Life after Debt: Post-Graduation Consequences of Federal Student Loans*

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February 21, 2017

Abstract

We estimate the causal effect of student loans on post-graduation outcomes exploiting a kink in the formula determining eligibility for need-based student loans. Using a representative sample of students graduating with a bachelor's degree in 1993, we establish that student debt leads to lower earnings soon after graduation, an effect which dissipates over time. Surprisingly, the negative effect on earnings is driven by hours worked rather than wages. Students with debt are less "choosy" on the job market: they are more inclined to accept part-time work and jobs that are less related to their degree and offer limited career potential.

Journal of Economic Literature Classification Number: I22.

*This paper has benefitted greatly from helpful conversations with Dave Frisvold, Alice Schoonbroodt, Lance Lochner, and Michael Choi. Chander Kochar and Larry Warren did important early work in organizing the data and formulating the identification strategy. We are also grateful for comments and feedback received from seminars at Auburn, Iowa State, UQAM, Southhampton, and the REDg conference in Madrid. All errors are our own.

1 Introduction

College student debt has become a pervasive form of financing in scale and scope. The most recent 'Quarterly Report on Household debt and Credit' from the Federal Reserve Bank of New York reports that outstanding student loan debt, currently valued around $1.26 trillion and accounting for 10% of total household debt, is the second largest source of household debt in the United States—it is surpassed only by home mortgages.[1] In 2012, around two thirds of undergraduates seeking a bachelor's degree had accumulated some debt.[2] While there is an extensive literature concerned with delinquency, default, and the design of alternative student loan programs (Lochner and Monge-Naranjo 2015; Looney and Yannelis 2015), work on the impact of student debt on post-graduation economic outcomes such as labor market earnings is relatively sparse.

To study this relationship, we use data from the Baccalaureate and Beyond Longitudinal Study: 1993/03 (B&B: 93/03). The B&B: 93/03 surveyed a representative sample of undergraduate students who received their bachelor's degree during the 1992–93 academic year, with follow-up surveys conducted in 1994, 1997 and 2003. The study collects data at the administrative, institutional, and individual levels. The administrative data are linked to the National Student Loan Data System (NSLDS), which provides information on loan types and amounts disbursed throughout the course of an individual's undergraduate study. The institutional-level data, besides providing information on individual demographics, provide the relevant information necessary to determine eligibility for need-based Title IV loans (the most important being subsidized Stafford loans) during the last year of schooling. Finally, individual survey data provides information on post-graduation labor market outcomes such as earnings, as well as many other outcomes ranging from job satisfaction to graduate school attendance and tenure choice.

Unconditional means *do* show that earnings for borrowers in 1994, a year after graduating for students in our sample, were approximately 4% lower those with no debt. Of course, there are any number of factors that affect borrowing, such as motivation and family support, and presumably earnings after college as well. So to estimate causal effects of student debt

[1] The August 2016 report can be found at `https://www.newyorkfed.org/medialibrary/interactives/householdcredit/data/pdf/HHDC_2016Q2.pdf`.

[2] This figure comes from the 2012 National Postsecondary Student Aid Study (NPSAS). Other papers that document the rise in student loans include Akers and Chingos (2014), Haughwout et al. (2015), Steele and Baum (2009), Woo (2014) and Arvidson et al. (2013).

accrued on earnings, we employ an identification strategy that exploits a kink in the federal subsidized loan program eligibility. A demonstrated financial need is required to be eligible. Financial need for any given academic year is computed as the difference between annual cost of attendance (COA) and the sum of expected family contribution (EFC) and grants or scholarships the student receives from government or institutional sources. For students with financial need, the borrowing cap increases linearly with need. This results in a kink in the amount of permitted subsidized loans around the need threshold value of 0. To the left of the threshold, the slope of the amount of subsidized loans that can be borrowed as a function of need is 0, to the right of the threshold, the slope is 1.[3]

We take advantage of the fact that dependent undergraduate students had access to only subsidized loans through the 1992–93 academic year.[4] Because all students in our sample graduated in 1993, all federal loans received by dependent students must be subsidized loans. This motivates our benchmark sample to consist of only dependent students though our results are robust to including independent students. While we only have information on financial need in the last year of schooling, our key independent variable consists of cumulative debt accumulated over the course of the entire undergraduate program. Given the transitory nature of financial need from year to year as well as the fact that students need not accept all loans for which they are eligible, the mapping between financial need and total amount borrowed (simply referred to as debt or Title IV loans hereafter) is not a sharp one but "fuzzy." Hence, we use a fuzzy regression kink (RK) estimation strategy.

We estimate that an additional $1,000 of borrowing reduces income one year after graduation by approximately 2.5%, though this estimate is not statistically significant. Extrapolating this result, earnings for an individual with the mean level of borrowing are on average 5% lower than earnings of an individual with no debt. In an appendix, we provide additional evidence to support these findings using a simple OLS specification with a whole battery of controls as well as a partially linear model where we allow parental income to enter non-parametrically. Intuitively, the effects of debt on earnings dissipate over time with barely any effect 4 years after graduation and, by 10 years after graduation, nothing. We note that our dataset focuses on individuals that actually graduate so we are not able to address the

[3] There is an upper bound on how much can be borrowed as well but very few students actually reach it.

[4] While subsidized (Stafford) loans constitute the bulk of Title VI subsidized loans, students with exceptional financial needs were also illegible for Perkins loans. Meanwhile, independent students also has access to Supplemental Loans for Students (SLS, now known as unsubsidized Stafford loans), for which dependent students only became eligible during the 1993–94 academic year.

"total" effects of debt on income. Instead the effects we estimate here can be thought of as an "intensive" margin effect conditional on graduating. There is an additional possible "extensive" margin effect if increased in debt reduce the probability of graduating at all.

We also examine the channels through which debt might matter for earnings. First, we consider a decomposition of earnings into hours worked and the hourly wage. In both 1994 and 1997, there are significant negative effects on hours worked with basically no effects on the hourly wage in any of the three years for which we observe earnings. We also examine whether the effects are due to effects while in school by focusing on college GPA and major choice. While there is limited effects on major choice, there are sizable negative effects of debt on GPA. Finally, we examine whether debt has longer term consequences for demographic and educational outcomes such as purchasing a house or going to graduate school. We find no effect here in contrast to much of the literature.

The closest works to ours are by Chapman (2016) and Weidner (2016). They both study the same question with the same dataset with the key difference in the identification strategy. Chapman (2016) uses state merit aid policies that, in effect, reduce the amount of debt students who receive such grants graduate with.[5] Her main result contrasts sharply with ours, in that higher levels of debt are associated with higher earnings. She suggests that this result is driven by graduates choosing jobs with low pecuniary payouts but high non-pecuniary benefits, e.g. jobs in public service, echoing earlier results by Rothstein and Rouse (2011). Weidner (2016), on the other hand, argues that higher debt levels reduce earnings using a different identification strategy and a more structural approach. Besides these works, Bettinger et al. (2016) study the long run effects of California's state merit aid policy. Because of how the program operates they were able to exploit discontinuities in aid as a function of need as well as ability. They document positive effects on earnings at the ability discontinuity. The implications for the effect of student loans on income are unclear since they are unable to identify a significant effect of student debt induced by this additional aid. Minicozzi (2005) also finds debt to have a positive impact on income, at least in the short-run.

There has been some other work examining the effects of student debt on other post-graduation outcomes. For instance, students with higher debt levels tend to delay forming

[5] Cohodes and Goodman (2014) study a version of this kind of policy implemented in Massachusetts, whereby high-scoring students were offered tuition waivers to attend in-state public colleges. They find that this policy reduces completion rates by shifting students towards lower quality colleges.

new households (Bleemer et al. 2015; Addo 2014), which leads to delays in home ownership (Mezza et al. 2014), marriage (Gicheva 2016), and having children (Shao 2014). Baum (2015) raises the possibility that student debt can also discourage entrepreneurship, perhaps through reduced access to credit necessary to start a business after graduation. Others have argued that debt has limited effects. Monks (2001) finds that students with higher levels of debt go to graduate school at similar rates to those with lower debt levels.

Much of the work on the effects of financial aid has focused on whether it fosters educational attainment, either on the extensive margin by encouraging people to attend or on the intensive margin by encouraging people to persist in school.[6] One of the first to examine the link between aid and persistence in college is the work by Dynarski (2003) who uses the elimination of the Social Security Student Benefit Program in 1982. She finds that a $1,000 increase in grants increases the probability of attending school by 3.6 percentage points. More recently, Turner (2014) and Marx and Turner (2015) implement a regression kink design to study the impact of the federal Pell Grant program on educational attainment and student borrowing. Bettinger (2004), using data from the Ohio Board of Regents, exploits a different discontinuity related to family size in the Pell Grants awards formula to study education persistence. He finds that Pell Grants increase persistence, although the results are not robust. Denning (2016) uses a discrete change in the amount of federal financial aid available to financially independent students. Scott-Clayton (2010) studies the PROMISE program in West Virginia that links grants to academic achievement. Kane (2006) studies the D.C. Tuition Assistant Grant Program, which dramatically reduced the price of tuition at public institutions for D.C. residents. This led to a doubling of attendance at public institutions in Virginia and Maryland, the first states eligible, and sharp increases in enrollment at institutions in other states as they are added to the program. Finally, in perhaps the most ambitious study, Angrist et al. (2015) conduct a randomized evaluation of aid from a privately funded scholarship program for applicants to Nebraska's public colleges. Aid offers increased enrollments, persistence, and shifted students from two- to four-year schools.

[6]This literature is summarized in the paper by Deming and Dynarski (2009) with a particular focus on individuals from a low income background.

2 Institutional Details on Financial Need and Aid

In this section, we review various institutional details, including the difference between subsidized and unsubsidized loans and how eligibility for these two types of loan has evolved over time. This information motivates our focus on dependent students and illustrates that the B&B: 93/03 survey is particularly well suited to take advantage of the regression kink design.

2.1 Federal Student Loans

Title IV of the Higher Education Act of 1965 covers the administration of the federal student financial aid programs, consisting of grants, loans, and work-study programs.[7] Title IV Federal student loans can be categorized into (Stafford) subsidized loans, (Stafford) unsubsidized loans, Perkins loans and parent PLUS loans. Perkins loans are need-based subsidized loans that are disbursed by the educational institution and carry a fixed interest rate of 5%. One has to prove exceptional need to be eligible for Perkins loans. PLUS loans are taken out by the parents of a dependent student to help cover expenses remaining after having taken out subsidized and unsubsidized loans. Since students themselves do not in general pay back PLUS loans, we do not include them in our definition of Title IV loans.

The most prominent loans in terms of dollar volume and number of borrowers are subsidized and unsubsidized loans. Table 1 displays the main characteristics of these two types of loans during the period of interest (1987–1993) relative to subsequent years. While the names of these loans have changed over time—from Stafford loans to Subsidized Stafford loans and from Supplemental Loans to Students (SLS) to Unsubsidized Stafford loans—we will nevertheless refer to them as Stafford loans in this paper. Each loan type had its own program prior to the 1993, but collapsed to the single Federal Family Educational Loan Program (FFELP) thereafter. In addition, the Direct Loan Program was established as an alternative system of processing Title IV loans.[8]

There are two notable differences between subsidized and unsubsidized Stafford loans. The

[7] The following institutional details are mostly obtained from a National Center of Educational Statistics (NCES) report: Berkner (2000).

[8] While FFELP and the Direct Loan program co-existed for many years, FFELP ended with the enactment of the Health Care and Education Reconciliation Act of 2010. Since July 2010, the Direct Loan Program is the only source of federal student loans.

