatment	0.006 (0.008)	0.079 (0.098)	-0.025 (0.020)
<i>Higher</i> *Treatment	0.008 (0.008)	0.031 (0.094)	-0.026 (0.019)
<i>Missing</i> *Treatment	-0.026 (0.024)	-0.194 (0.294)	-0.038 (0.064)
Father's income			
Treatment (<i>Low</i> is left-out category)	0.027*** (0.007)	0.323*** (0.084)	0.022 (0.017)
<i>High</i> *Treatment	-0.021*** (0.006)	-0.121 (0.077)	-0.006 (0.016)
<i>Missing</i> *Treatment	0.013 (0.014)	-0.050 (0.166)	-0.010 (0.035)
Mother's income			
Treatment (<i>Low</i> is left-out category)	0.017** (0.007)	0.301*** (0.083)	0.023 (0.017)
<i>High</i> *Treatment	0.001 (0.006)	-0.053 (0.075)	-0.006 (0.016)
<i>Missing</i> *Treatment	-0.021 (0.020)	-0.404* (0.245)	-0.016 (0.052)
Gender			
Treatment (<i>Men</i> is left-out category)	0.023*** (0.007)	0.287*** (0.085)	0.031* (0.018)
<i>Women</i> *Treatment	-0.011* (0.006)	-0.029 (0.075)	-0.014 (0.016)
10th grade			
Treatment (<i>Attended</i> is left-out category)	0.017** (0.007)	0.221*** (0.080)	0.017 (0.017)
<i>Did not attend</i> *Treatment	0.001 (0.006)	0.123 (0.076)	-0.000 (0.016)

CONTINUED ON NEXT PAGE

Table 8 – CONTINUED

	LMP during 12 months after 18th birthday Coef./Std.Err	No. of months worked during 12 months after 18th birthday Coef./Std.Err	High school GPA Coef./Std.Err
Extent of work in six month period one year prior to 18th birthday			
Treatment (<i>0-1 months</i> is left-out category)	0.006 (0.008)	0.193** (0.093)	0.019 (0.022)
<i>2-4 months</i> *Treatment	0.002 (0.010)	-0.027 (0.107)	0.012 (0.025)
<i>5-6 months</i> *Treatment	0.006 (0.007)	0.028 (0.085)	0.013 (0.020)

Notes:

- a) '***', '**', and '*' indicate significance at the 1, 5, and 10 percent level, respectively.
- b) To estimate subgroup treatment effects the relevant grouping variables and interactions these variables and the treatment indicator were included in a linear regression of the outcome of interest on the treatment indicator and the assignment variable. The actual treatment effects for each subgroup can be computed by adding the reported coefficient on the interaction term and the coefficient on the treatment indicator.

Economics Working Paper

- 2008-05: Torben M. Andersen and Michael Svarer: The role of workfare in striking a balance between incentives and insurance in the labour market
- 2008-06: Michael Svarer: Crime and Partnerships
- 2008-07: Marianne Simonsen and Lars Skipper: The Incidence and Intensity of Formal Lifelong Learning
- 2008-08: Torben M. Andersen and Allan Sørensen: Globalisation squeezes the public sector - is it so obvious?
- 2008-09: Kristin Kleinjans: Do Gender Differences in Preferences for Competition Matter for Occupational Expectations?
- 2008-10: Martin Paldam: Development and foreign debt: The stylized facts 1970-2006
- 2008-11: Christian Dahl Winther: Brand popularity, endogenous leadership, and product introduction in industries with word of mouth communication
- 2008-12: Maria Knoth Humlum: Timing of Family Income, Borrowing Constraints and Child Achievement
- 2008-13: Erich Gundlach and Martin Paldam: Income and Democracy: A Comment on Acemoglu, Johnson, Robinson, and Yared (2008)
- 2008-14: Helena Skyt Nielsen, Torben Sørensen and Christopher Taber: Estimating the Effect of Student Aid on College Enrollment: Evidence from a Government Grant Policy Reform
- 2008-15: Erich Gundlach and Martin Paldam: The Democratic Transition. A study of the causality between income and the Gastil democracy index
- 2008-16: Alexander K. Koch and Eloïc Peyrache: Aligning Ambition and Incentives
- 2009-01: Alexander K. Koch, Albrecht Morgenstern and Philippe Raab: Career concerns incentives: An experimental test
- 2009-02: Maria Knoth Humlum and Rune Majlund Vejlin: The Effects of Financial Aid in High School on Academic and Labor Market Outcomes: A Quasi-Experimental Study