Table 1: Characteristics of Stafford Loans over Time

Academic years	Name of loan	Eligible students	Student loan Program
1987–88 to 1992–93	Stafford loans (subsidized)	Dependent and independent students	Guaranteed Student Loan Program
	SLS loans (unsubsidized)	Primarily independent students	Supplemental Loans to Students
1993–94 to 1998–99 (and after)	Subsidized Stafford loans	Dependent and independent students	Federal Family Education Loan Program (FFELP) William D. Ford Direct Loan Program
	Unsubsidized Stafford loans	Dependent and independent students	Federal Family Education Loan Program (FFELP) William D. Ford Direct Loan Program

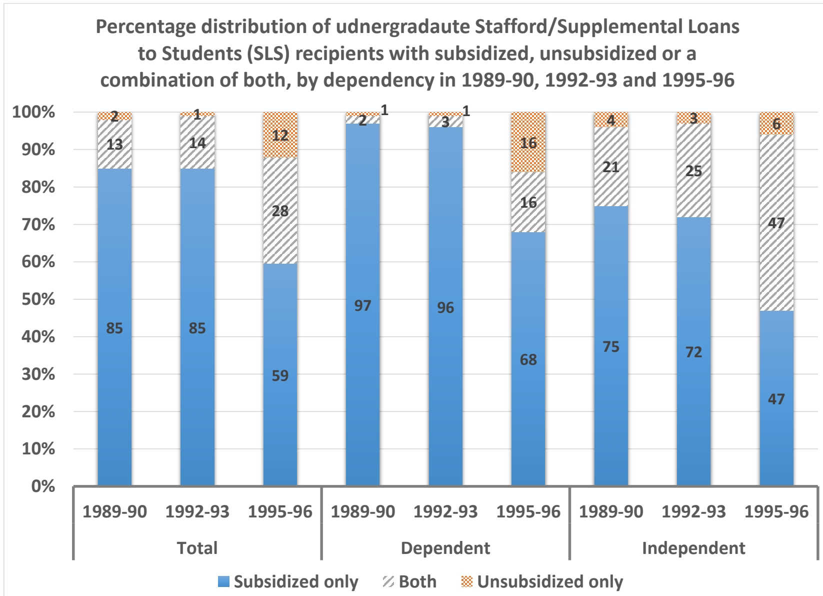
Source: Table 1 from Berkner (2000).

first one concerns the time at which interest begins to accrue on the loan (and sometimes they carry different interest rates as well). The second and more important from our perspective difference is their respective eligibility requirements. The requirement to qualify for subsidized loans has been consistent over time: a student must demonstrate financial need—this will be discussed in detail below. By contrast, the requirements to qualify for unsubsidized loans have varied over time, in particular with regard to dependency status.[9] Note that the dependency status is key not only for eligibility purposes, but also to determine financial need. While the income of parents plays a major role for dependent students, only student (plus spousal) income is considered for independent students.

As shown in Table 1, unsubsidized Stafford loans were primarily available to independent students from the 1987–88 academic year up to and including the 1992–93 academic year, academic years which fall under the 1986 Reauthorization Act. Dependent undergraduates whose parents applied but were rejected for parent PLUS loans—which require meeting creditworthiness criteria—were able to qualify for unsubsidized Stafford loans. While this

[9]Students under the age of 24 at the start of an academic year are considered dependent, unless they are married, have children or dependents for whom they provide more than half support, or are veterans, orphans or wards of the state (e.g. children in foster care), in which case they are classified as independent students.

Figure 1: Distribution of Subsidized and Unsubsidized Loans by Dependency Status.



Source: Figure 4 from Berkner (2000).

provision allows some dependent students to obtain unsubsidized loans at the independent student maximum amounts, in practice very few dependent students qualified for unsubsidized loans. Figure 1 displays the percentage distribution of subsidized and unsubsidized loans by dependency status. Among dependent borrowers, 97% and 96% had *only* subsidized loans in 1989–90 and 1992–93 respectively. By the 1995–96 academic year, the proportion of dependent borrowers with only subsidized loans had dropped to 68%.

The upshot is that during the years leading up to the 1992–93 academic year, Title IV loans accumulated by dependent students while in college consist almost exclusively of Stafford subsidized loans and Perkins Loans, both of which are need-based.[10] We now address how 'need' is determined and how variation derived from the formula for financial need naturally lead us to use a regression kink design.

[10]Over the years of interest, about 75% of independent students and 85% of the entire undergraduate student population only had subsidized loans. While our focus is on dependent students, we nevertheless investigate the robustness of our results to the inclusion of independent students.

2.2 Financial Need

Financial need is determined by comparing the *cost of attendance* (COA) to the student's ability to pay for these costs. COA is the sum of tuition, fees and other educational expenses (books, supplies, board, lodging, etc.). It is estimated for various categories of students by the financial aid office at each institution based on factors such as attendance status, dependency, and type of housing.

The ability to pay is measured by an index called the *expected family contribution* (EFC). Students wishing to receive federal aid for a given academic year must fill out the Free Application for Federal Student Aid (FAFSA) in the previous spring. Since FAFSA requires tax information, it is usually filled out after filing tax returns. The federal government determines EFC and provides the number to the student's institution based on the information provided by the student and their parents in FAFSA. The EFC is primarily based on income and assets with adjustments for family size and the number of family members enrolled in postsecondary education.

Need-based federal aid eligibility and the amount are determined by comparing the COA to EFC. If the EFC is greater than the COA (negative *need*), the student is not eligible for need-based aid. If the EFC is less than COA, the amount of aid for which the student qualifies is equal to COA minus EFC. If the student receives any grants or other aid, the amount is subtracted from need. Any remaining need may be covered by a subsidized loan, up to the annual limit.[11] Therefore, the amount *need* is defined as

$$need = COA - EFC - Grants.$$

To reemphasize, anyone with *need* less than or equal to 0 is ineligible for subsidized Stafford loans. Anyone with positive *need* is eligible for an amount equal to *need* up to the annual limit.[12] This change in the slope of the amount that can be borrowed around the need threshold value of 0—the slope is 0 for those below the threshold, and 1 for those above it—forms the basis for our regression kink (RK) design.

[11] The limit in 1992–93 was \$2,625 for students in their first or second year, and \$4,000 for students in their third or fourth year, with a total not to exceed \$17,250 for their entire undergraduate education.

[12] As discussed above, students with exceptional need could also qualify for Perkins loans, which are also subsidized Title IV loans with their own limit. In 1992–93, total Perkins loans could not exceed \$4,500 for students who had completed less then two years of a Bachelor's program, and \$9,000 for students who had completed two or more years toward a Bachelor's degree.

Once a student graduates from school (or enrolls less than half time, or drops out), she receives a six month grace period before repayments begin. Loans for individuals who enroll in graduate school automatically go into deferment, so no payments have to be made during this period.[13] During this six month grace period a borrower can choose one of several repayment plans. The most popular is the ten year standard repayment plan. Under this plan, individuals make fixed monthly payments deemed actuarially fair. If the interest rate on the loans is fixed or the loans were consolidated, the monthly payments would be the same for every month. The monthly payments change annually if the interest rate is variable, but remain constant during the year.

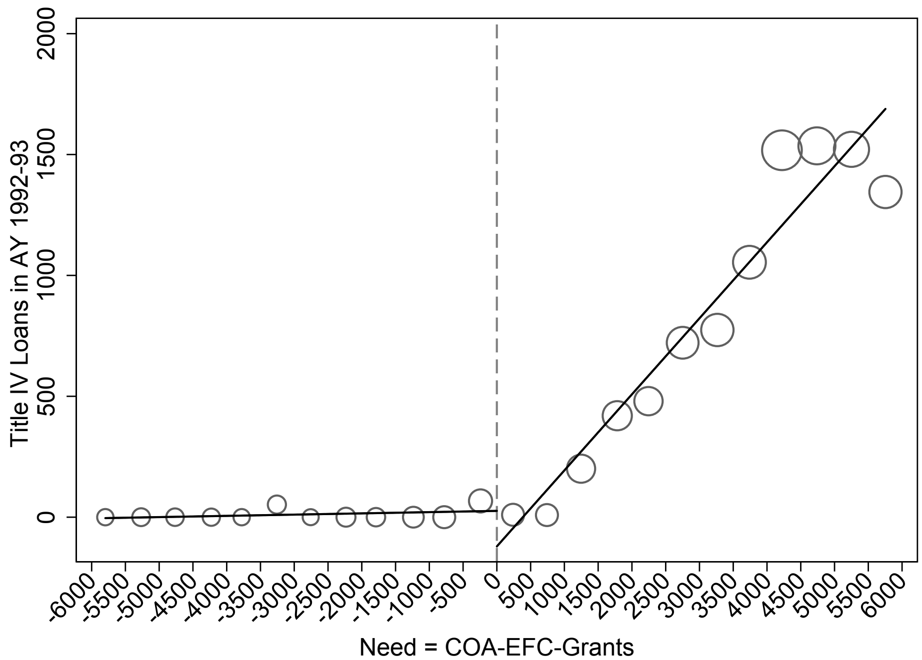
Figure 2 shows the empirical distribution of Title IV loans taken out in the last year of schooling as a function of *need* for our benchmark sample (see Section 3). While our analysis uses *cumulative* amount borrowed rather than borrowing in the last year alone, Figure 2 nevertheless serves as a useful illustration of the kink in the policy and a check on the quality of these data: ineligible students ($need < 0$) have no debt except for a few exceptions possibly due to measurement error. The kink at $need = 0$ is evident. At the same time, the relationship between *need* and amount borrowed is "fuzzy" in the sense that the slope of the line to the right of $need \geq 0$ is much less than 1. This less than perfect relationship is driven by both the extensive margin (58% of students with positive need do not take out any loan) and the intensive margin (conditional on borrowing at all, students borrow on average 54% of their need).

These low take-up rates may appear somewhat puzzling. If need as computed by the government is truly a student's need and it is not being filled by borrowing, what is? There are three possible answers. Either COA or EFC (or both) are mis-estimated, or students have other sources of income. First, COA, while provided by the institution, is nevertheless just an estimate. Certain components like tuition are easy to forecast. Others like room and board are not. For example, students that do not want to borrow but have high "need" could choose cheaper accommodations than the costs associated with the expected room and board.

Second, the fact that the word "expected" is in the name of the variable EFC gives away the fact that this is just a prediction and that in reality, parents can contribute more (or

[13] The grace period is nine months for Perkins loans, but these loans constitute a very small fraction of the student loan program.

Figure 2: Empirical Distribution of Title IV Federal Student Loans in the Last Year of Schooling as a Function of *need*

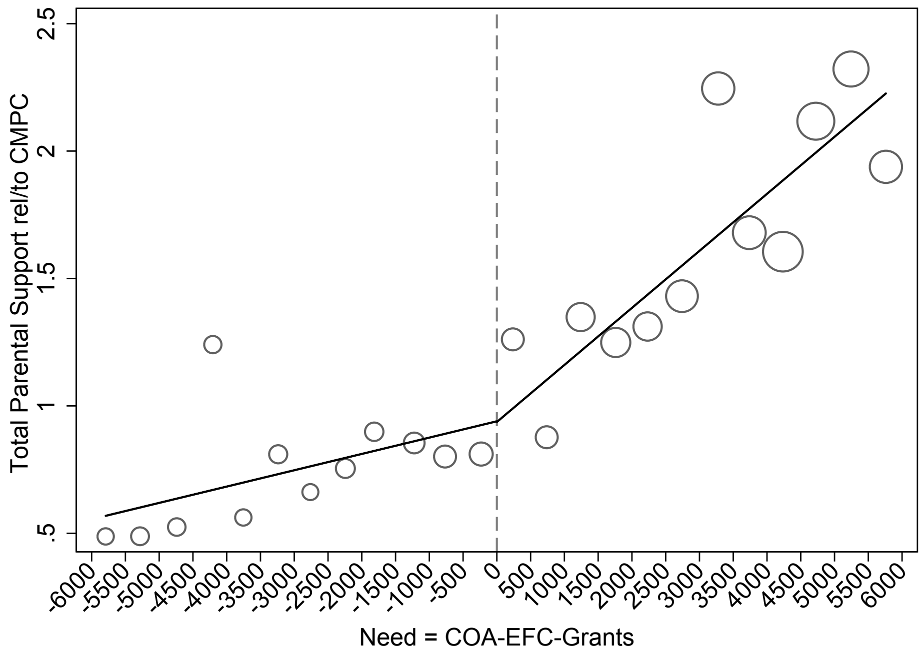


Notes: Bin size is \$500 with the center of each circle represents the average amount borrowed in the bin. The size of the circles reflect the number of observations in each bin. The best fit lines are estimated separately for $need < 0$ and $need \geq 0$ for our benchmark sample described in Section 3. We restrict attention to $|need| < 6,000$ to zoom in on the relationships close to $need = 0$.

less) than the EFC for their child's education.[14] In fact, information in the B&B allows us to compare the expected contribution of parents to at least an approximation of the actual family contribution: (i) students report the value of their parents' direct contribution, amount of loans from parents, in addition to in kind contributions; (ii) the EFC in the B&B is broken down into the expected contribution from parents (Congressional Methodology Parental Contribution, or CMPC) and the expected contribution from the student (Congressional Methodology Student Contribution, or CMSC). Figure 3 illustrates that the measured parental contribution relative to CMPC is in general increasing in *need*, and, in particular, much higher than 1 for students with positive need. In other words, eligi-

[14] EFC is not available for students who do not indent to apply for Federal aid. For these students, the Institute of Education Sciences (IES) computes the EFC according the the Congressional Methodology employed for students who do fill out the form. However, for students with insufficient information to allow such calculations, the IES estimates the EFC.

Figure 3: Actual Relative to Expected Parental Contribution as a Function of *need*



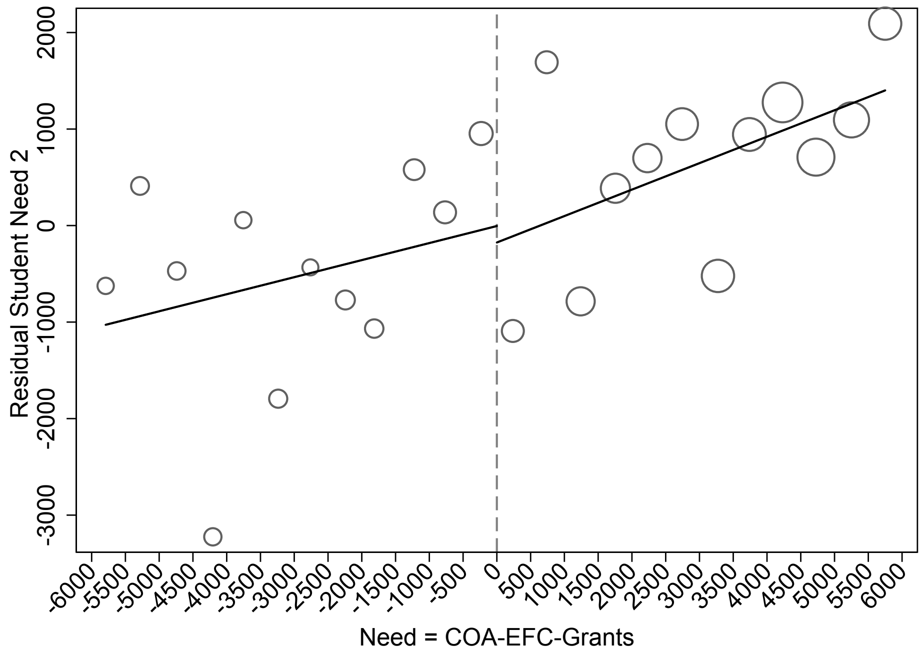
Notes: Bin size is \$500. The center of each circle represents the average amount borrowed in the bin. The best fit lines are estimated separately for *need* < 0 and *need* ≥ 0 for our benchmark sample described in Section 3. CMPC stands for Congressional Methodology for Parental Contribution.

ble students tend to receive more contributions from their parents than the Congressional Methodology assumes, thereby lowering their need to take on debt.

The final possibility is that students have other sources of income—that is, students can contribute more than their own portion of the Congressional Methodology. The most obvious source is labor earnings either through work-study awards or otherwise. While the propensity to work outside of school is fairly even across eligibility status, work-study awards are highly concentrated among eligible students: only 2.5% of students with negative *need* are work-study award recipients, compared to 17% of eligible students, with an average amount close to \$1,700. Many students also receive merit-based awards, as well as various kinds of assistantships, all of which can reduce their need to borrow.

Putting all of these factors together, we can compute a measure of unexplained or 'residual need' as follows. Starting with the original value of need, we subtract the extra contribution

Figure 4: 'Residual Need' as a Function of *need*



Notes: Bin size is \$500. The center of each circle represents the average amount borrowed in the bin. The best fit lines are estimated separately for $need < 0$ and $need \geq 0$ for our benchmark sample described in Section 3. 'Residual Need' is defined in the text.

from parents (this can of course be negative), as well as merit-based aid, work-study and assistantship amounts. We then subtract the amount of Title IV loans actually taken out. If a student's own labor earnings covers their part of the EFC, that is their labor earnings is exactly equal to their CMSC, then our measure of 'residual need' would be exactly zero.[15] Figure 4 shows this measure of residual need as a function of *need*. The discrepancy is still there for many students, but the low take up rate discussed above does not seem quite so puzzling in light of this figure, as the average residual *need* is not far from zero for either eligible (\$839) or ineligible (-\$176) students.

[15]While we have a measure of hours worked while enrolled, we cannot separately identify work-study hours from non-school based work hours. In addition, we do not have a measure of hourly wage.

3 Data and Sample Selection

The data source is the Baccalaureate and Beyond Longitudinal Study (B&B). The sample is derived from the 1993 National Postsecondary Student Aid Survey (NPSAS), a nationally representative cross-sectional sample of all post-secondary students in the U.S. The B&B itself is a nationally representative sample of students who received their bachelor's degree during the 1992–93 academic year. We emphasize the fact that this is a sample of students that finished college. We do not have any information on students who started but did not graduate.

Individuals in the B&B were initially surveyed as part of the 1993 NPSAS, with follow-up surveys conducted in 1994, 1997 and 2003. The study collects data at the administrative, institutional, and individual levels. The administrative data are linked to the National Student Loan Data System (NSLDS), which provides information on loan types and amounts disbursed throughout an individual's undergraduate study. The institutional-level data, besides providing information on individual demographics, provide the relevant information necessary to determine eligibility for need-based loans during the last year (1992–93) of schooling, in addition to institutional characteristics. Finally, individual survey data provides information on post-graduation labor market outcomes such as earnings, as well as many other outcomes that we investigate in Section 6.

Our primary outcome variable of interest is earnings, which we observe in 1994, 1997 and 2003. Specifically, this is a measure of individual's annual salary at the job they held in April of the survey year. We emphasize that it is self-reported like all the other dependent variables we consider. Because of worries about respondent error particularly at the top of the distribution, we trim the observations in the top 2% of the earnings distribution.

Our measure of student debt consists of Title IV loans for which the student is responsible for repayment, that is, Stafford loans plus Perkins loans—parent PLUS loans are not included. While our measure of debt includes both subsidized and unsubsidized Stafford loans, our measure of debt consists almost exclusively of subsidized loans because we focus on dependent students. We measure Title IV loans in the last year of schooling—the only school year for which we have institutional details on a student's need—as well as cumulative Title IV debt accumulated throughout an individual's undergraduate career.

The starting point of our sample consists of all individuals from the 1993 NPSAS that are eligible for the B&B survey, i.e. 11,200 students who graduated with a 4-year degree in 1992–93.[16] Of those, 10,080 responded to the 1994 interview. We lose around 360 observations because of missing characteristics such as age, citizenship, disability and dependency status. We drop non-US citizens (150) and students who report being disabled (340), leaving around 9,240 observations. Out of those, more than half are independent students (5,690). Since we are primarily interested in the impact of debt on earnings, we drop individuals who are enrolled at the time of the 1994 survey (1,310) as well as those with missing income data in 1994 (760). That leaves us with 3,580 observations. Finally, we drop about 20 observations on students who attended for-profit institutions. Our benchmark sample, then, consists of around 3,560 observations.

4 Empirical Framework

We are interested in the causal relationship between the log of annual income in 1994, (y), and cumulative Title IV loans borrowed, ($TitleIV$). To identify this relationship, we estimate the following:

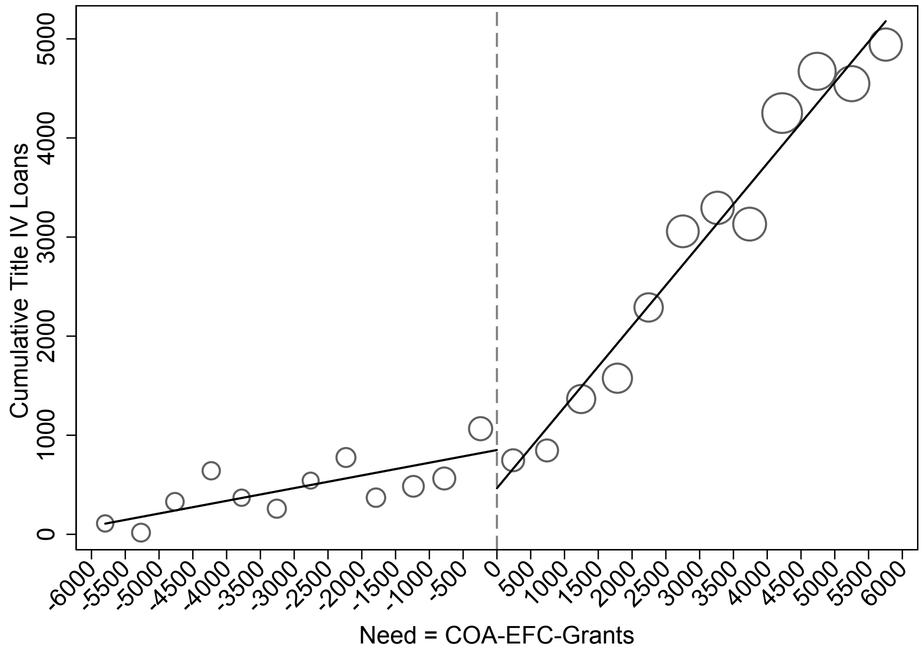
$$y = \tau \times TitleIV + g(need) + U \tag{1}$$

where U is a random variable that summarizes the effect of unobservable, predetermined characteristics, and g is a non-parametric function summarizing how *need* is related to post-graduation income y. The difficulty with estimating equation 1 as written is how to separately identify the effect of $TitleIV$ from the non-parametric effects of *need*. To solve this problem, we exploit the non-differentiability ("kink") of the statutory formula for how $TitleIV$ is determined as a function of *need* around the threshold of $need = 0$. The intuition is that if we assume that g is differentiable at $need = 0$, then any kinks in the relationship between $TitleIV$ and income must be due to the variation induced by the formula rather than unobservables that vary with *need*. One final note on the specification is that we will be estimating a semi-elasticity of log earnings on the level of debt. We chose this specification to handle the individuals that graduate with no debt.

Note that while we use the kink in the Stafford loan program for *the 1992–93 academic year as a function of need in 1992–1993*, which induces a kink in borrowing in the 1992–1993 academic year, we are interested in identifying the impact of *cumulative* student debt

[16]For confidentiality reasons, all references to number of observations are rounded to the nearest ten.

Figure 5: Empirical Distribution of Cumulative Title IV Federal Student Loans as a Function of *need*



Notes: Bin size is \$500 with the center of each circle represents the average amount borrowed in the bin. The size of the circles reflect the number of observations in each bin. The best fit lines are estimated separately for $need < 0$ and $need \geq 0$ for our benchmark sample described in Section 3. We restrict attention to $|need| < 6,000$ to zoom in on the relationships close to $need = 0$.

on earnings. However, a student's value for *need* can change from year to year based on parental income, say. So there might be students that have positive *need* some years and negative *need* other years. This will generate some "smoothing" out of the kink between *need* and cumulative borrowing. It is then an empirical question to what extent there is an identifiable kink that can be exploited. Figure 5 displays the empirical distribution of cumulative Title IV loans borrowed as a function of *need* in the final year of schooling (1992–93), where *need* is calculated as discussed above. There is still a clear kink around $need = 0$. At the same time, as expected, the kink is not as "sharp" as in Figure 2 which uses borrowing in 1992–93.[17]

Because we use cumulative borrowing, together with the incomplete take-up of loans even

[17] A different identification strategy that would exploit the sharper relationship between *need* and borrowing in AY 1992–1993 would be to use borrowing in the last year as the treatment and to control for total borrowing up to the final year of enrollment.

in the last year of enrollment as discussed in Section 2, we employ a fuzzy RK estimation strategy. Rather than imposing the statutory change in the slope (which would be 1 in our case) as in a sharp RK design, we estimate the kink in the relationship between cumulative borrowing and need in the last year. We can define the fuzzy RK estimator ($\hat{\tau}_{RK}$) as:

$$\hat{\tau}_{RK} = \frac{\lim\limits_{\varepsilon \downarrow 0}\left[\frac{\partial y | need=0+\varepsilon}{\partial need}\right] - \lim\limits_{\varepsilon \uparrow 0}\left[\frac{\partial y | need=0+\varepsilon}{\partial need}\right]}{\lim\limits_{\varepsilon \downarrow 0}\left[\frac{\partial TitleIV | need=0+\varepsilon}{\partial need}\right] - \lim\limits_{\varepsilon \uparrow 0}\left[\frac{\partial TitleIV | need=0+\varepsilon}{\partial need}\right]}.$$

The numerator measures the change in log income (y) as a function of *need*, and the denominator measures the change in cumulative loan amounts ($TitleIV$) as a function of *need*. So the RK estimator is based upon the relative changes in the slopes about the threshold. Notice the clear analog to the IV estimator where we think of *need* as the instrument and restrict attention to observations close to $need = 0$.

The assumptions necessary for the consistency of this estimator as discussed by Card et al. (2012) are: (1) the direct marginal impact of *need* on y is continuous i.e. $g(need)$ is continuous at 0; (2) the conditional density of *need* (with respect to U) is continuously differentiable at the threshold for Title IV loan eligibility; (3) monotonicity of the kink i.e., the change in the slope between $TitleIV$ and *need* is of the same sign for all individuals. This last assumption is clearly satisfied in our case since borrowing is an increasing function of *need*. Note that assumption (1) is untestable but is really the crucial identifying assumption. This assumption could be violated, for example, if unobservable characteristics correlated with *need* change in a "non-smooth" fashion around $need = 0$. One way to build support for the assumption is to show that, at least, *observables* flow smoothly around $need = 0$. It is worth emphasizing that because we are utilizing a kink rather than a discontinuity, observables need not to be constant around $need = 0$: they just need to change smoothly around the threshold. The second assumption, though, generates predictions about the density of *need* and the distribution of observable characteristics in the neighborhood of the threshold that we can and do test below.[18]

It is important to think about what exactly our estimator $\hat{\tau}_{RK}$ recovers. Our specification

[18]Card et al. (2012) impose an additional identifying assumption that the right and left limits of $TitleIV(need)$ are equal at the threshold. In theory (and practice), this assumption is satisfied by the *need* formula. Even if it were violated, under the assumption of locally constant treatment effects—or that $\frac{\partial y}{\partial need}$ does not vary in the neighborhood of the threshold—this assumption can be relaxed without affecting identification (Turner 2014). In particular, with locally constant treatment effects, the RK estimator, $\hat{\tau}_{RK}$, will identify the causal impact of cumulative Title IV loans on log earnings.

takes *need* as the running variable and focuses on the kink around $need = 0$. One could also consider EFC as the running variable and, in this case, there would be multiple kinks where $need = 0$ as a function of the cost of attendance. A student with $need = 0$ at a high cost school will have a higher EFC than a student with $need = 0$ at a low cost school holding fixed grants. By taking the running variable as *need*, we treat these students symmetrically whereas there might be reason to believe that the treatment effects differ across these groups of students. In this sense, our estimator $\hat{\tau}_{RK}$ is recovering an average of local average treatment effects where we are averaging across students with different levels of EFC that all end up at $need = 0$ because of differences in costs of attendance (or, to a lesser extent, grants). One might imagine that students with a lower EFC would be most sensitive to the effects of debt since, presumably, their parents have fewer resources with which the students could fall back on. In this case, our estimates perhaps understates the negative effects of debt for the neediest students.

There are a number of questions surrounding the implementation of the estimator $\hat{\tau}_{RK}$. In particular, how do we estimate the function g? The most common approach in the literature is to estimate g using low order local polynomials (Gelman and Imbens 2014). The question after choosing the order is how to set the smoothing bandwidth for these local polynomials.[19] This bandwidth is crucial since it controls how important observations far away from $need = 0$ are in estimating the behavior of g around $need = 0$. While there is relative agreement over the need for low order polynomials, there is considerably less agreement over how to pick the bandwidth. Imbens and Kalyanaraman (2012) were one of the first to study this question and proposed an optimal bandwidth based on minimizing the mean square error (MSE). Calonico et al. (2014) developed a different bandwidth selection procedure tailored to their new method for constructing confidence intervals and argued that this bandwidth choice should be paired with a local quadratic estimator. Recently, Card et al. (2016) have suggested that many of these adjustments to the choice of the bandwidth such as regularization aimed to solve particular problems end up causing more problems. This is because the adjustments themselves need to be estimated and, in many cases, the available estimates are not particularly precise or are biased in small sample. Instead they found that a local linear approximation has less bias and the bias correction proposed in Calonico et al. (2014) leads to a large loss of precision. In light of these findings, we will use a local linear polynomial and the MSE optimal bandwidth where we will allow the bandwidth to

[19] Of secondary importance is the choice of the kernel. We use a triangular one.

be different on either side of $need = 0$.[20] Finally, Calonico et al. (2016a) show how to include additional covariates in the RK specification. While in a well-identified RK design the addition of covariates should not have much effects on the point estimate of τ, they may help improve the precision of the estimate and so we include a set of demographic controls.

5 Evaluating the RK Assumptions

We evaluate the RK identifying assumptions through two exercises. First, we test for discontinuities in the slope of the distributions of observable characteristics including age, gender, race, log parental income, parental education and family size. Second, we test for a discontinuity in the density of *need* at the threshold by estimating the density at $need = 0$ from the right and the left and then comparing the two.

Focusing on observable characteristics, we begin with Table 2, which displays some basic demographic characteristics of our sample by eligibility status. The first column shows characteristics for those respondents who were ineligible for a subsidized loan in the last year of their undergraduate degree program ($need < 0$) while the second displays characteristics for students who were eligible ($need > 0$). Table 2 is divided into two panels: student demographics characteristics and educational related costs. While in the regressions we will include all students and downweight individuals far from $need = 0$ through the choice of the bandwidth (and kernel), here to make the comparison as "fair" as possible, we restrict attention to students with *need* close to the threshold by only including students with $|need| < 6,000$.

Panel A shows that demographic characteristics across ineligible and eligible borrowers are relatively similar. There are only minor differences in fraction male, age, SAT,[21] and family size and some slightly larger differences for the selectivity of the school[22] and whether it

[20]The estimation is implemented by the Stata package rdrobust (Calonico et al. 2016b). It offers a whole array of options for bandwidth selection as well as the order of the polynomial approximation.

[21]For those students who had only an ACT score, we used an ACT-SAT conversion table to determine a comparable SAT score. This conversion table is the outcome of a joint study by the ACT and the College Board, which conducts the SAT: https://www.act.org/content/dam/act/unsecured/documents/ACT-SAT-Concordance-Tables-Report.pdf.

[22]The variable 'selective' refers to the level of selectivity of the institution. The IES builds an index of selectivity based on the percentage of students that were admitted to each institution (of those who applied), and the SAT/ACT scores of that pool of students. Schools are classified into one of 5 levels of selectivity: from open admission to most selective. We refer to an institution as being 'selective' if it is either most or

Table 2: Characteristics of Respondents by Title IV Loan Eligibility

	Ineligible	Eligible
A. Student demographic characteristics		
Age	21.7	21.8
Male	0.431	0.404
Black	0.034	0.055
Hispanic	0.014	0.048
SAT (converted)	1071	1051
Income of Parents 1991	66.6	46.0
Less than Bachelor	0.367	0.536
Parent Bachelors or Higher	0.313	0.241
Masters and Higher	0.318	0.222
Selective School	0.365	0.321
Public	0.791	0.728
B. Education related costs		
COA	11.4	11.8
EFC	13.0	6.62
Grants	1.10	1.82
Cumulative Title IV Loans	0.50	3.29
N	410	1140

Notes: A student is ineligible if $need < 0$. The variable "Cumulative Title IV Loans" is the unconditional cumulative amount borrowed for undergraduate degree. Students with $|need| > 6,000$ are excluded. Variables measured in dollars are all nominal and in units of \$1,000.

is public. As might be expected, the biggest differences are in terms of ethnicity, parental education, and parental income. Turning to Panel B, we find that there are not large differences in the cost of attendance between eligible and ineligible students. The biggest differences are, not surprisingly, in terms of EFC and, to a lesser extent, grants. Finally, there is a clear difference in the unconditional cumulative average amount borrowed by individuals based on whether they are eligible in their *last* year. This reinforces Figure 5, which showed the kink in cumulative borrowing around *need* in the last academic year before graduation.

We now present some graphical evidence on and formal tests of the relationship between

very selective.

Table 3: Relationship between *need* and Predetermined Characteristics

	Age	Male	White	Log Income of Parents 1991	Parental Education	Family Size	Predicted Earnings
RK_Estimate	0.050*	0.013	-0.015*	-0.039	-0.023	-0.009	0.002
	(0.026)	(0.013)	(0.008)	(0.026)	(0.019)	(0.016)	(0.002)
Lower BW	11.06	7.91	6.96	11.67	21.25	15.93	8.17
Upper BW	4.89	8.98	10.81	3.97	4.71	14.90	12.14
N	2950	2950	2950	2950	2950	2950	2950

Notes: Heteroskedasticity robust standard errors are in parenthesis. We use a local linear polynomial approximation and the MSE optimal bandwidth with a possibly different bandwidth on either side of the threshold and no regularization adjustment. The kernel is triangular. The variable *need* is in units of $1,000.

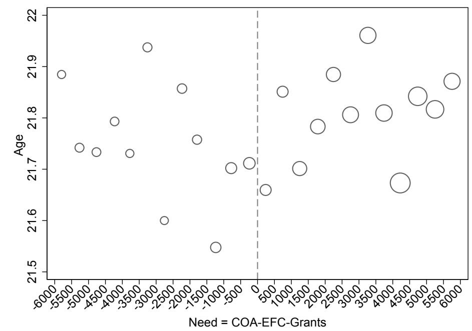
need and some demographics around $need = 0$. First, Figure 6 shows that there are no clear discontinuous changes in the slope of the relationship between *need* and various observables. To formally test whether there are such kinks, we run RK specifications where the dependent variable is some predetermined characteristic and the running variable is *need*. We estimate the specification using local linear polynomials and use the MSE optimal bandwidth with possibly different bandwidths on either side of the threshold. Table 3 shows the results for age, gender (male), race (white vs non-white), parental income, parental education (college vs less than college) and family size. For all but age and race (and these are only significant at the 10% level), we fail to reject the null of no kink.[23] We can also do a test based on a suggestion in Card et al. (2016). The idea is to use all the demographic characteristics to predict earnings in 1994 and then to estimate if there is a kink in predicted earnings as a function of *need*. These results are presented in the last column of Table 3 and displayed in Panel (f) of Figure 6. Neither suggests the presence of a kink.

One of the conditions for consistency of the RK estimator is that the density of *need* should be continuous at the cutoff. If it were not continuous, this would cast doubt on the implicit assumption that after controlling flexibly for *need*, it is random whether a student ends up slightly above or below $need = 0$. While we will test this assumption formally, there are

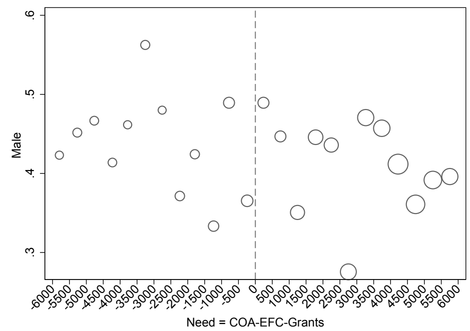
[23]The age effect, while statistically significant, is economically insignificant given the very limited range of age in our sample.

Figure 6: Distribution of Baseline Characteristics

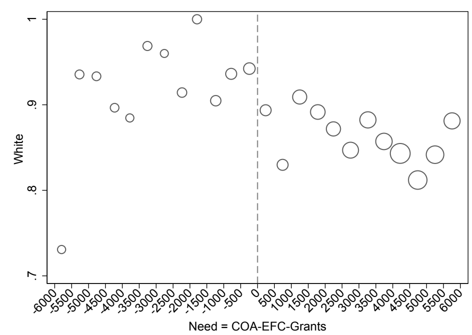
(a) Age



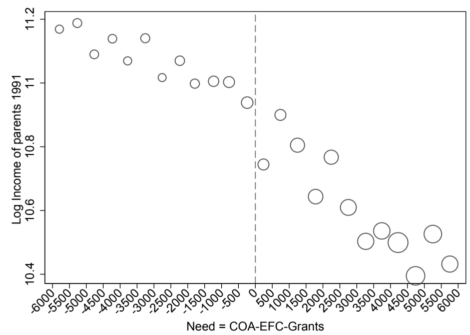
(b) Male



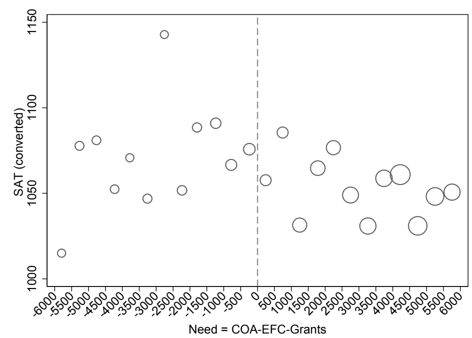
(c) White



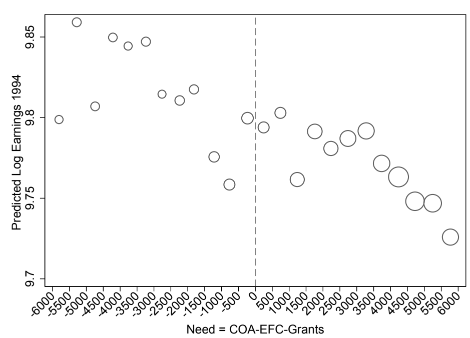
(d) Log Parental Income



(e) SAT



(f) Predicted Earnings

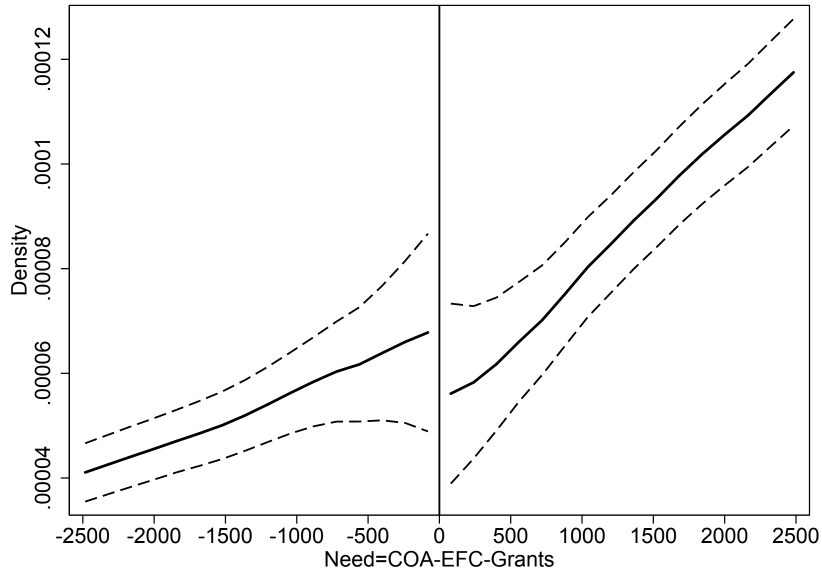


Notes: Bin size is \$500 with the center of each circle represents the average amount borrowed in the bin. The size of the circles reflect the number of observations in each bin. The best fit lines are estimated separately for $need < 0$ and $need \geq 0$ for our benchmark sample described in Section 3. We restrict attention to $|need| < 6,000$ to zoom in on the relationships close to $need = 0$. For Panel 6f, the dependent variable is the predicted value from earnings run on age, gender, race, SAT score, merit aid, parental education, parental income, parental family size, school quality, and indicator for public (vs. private) school.

compelling practical reasons why it would be difficult to affect marginal changes in *need*. It is, of course, possible to imagine that students could cause rather drastic changes in *need* by moving between schools with large differences in tuition This possibility, however, is rather remote for students in their final year of college. Putting that possibility aside, a student is left to attempt to manipulate either EFC or grants. There are clear hurdles to manipulating EFC due to the complexity of the formula used in determining EFC and the fact that much of the information used to determine EFC is drawn from the parents' tax returns, which are likely to be accurate. Grants, the third component of *need*, are usually determined by factors directly out of the control of the student. Most grant related factors, moreover, are not known at the time of filing for FAFSA (some are dependent on FAFSA), thereby making it an instrument that is hard to precisely manipulate. We note as well that the marginal benefits in terms of additional aid from marginal changes in *need* are not that large in terms of additional *need*. Remember that we are exploiting a *kink*, not a discontinuity.

With these practical considerations in mind, we formally test for whether the distribution of the running variable, *need*, is continuous at $need = 0$. Following McCrary (2008), we plot the density of *need* around 0 in Figure 7. It is clear that we cannot reject the null that the left and right limits of the density are equal as *need* approaches 0 since the confidence intervals on either side of the threshold have a non-empty intersection. Note that the estimated difference in the density is even the "wrong" sign with a jump down in the density. One would imagine that, if anything, students would attempt to manipulate their *need* to be slightly higher, which would lead to a jump up in the density of *need* around $need = 0$. This is clearly not the case.

Figure 7: Distribution of *need* around $need = 0$



Notes: Estimated density of *need* is solid line and standard errors of that estimate are dashed lines based on McCrary (2008).

6 Results

We now present results from our main specification, which, as discussed in Section 4, uses a local linear polynomial in *need* and a two-sided MSE optimal bandwidth without regularization. We first explore the impact of debt on earnings, as well as its decomposition into wages and hours worked. For the main outcomes variables, we report results with and without controls. Controls include age, gender, race, SAT score, merit aid, parental education, parental income, parental family size, school quality, and indicator for public (vs. private) school. Recall that we removed the very few students who attended private for-profit schools. As such, all private schools in our sample are not-for-profit institutions. To maintain a consistent sample throughout the analysis, we remove observations with any missing control variable information: this reduces our benchmark sample size from 3,560 to 2,950 for 1994. Of those, income data for 1997 (2003) is available for 2,350 (2,130) individuals, which form our benchmark samples for outcomes in those respective years. Note that by including parental income (and family size) directly, we are controlling for any effects of parental income (and family size) beyond the direct effect it has on *need* through the formula for EFC.

6.1 Income effects

Panel A of Table 4 shows estimation results for 1994 log earnings, where Title VI loans are in 1,000's of dollars. The impact of debt on log earnings is negative but not significantly different from zero at conventional levels. The interpretation of the coefficient is that an extra \$1,000 in total debt at graduation lowers earnings by about 2.5%. This is an economically meaningful amount as the average difference in cumulative borrowing between those that are eligible versus those are not eligible in the last year is over \$2,500. Figure 8 graphically shows the origin of this result. The slope of earnings right of the threshold is steeper than the slope of earnings left of the threshold. Combined with the positive kink in the relationship between *need* and cumulative loans, this delivers the negative point estimate. The fact that the estimate is stable when we add a battery of controls is reassuring. If individual characteristics flow smoothly about the threshold ($need = 0$), as they seem to in Figure 6, controls should not have a large impact on the estimate. We offer a number of robustness checks in the Appendix.[24]

Panels B and C of Table 4 present medium and long-run results for earnings in 1997 and 2003, that is, the impact of student debt on earnings 4 and 10 years post graduation. Besides the 2003 estimate with controls, all of the other estimates are very close to zero in any meaningful sense. Note any effects here are not due to large remaining student debts since most student debt has a 10 year maturity. By 1997, students will have paid down a substantial part of what they borrowed and practically all of it by 2003. In this respect, it is perhaps not surprising that the effects are small since whatever mechanism that generated the initial sharper negative relationship between student debt and earnings may no longer be operating. Our short term effects combined with limited long term effects are consistent with those found in a paper by Minicozzi (2005) using a different survey and different identification strategy, as well as those in Weidner (2016). That said, these results do stand in contrast to some other literature that suggests that shocks particularly at the start of a career can have persistent effects. For example, Oreopoulos et al. (2012) find that graduating into a recession has permanent negative consequences on earnings.

[24] These include an IV strategy where we vary the *need* bandwidth. We also ran a simple OLS specification and a partially linear model. We also consider variations on the RK estimation strategy: (1) Epanechnikov kernel, (2) one bandwidth for both sides of the cutoff, (3) the bandwidth selection method of Calonico et al. (2014), and (4) regularization in the bandwidth selection. We also experiment with various sub-samples including putting independent students back in to see if results are similar for various sub-groups of the sample.

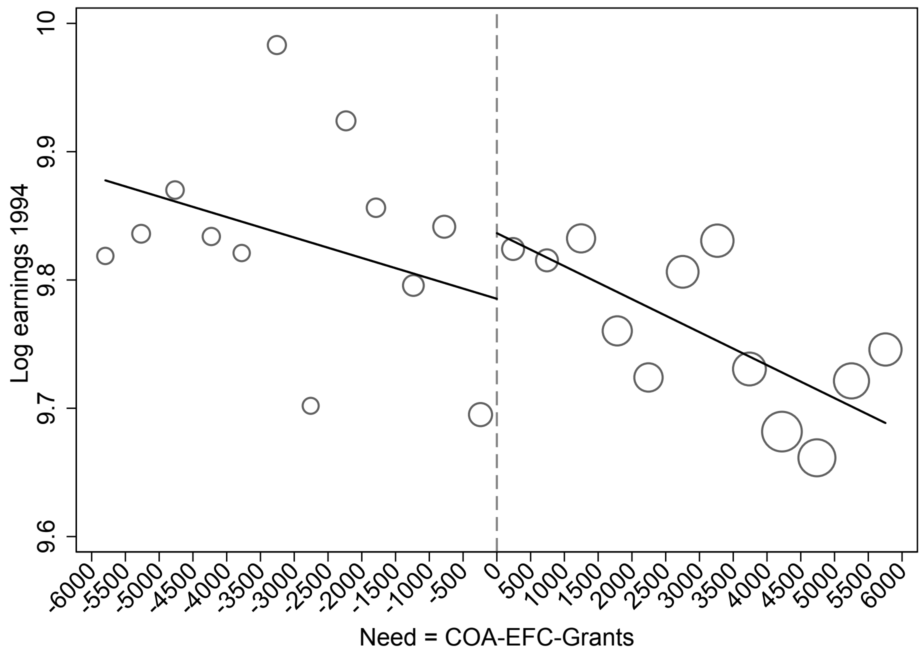
Table 4: Effect of Title IV Loans on Log Earnings

	Log Earnings	
Panel A: 1994		
RK_Estimate	-0.025	-0.027
	(0.023)	(0.019)
Lower BW	28.24	35.91
Upper BW	4.32	4.65
Controls	Yes	No
N	2950	2950
Panel B: 1997		
RK_Estimate	-0.006	-0.003
	(0.014)	(0.015)
Lower BW	32.84	19.10
Upper BW	10.28	8.56
Controls	Yes	No
N	2350	2350
Panel C: 2003		
RK_Estimate	-0.035	0.003
	(0.036)	(0.018)
Lower BW	15.27	20.71
Upper BW	4.29	7.00
Controls	Yes	No
N	2130	2130

Notes: Heteroskedasticity robust standard errors are in parenthesis. We use a local linear polynomial approximation and the MSE optimal bandwidth with a possibly different bandwidth on either side of the threshold and no regularization adjustment. The kernel is triangular. We drop any observations that have any missing values for the controls to keep the sample consistent across specifications within a year. Title IV loans are in units of $1,000. Controls include age, gender, race, SAT score, merit aid, parental education, parental income, parental family size, school quality, and indicator for public (vs. private) school.

The negative effect of debt on log earnings can be decomposed into effects on the log of the wage rate and log hours worked. Note that while we keep the sample consistent

Figure 8: Reduced Form Impact of *need* on Log Earnings



Notes: Bin size is \$500 with the center of each circle represents the average amount borrowed in the bin. The size of the circles reflect the number of observations in each bin. The best fit lines are estimated separately for $need < 0$ and $need \geq 0$ for our benchmark sample described in Section 3. We restrict attention to $|need| < 6,000$ to zoom in on the relationships close to $need = 0$.

across the regressions for these dependent variables, the point estimates will still not "add up" exactly as in a basic linear regression. The reason is that we allow the bandwidth to differ by dependent variable. Panel A of Table 5 shows that the impact on hours worked is large and significant, statistically and economically, over the first few years, but becomes muted by 2003. On the other hand, Panel B of Table 5 shows that debt has a negative but statistically insignificant impact on wages. Furthermore, while the large effect of debt on wages 10 years after graduation is surprising—albeit consistent with the large effect on 2003 earnings with controls—it may be because our measure of hourly wage in 2003 is not as precise as it is for other years. For 1994 and 1997, individuals are asked about the amount of their last paycheck as well as the frequency of their paychecks, in addition to the number of hours worked per week, from which we construct hourly wage. In 2003, the only available information is annual salary and weekly hours worked. As such, our measure of hourly wages in 2003 is very imprecise. Overall, this shows that most of the effects on earnings are coming

Table 5: Effect of Cumulative Title IV Loans on Hours Worked and Wages

	1994	1997	2003
Panel A: Log Hours Worked			
RK_Estimate	-0.024***	-0.041***	-0.004
	(0.009)	(0.016)	(0.011)
Lower BW	14.25	71.94	14.77
Upper BW	5.99	4.15	16.51
Mean	3.68	3.71	3.75
N	2950	2350	2130
Panel B: Log Hourly Wage			
RK_Estimate	-0.011	0.005	-0.040
	(0.016)	(0.016)	(0.028)
Lower BW	33.86	38.51	34.34
Upper BW	4.94	7.59	4.38
Mean	2.16	2.49	3.02
N	2950	2350	2130

Notes: Heteroskedasticity robust standard errors are in parenthesis. We use a local linear polynomial approximation and the MSE optimal bandwidth with a possibly different bandwidth on either side of the threshold and no regularization adjustment. The kernel is triangular. We drop any observations that have any missing values for the controls to keep the sample consistent across specifications within a year. The dependent variables here are defined to be missing if the corresponding earnings variable is missing. So the sample here is also consistent with Table 8, Title IV loans are in units of $1,000. Controls include age, gender, race, SAT score, merit aid, parental education, parental income, parental family size, school quality, and indicator for public (vs. private) school.

through effects on hours worked rather than wages, and only for the first few years after graduation.

6.2 Effects on Search Behavior

One potential way in which debt could affect an individual's success in the labor market is through effects on search effort. For example, **?** builds just such a search model to examine the effects of debt on search and labor market outcomes. For example, a student needing to

Table 6: Effect of Cumulative Title IV Loans on Labor Search Behavior

	Occupation Change from 1994 to 1997	Number of Jobs 1994 to 2003	Number of Interviews	Months Searching
RK_Estimate	-0.013	0.099	0.057	0.279
	(0.012)	(0.094)	(0.142)	(0.305)
Lower BW	31.70	14.30	44.25	38.51
Upper BW	8.35	5.98	9.92	7.98
Mean	0.48	5.07	5.75	2.60
N	2350	2080	2690	2200

Notes: Heteroskedasticity robust standard errors are in parenthesis. We use a local linear polynomial approximation and the MSE optimal bandwidth with a possibly different bandwidth on either side of the threshold and no regularization adjustment. The kernel is triangular. Title IV loans are in units of $1,000. All regressions also include controls for age, gender, race, SAT score, merit aid, parental education, parental income, parental family size, school quality, and indicator for public (vs. private) school.

pay back debt that is not dischargeable may be more interested in finding a job quickly than one that is the best fit. In addition, students with high debt may be hesitant to switch jobs. Table 6, which examines the effects of debt on some of these variables, suggests that these mechanisms are not borne out by the data. We find a small positive effect on the number of job interviews, but also a non-trivial positive effect on the number of months individuals search for their first job after graduation. And while debt makes individuals less likely to have switched occupation from their job in 1994 to that in 1997, it does make individuals more prone to experience more jobs over the first 10 years of their career. The fact that debt leads to more job hopping need not be seen in a negative light. Gervais et al. (2016) show that job hopping may be part of a necessary process to find a good match, especially early in individuals' working career. The fact that the effects disappear 10 years after graduation is suggestive that such a process is indeed at work.

To investigate this further, we now turn to potential explanations for this job hopping, namely job satisfaction and 'mismatch' between a person's job and a person's qualifications. As noted earlier, it is possible that new graduates with higher debt levels are less choosy when it comes to accepting job offers. As such, they may be more inclined to accept jobs that are less related to their degree and offer limited career potential. Table 7 shows that this is indeed the case for these self-reported measures of job 'mismatch' and satisfaction,

Table 7: Effect of Cumulative Title IV Loans on Self-Reported Job Satisfaction and Career Mismatch

	1994 Job Closely Related to Degree	1997 Job Closely Related to Degree	1994 Job: Career Potential	1997 Job: Career Potential	1994 Job: Satisfied with Pay	1997 Job: Satisfied with Pay	2003 Job: Satisfied with Pay
RK_Estimate	-0.022**	-0.003	-0.011	-0.022*	-0.015*	-0.017	0.018
	(0.010)	(0.011)	(0.014)	(0.012)	(0.009)	(0.019)	(0.032)
Lower BW	23.29	31.83	18.54	38.61	25.59	15.56	17.05
Upper BW	11.26	10.92	18.52	8.01	8.78	4.49	3.90
Mean	0.50	0.51	0.38	0.55	0.78	0.87	0.65
N	2950	2350	2940	2350	2940	2350	2120

Notes: Heteroskedasticity robust standard errors are in parenthesis. We use a local linear polynomial approximation and the MSE optimal bandwidth with a possibly different bandwidth on either side of the threshold and no regularization adjustment. The kernel is triangular. Title IV loans are in units of $1,000. All regressions also include controls for age, gender, race, SAT score, merit aid, parental education, parental income, parental family size, school quality, and indicator for public (vs. private) school.

and significantly so. While the effect on work unrelated to degree is only prominent the first year after graduation, that on career potential remains prominent 4 years later. In addition, students with debt are initially less likely to be satisfied with their pay, although this effect is overturned 10 years after graduation.

These effects are large in an economic sense if we compare the point estimates to the mean fraction of students being in a 'mismatched' job, for example. In particular, an additional $2,500 in debt reduces the probability of being in a closely related job by almost 5 percentage points relative to a mean of 0.50. While there may be several reasons underlying this finding, a prominent one which we emphasized above might be that individuals with debt cannot afford to be choosy in the months after graduation, as debt repayments are set to start.

6.3 Effects on Graduate School Enrollment Immediately After

One possible explanation for the various negative effects discussed above is that those with high debt enroll in graduate school to defer payment on loans, and earn higher income later in their career. Recall that our benchmark sample excludes individuals who are enrolled in school in 1994, most of which are attending graduate/professional schools. This would explain why we find negative effects on impact due to differential selection of individuals with high debt into graduate school. However, we find the exact opposite effect.

Table 8 reports the RK results of student debt on graduate school attendance in 1994 using our usual specification, but with a sample that includes all students who were enrolled in 1994. We find that increasing student debt by $1,000 decreases graduate school enrollment immediately after graduating by a little under 1 percentage point thought it is not statistically significant. This is a non-trivial effect relative to the mean of about 10% of students who go on to graduate school immediately. At a minimum, there are not strong positive effects here of debt on graduate enrollment. So rather than suggesting students with high debts use graduate school to simply defer paying down their debts, these results imply that increased debt levels "force" students to bypass graduate school in order to get a job to pay down their debt. Our results are similar in magnitude to those in Monks (2001). The question then becomes whether these (slightly) negative effects on immediate enrollment in graduate school translate into persistently low levels of graduate and professional degree attainment by 2003, which we study below.

6.4 Effects While in School

Another explanation for the effects on earnings and other labor market outcomes is that debt affects students while they are enrolled in school, including effects on GPA and major choice. There have been a number of papers that have attempted to identify the returns to GPA for earnings (Wise 1975; Jones and Jackson 1990). Most of these studies have found a positive relationship between the two (though less is known about whether this effect is persistent). So this is at least a plausible channel through which debt may affect earnings.

Table 9 shows the results for overall and major GPA. The impact of debt on GPA, either cumulative or major, is striking. A $1,000 increase in student debt decreases GPA by 0.023 points. Extrapolating to average debt at graduation, the effect of debt on GPA, at about 0.2,

Table 8: Effect of Cumulative Title IV Loans on Graduate School Attendance in 1994

	In Grad. School in 1994?	
RK_Estimate	-0.007	-0.011
	(0.007)	(0.008)
Lower BW	17.79	34.30
Upper BW	7.28	5.53
Controls	Yes	No
Mean	0.10	0.10
N	4730	4730

Notes: Heteroskedasticity robust standard errors are in parenthesis. We use a local linear polynomial approximation and the MSE optimal bandwidth with a possibly different bandwidth on either side of the threshold and no regularization adjustment. The kernel is triangular. Title IV loans are in units of $1,000. All regressions also include controls for age, gender, race, SAT score, merit aid, parental education, parental income, parental family size, school quality, and indicator for public (vs. private) school.

is sizable. It is important to note here that these effects are large even after controlling for ability (through SAT scores) as well as socioeconomic background more generally (through parental income and other demographics).[25] One possible explanation for this negative effect on GPA is that students with higher levels of *need* end up working more while enrolled, which reduces the amount of time available for studying (DiSimone 2008).

Second, a long literature (Rumberger and Thomas 1993; Arcidiacono 2004; Hamermesh and Donald 2008) has documented the differences in returns to college major even after controlling for sorting based on ability. So one possible explanation for the effects on income could be due to effects of loans on major choice. To the extent that major choice particularly affects first job placement, this may be a channel through which borrowing matters. If students are risk averse and uncertain about their ability to graduate, higher levels of debt may push them to choose majors with higher chances for graduation even if the expected return is lower. Other work by Kinsler and Pavan (2015) finds large differences in earnings *within* major categories: they argue that students are uncertain about their future productivity. This would suggest that risk averse students with relatively high levels of debt would tend to select majors that have lower *ex ante* risk. To address these possible theories for a link between major choice and debt, we classify majors into broad categories and examine

[25]This lead us to consider a specification with GPA as a control variable. The impact on earnings are very similar when we control for cumulative GPA, although 1997 looks more like 1994 under this specification.

Table 9: Effect of Cumulative Title IV Loans on GPA and Major Choice

	Cumulative GPA	Major GPA	Humanities	Education	Business
RK_Estimate	-0.023*	-0.023**	0.022	-0.007	-0.009
	(0.013)	(0.010)	(0.019)	(0.009)	(0.025)
Lower BW	42.37	18.51	22.81	15.44	13.41
Upper BW	6.09	9.74	4.01	15.32	4.37
Mean	3.11	3.27	0.13	0.15	0.16
N	2840	2730	2950	2950	2950

Notes: Heteroskedasticity robust standard errors are in parenthesis. We use a local linear polynomial approximation and the MSE optimal bandwidth with a possibly different bandwidth on either side of the threshold and no regularization adjustment. The kernel is triangular. Title IV loans are in units of $1,000. All regressions also include controls for age, gender, race, SAT score, merit aid, parental education, parental income, parental family size, school quality, and indicator for public (vs. private) school.

whether borrowing affects graduating with a particular kind of major.

We focus on 3 major categories: humanities, education and business and estimate a separate specifications for whether a student majors in one of these categories. This approach, rather than estimating say an unordered logit, allows us to maintain continuity with the specifications used in earlier sections. Consistent with the temporary effects of loans on earnings, we find limited effects on major choice. Perhaps there is a slight shift towards humanities majors and away from business and education. Note that, in some sense, it would have been surprising to find effects here since the effects of major choice on earnings tend to be permanent (Arcidiacono 2004).

6.5 Effects on Later Educational and Demographic Outcomes

Earlier literature has argued that student debt has broader, longer-term effects beyond just short-term labor market outcomes, including effects on obtaining a graduate or professional degree (Monks 2001), marriage (Gicheva 2016), fertility (Shao 2014), and homeownership (Mezza et al. 2014). We reexamine the effects of debt on these variables here using the same identification strategy. Notice that the effects we will estimate are "total" effects of student debt in that they include direct effects of debt on these outcomes as well as any other indirect effects. For example, as we showed earlier, there is some evidence that debt affects earnings,

Table 10: Effect of Cumulative Title IV Loans on Other Educational and Demographic Outcomes

	Graduate/Prof Degree by 2003	Married 2003	Children 2003	Homeowner in 2003
RK_Estimate	-0.018	-0.014	0.012	-0.037
	(0.026)	(0.011)	(0.022)	(0.023)
Lower BW	33.02	28.77	16.91	19.16
Upper BW	3.90	13.59	5.51	4.18
Mean	0.23	0.70	0.57	0.74
N	2130	2130	2130	2080

Notes: Heteroskedasticity robust standard errors are in parenthesis. We use a local linear polynomial approximation and the MSE optimal bandwidth with a possibly different bandwidth on either side of the threshold and no regularization adjustment. The kernel is triangular. Title IV loans are in units of $1,000. All regressions also include controls for age, gender, race, SAT score, merit aid, parental education, parental income, parental family size, school quality, and indicator for public (vs. private) school.

which would presumably affect these outcomes as well.

Table 10 shows that the signs of our estimates, except for the presence of children, are consistent with the negative effects of debt found in the literature. At the same, the magnitudes of some of the effects are not particularly large. For example, an increase of $1,000 of debt decreases the probability of being married in 2003 by 1.4 percentage points. The effects of homeownership are much larger in magnitude but also much less precisely estimated. We noted earlier that debt decreases the probability of enrollment in graduate school immediately following graduation. We now see that this effect is persistent at least in terms of the point estimate. The effect of student debt among individuals who did not pursue a graduate education immediately after graduation is indeed larger in the long run than soon after graduating, albeit the coefficient is not statistically significant in the long run.

7 Conclusion

While the recent rise in federal student loan debt is well documented, there is considerable debate over the economic consequences of this increase. Our contribution to this debate is to establish the relationship between student debt and various post-graduation outcomes both in the short and long runs. Using data from the Baccalaureate and Beyond Longitudinal Study: 1993/03, we implement a regression kink (RK) design to determine the causal impact of debt on earnings. Key to this design is that up to 1993, dependent students could only borrow need-based Title IV loans.

We find limited negative effects of student debt on earnings in the short run, and little evidence that this effect persists in the long run. The short run impact comes from the effects of debt on hours worked rather than wages. Part-time work may be behind this finding. More generally, we find some evidence that graduating with debt leads individuals to accept job that are not ideal. Debt also makes individuals less likely to be unemployed, at least in the early years after graduation. Finally, there is some though limited evidence that graduating with debt affects individuals' future demographic outcomes such as reduced homeownership rates and attending graduate school.

The question then is how we can reconcile our rather mild (though negative) effects with the worries that are pervasive across the political spectrum. We can imagine at least three reasons why our estimates based on the 1993 cohort might not be directly applicable to the 2008 cohort—the next wave of B&B data. The first and probably most promising is the large difference in size of the average debt load. Perhaps these local average treatment effects estimated from a sample where the average debt even for the eligible student is less than $3,500 (or about $5,700 in 2016 dollars) are just not applicable to today where the average debt load is around $37,000. Another possibility is the rise of income contingent repayment plans. One can show in a simple directed search model that because student debt is non-dischargeable, students with debt are less picky about job offers, which delivers both shorter unemployment durations along with lower incomes. But the non-disposability of debt is key: if debt were dischargeable, the opposite pattern whereby students "gamble" on finding a high paying job obtains (Kaas and Zink 2011; Ji 2017). Finally, there is the question of the broader macroeconomic environment that students are graduating into. The cohort in 2008 was graduating into the worst recession since the Great Depression while those from 1993 were entering one of the strongest labor markets in US history. This difference could lead students in 2008 with high debt levels to be particularly quick to accept any job offer. We leave addressing these possibilities for future work.

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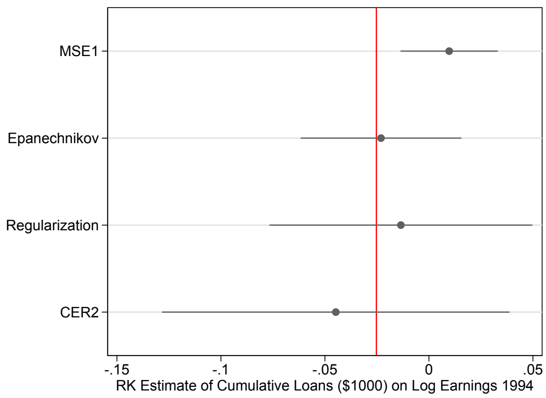
A Robustness Check: Varying the RK Specification Details

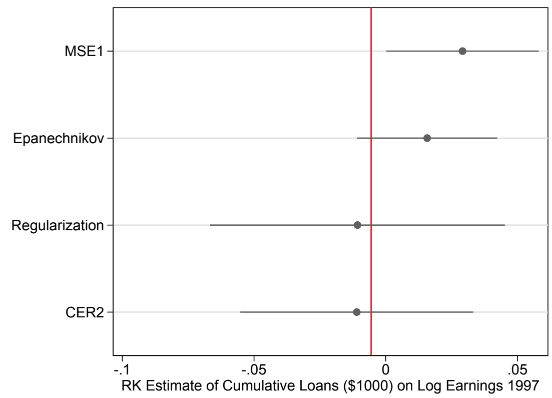
As noted in the main text, many choices must be made in specifying the RK estimator with regards to the order of the polynomials, the bandwidth, and the kernel. Here we examine the sensitivity of our results to these choices. Figure 9 shows the results of 4 different specifications for earnings in the 3 years for which we observe income. In specification "MSE1," we choose a single bandwidth for both sides of the cutoff to minimize the MSE rather than allowing the bandwidth to be different on either side. The specification "Epanechnikov" uses a kernel of that name rather than a triangular one. The specification "Regularization" makes a regularization adjustment to the bandwidth selection suggested by Imbens and Kalyanaraman (2012). Finally, the specification "CER2" chooses bandwidths along both sides of the threshold using the criterion proposed by Calonico et al. (2014). Overall, the results here are similar with those reported in the paper. What seems to matter more than these specification details is whether or not we include controls. One thing to notice as reported in Card et al. (2016) is that both the regularization adjustment and the CER procedure for choosing the bandwidth lead to much less precise estimates.

Figure 9: Robustness check: Effect of Varying RK Specification

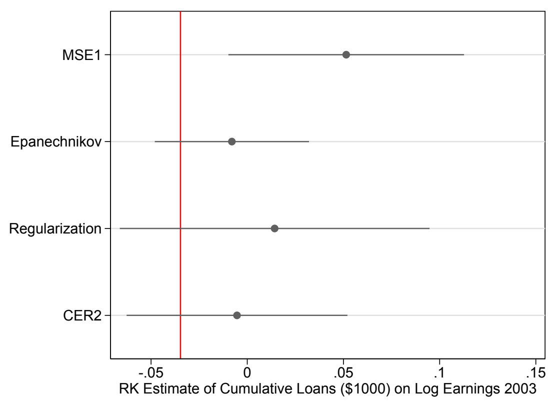
(a) 1994



(b) 1997



(c) 2003



Notes: The specification "MSE1" is a specification where we choose a single bandwidth for both sides of the cutoff to minimize the MSE. The specification "Epanechnikov" uses a kernel of that name rather than a triangular one. The specification "Regularization" makes a regularization adjustment to the bandwidth selection. The specification "CER2" chooses bandwidths along both sides of the threshold along the lines of Calonico et al. (2014). All of these regressions include controls for age, gender, race, SAT score, merit aid, parental education, parental income, parental family size, school quality, and indicator for public (vs. private) school.

B Robustness Check: "Global" Estimation Strategy

A different way to estimate the causal effect is to use a "global" estimation strategy. Rather than using local polynomials, we simply include a polynomial in need so unlike the case in the main text, observations far from $need = 0$ will be used to estimate these polynomial terms. To be more specific, consider the following first stage equation (2) and reduced form equation (3):

$$Staf_i = f(need_i) + \beta_1 \mathbf{1}\left[need_i > 0\right] + \beta_2 need_i \times \mathbf{1}\left[need_i > 0\right] + \eta \ \mathbf{X}_i + \nu_i \tag{2}$$

$$y_i = g(need_i) + \gamma_1 \mathbf{1}\left[need_i > 0\right] + \gamma_2 need_i \times \mathbf{1}\left[need_i > 0\right] + \phi \ \mathbf{X}_i + \upsilon_i, \tag{3}$$

where i indicates an individual, and $f(\cdot)$ and $g(\cdot)$ are polynomial functions of *need*. In theory, the degree of the polynomials can be chosen to minimize some information criterion. In practice, we only use a quadratic. The term $\mathbf{1}\left[need_i > 0\right]$ is an indicator function taking on the value of 1 if *need* is positive and 0 otherwise. The variable $\mathbf{X}$ is a vector of predetermined demographic characteristics. β_2 measures the change in the slope of cumulative Title IV loans around the threshold. It is interpreted as the change in cumulative borrowing for every dollar increase in *need*, by an individual barely eligible for Title IV loans. Similarly, γ_2 measures the change in the slope of log earnings around the threshold. In this framework, $\hat{\tau}_{RK} = \frac{\hat{\gamma}_2}{\hat{\beta}_2}$. Note that with a continuous treatment variable as in our case—cumulative Title IV loan amount—the fuzzy estimator has intuitive interpretation as an IV-type estimator.

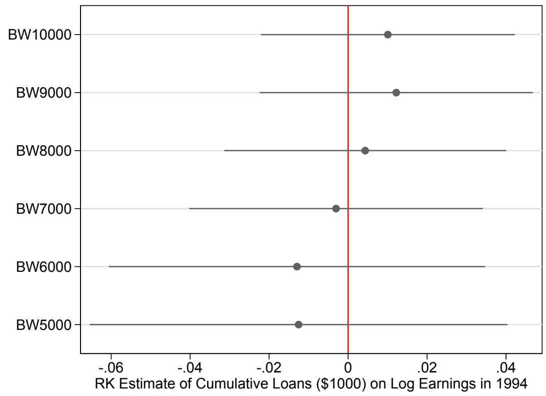
To make estimates based on this approach credible as estimates of a causal effect, it is necessary to restrict attention to the sample of individuals who in absolute value have *need* close to 0. The question then is how to choose such a bandwidth. Rather than make a single choice, we consider the sensitivity of the estimate to this choice. The trade off in choosing the bandwidth is clear. As we increase the bandwidth including individuals further and further away from the threshold, we gain additional power to identify the effect around the threshold with the drawback that we are comparing people that are perhaps very different in terms of unobservables. This makes interpreting the results as causal more challenging. Figure 10 plots the IV results for all the years along with standard errors for bandwidths ranging from \$5,000 to \$10,000.

For any year, notice that the intersection of all the confidence intervals is non-empty meaning that we could not reject the null that all of these estimates are the same.[26] First in 1994, the point estimate actually goes from negative to positive as we move from the smallest bandwidth to the largest one. A similar pattern emerges for 1997. For 2003, all the point estimates are positive and are quite stable in magnitude across the bandwidth choices. Notice as well across all of these bandwidths and years, we can never reject the null that the effect is equal to 0.

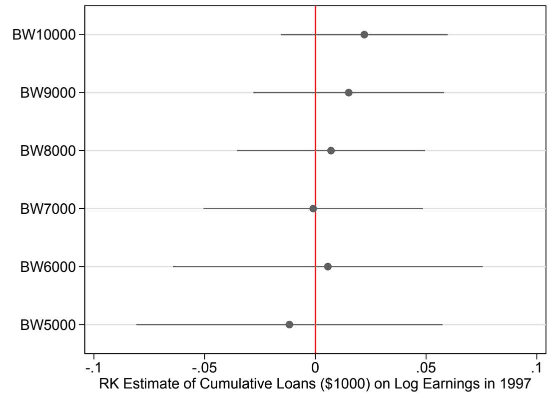
[26]This is, of course, a heuristic argument since the estimates are not really independent of one another.

Figure 10: Robustness check: Effect of Varying Bandwidth on IV Estimates

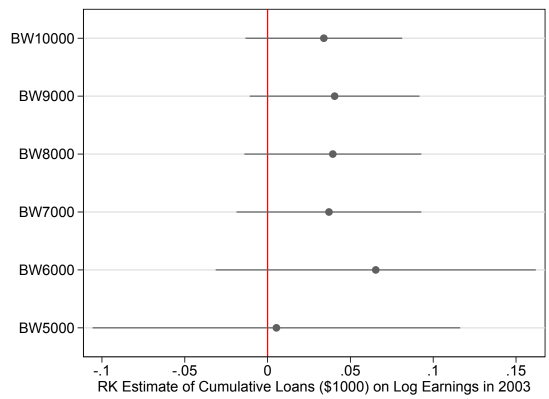
(a) 1994



(b) 1997



(c) 2003



Notes: Each point with whiskers representing standard errors is the estimated effect of cumulative Title IV loans on 1994 log income restricting to need equal to the value at that point. Effects are estimated using instrumental variables where the instrument is $need \times \mathbf{1}\left[need > 0\right]$ These regressions include controls for age, gender, race, SAT score, merit aid, parental education, parental income, parental family size, school quality, and indicator for public (vs. private) school. In addition, they include a quadratic in *need* and the indicator for $need > 0$.

C Robustness Check: OLS and Partially Linear Models

As an additional robustness check, we report the results from estimating a simple OLS model and a Robinson partially linear model where we flexibly control for the effects of parental income. This second model is specified as

$$y_i = \tau \times TitleIV_i + g(y_i^F) + \lambda \mathbf{X}_i + \delta_i, \tag{4}$$

where y_i^F is log of parental income. As is well known, while g is estimated at a non-parametric rate, our coefficient of interest τ, the effect of student loans on income, is still estimated at the standard parametric rate.[27] To make this comparison "fair" to the RK specifications, we restrict the sample to $|need| < 6000$.[28]

Before showing the results from these models, Table 11 shows the unconditional difference in earnings and its decomposition for eligible versus non-eligible students. There are unconditional differences between those that are eligible and those that are not eligible for financial need. The trend in the average annual earnings shows that those ineligible to borrow in the last year of schooling made more than those who were eligible to borrow in the final year, on average. In 1994, those eligible to borrow made about 5% less than those ineligible to borrow. In the subsequent years the gap between the two groups does not change much. In 1997, those ineligible have earnings slightly more than 5.8% than those eligible while the difference is 4% higher in 2003. In an accounting sense, much of this difference in earnings is due to difference in wage rates rather than number of hours worked. Note the disconnect between these unconditional differences and the RK estimates where hours appears to be the more important margin.

Table 12 shows the results from these two models for earnings in 1994, 1997, and 2003. We find similar results to the other specifications though smaller in magnitude. In addition, the standard errors are much smaller allowing us to rule out very large negative effects and almost rule out positive effects of debt on earnings. The fact that these estimates are smaller in magnitude than the RK ones suggests that the unobservables are pushing borrowing and future earnings in opposite directions. At the same time, loosely speaking we cannot reject the null hypothesis that the effects across all these specifications and the RK estimates are the same.

[27]We used the Stata command semipar to implement this. The default is to use a Gaussian kernel with a weighted local linear polynomial estimation of g.

[28]In results not reported here, we have experimented with different bandwidths like the IV specifications with no great differences.

Table 11: Earnings and Labor Market Outcomes by Eligibility

		Ineligible	Eligible
Earnings	1994	20.1	19.2
	1997	29.3	28.2
	2003	52.7	51.2
Weekly hours worked	1994	41.2	40.6
	1997	42.3	42.5
	2003	44.0	44.0
Hourly wage	1994	9.47	9.16
	1997	13.2	12.7
	2003	23.2	22.6
N		410	1140

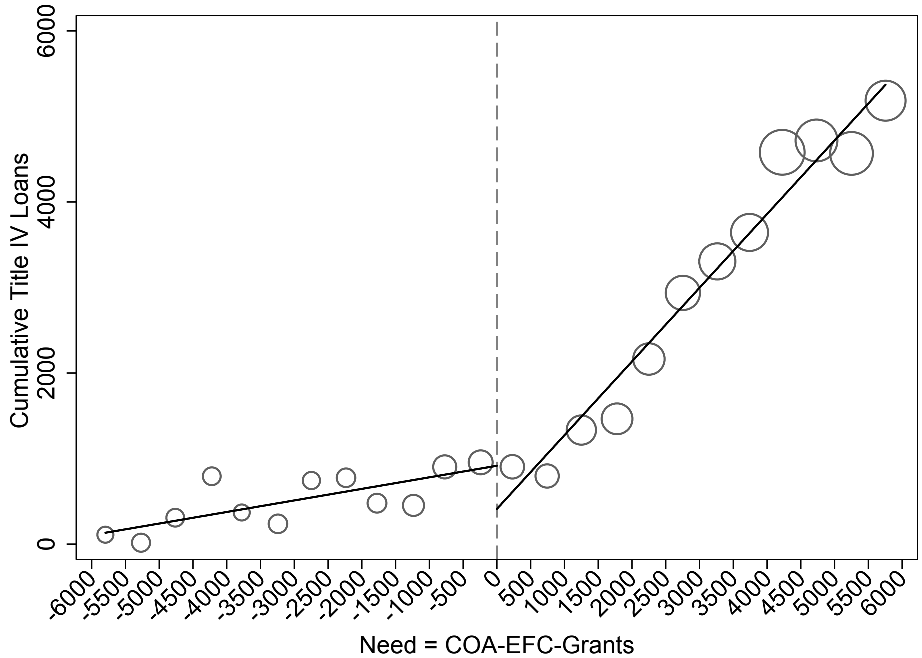
Notes: Ineligible is if $need < 0$. Students with $|need| > 6,000$ are excluded. Variables measured in dollars are all nominal and in units of $1,000.

Table 12: Effect of Cumulative Title IV Loans on Log Earnings: OLS and Partially Linear Models

	OLS		Robinson	
Panel A: 1994				
Cumulative Title IV Loans	-0.001	-0.003	-0.001	-0.003
	(0.004)	(0.003)	(0.004)	(0.003)
Panel B: 1997				
Cumulative Title IV Loans	0.000	-0.001	0.001	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)
Panel C: 2003				
Cumulative Title IV Loans	0.003	0.006	0.004	0.006
	(0.005)	(0.004)	(0.005)	(0.004)
Controls	Yes	No	Yes	No

Notes: Robust standard errors in parenthesis. Title IV loans are in units of $1,000. The controls include age, gender, race, SAT score, merit aid, parental education, parental income, parental family size, school quality, and indicator for public (vs. private) school. The variable *need* enters non-parametrically in the partially linear model and linearly for the OLS specifications. Only students with $|need| \leq 6,000$ are included.

Figure 11: Empirical Distribution of Cumulative Title IV Loans as a Function of *need*: Dependent and Independent Students



Notes: Bin size is \$500 with the center of each circle represents the average amount borrowed in the bin. The size of the circles reflect the number of observations in each bin. The best fit lines are estimated separately for $need < 0$ and $need \geq 0$ for our benchmark sample described in Section 3. We restrict attention to $|need| < 6,000$ to zoom in on the relationships close to $need = 0$.

D Robustness check: Including Independent Students

In our main specification, we dropped all independent students. Given that independent students were eligible for other types of unsubsidized federal loans, a concern with including this group might be that we do not observe the kink between *need* and total federal student loans. Figure 11 displays the empirical distribution of cumulative Title IV loans borrowed as a function of *need* in the final year of school when we include independent students in the sample. Qualitatively this figure is very similar to Figure 5: there still exists a kink at $need = 0$ even when we include independent students. Quantitatively, the figures differ in terms of average amounts borrowed on either side of the threshold. To the left of the threshold, the average amount borrowed is higher in Figure 11 than in Figure 5, but to the right of the threshold, it is does not go as high in Figure 11 as it does in Figure 5.

Table 13: Relationship between *need* and Predetermined Characteristics: Dependent and Independent Students

	Age	Male	White	Log Income of Parents 1991	Parental Education	Family Size	Predicted Earnings
RK_Estimate	0.101***	0.001	-0.021*	-0.024	-0.025	-0.006	0.000
	(0.033)	(0.013)	(0.011)	(0.021)	(0.018)	(0.011)	(0.002)
Lower BW	11.25	8.23	6.18	14.12	19.77	22.48	7.73
Upper BW	4.51	7.74	5.67	3.99	4.54	18.47	12.87
N	4060	4060	4060	4060	4060	4060	4060

Notes: Heteroskedasticity robust standard errors are in parenthesis. We use a local linear polynomial approximation and the MSE optimal bandwidth with a possibly different bandwidth on either side of the threshold and no regularization adjustment. The kernel is triangular.

With this enlarged sample, we recheck whether demographic characteristics still appear to flow smoothly at the boundary. Table 13 reports these results and, as before, we cannot reject the null of no kink for all observable characteristics besides age and race. Finally, Table 14 shows the RK results for earnings. Results are broadly consistent with the dependent only sample.

Table 14: Effect of Title IV Loans on Log Earnings: Dependent and Independent Students

	Log Earnings	
Panel A: 1994		
RK_Estimate	-0.026	-0.023
	(0.020)	(0.015)
Lower BW	33.73	25.22
Upper BW	4.33	4.77
Controls	Yes	No
N	4060	4060
Panel B: 1997		
RK_Estimate	-0.033	-0.007
	(0.023)	(0.013)
Lower BW	17.70	22.81
Upper BW	4.98	8.53
Controls	Yes	No
N	3260	3260
Panel C: 2003		
RK_Estimate	-0.001	0.069**
	(0.012)	(0.031)
Lower BW	34.96	11.66
Upper BW	11.00	19.07
Controls	Yes	No
N	2920	2920

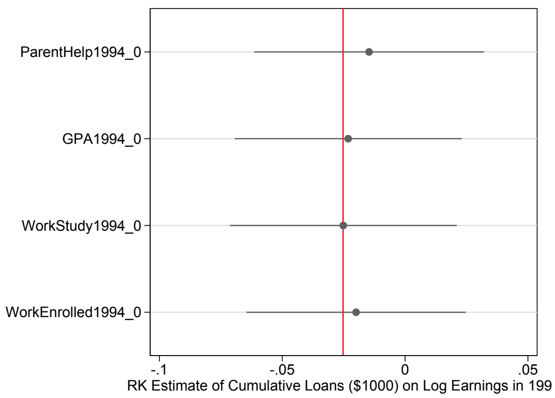
Notes: Heteroskedasticity robust standard errors are in parenthesis. We use a local linear polynomial approximation and the MSE optimal bandwidth with a possibly different bandwidth on either side of the threshold and no regularization adjustment. The kernel is triangular. Regressions also include a control for independent status. Title IV loans are in units of $1,000.

E Robustness Check: Alternative Sets of Controls in RK Estimates

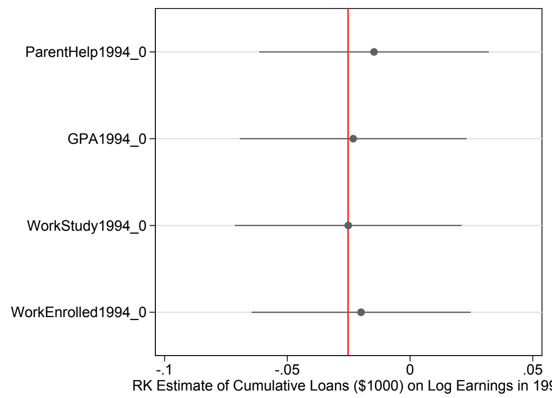
Here we explore the effects of varying the set of controls included in the RK estimation strategy. Figure 12 shows these results for the 3 years of earnings data where the red line in each picture is the point estimate from our main specification for that variable. The alternative specifications include the following variables in the set of controls: (1) "ParentHelp" which includes parental help; "GPA" which includes college GPA; "WorkStudy" which includes amount of work study; and "WorkEnrolled" which includes the amount of time working in the last year of enrollment. Across all these specifications, we cannot reject the null that the point estimates are the same as the main specification.

Figure 12: Robustness check: Effect of Varying Controls on RK Estimates

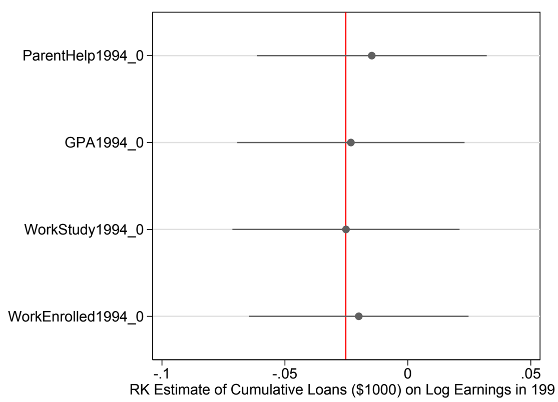
(a) 1994



(b) 1997



(c) 2003



Notes: All of these regressions include controls for age, gender, race, SAT score, merit aid, parental education, parental income, parental family size, school quality, and indicator for public (vs. private) school. The alternative specifications include the following variables in the set of controls: (1) "ParentHelp" which includes parental help; "GPA" which includes college GPA; "WorkStudy" which includes amount of work study; and "WorkEnrolled" which includes the amount of time working in the last year of enrollment.