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Constrained after college: Student loans and early-career occupational choices

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ABSTRACT

In the early 2000s, a highly selective university introduced a "no-loans" policy under which the loan component of financial aid awards was replaced with grants. We use this natural experiment to identify the causal effect of student debt on employment outcomes. In the standard life-cycle model, young people make optimal educational investment decisions if they are able to finance these investments by borrowing against future earnings; the presence of debt has only income effects on investment decisions. We find that debt causes graduates to choose substantially higher-salary jobs and reduces the probability that students choose low-paid "public interest" jobs. We also find some evidence that debt affects students' academic decisions during college. Our estimates suggest that recent college graduates are not life-cycle agents. Two potential explanations are that young workers are credit constrained or that they are averse to holding debt. We find suggestive evidence that debt reduces students' donations to the institution in the years after they graduate and increases the likelihood that a graduate will default on a pledge made during her senior year; we argue this result is more likely consistent with credit constraints than with debt aversion.
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1. Introduction

The returns to a college degree have risen substantially in recent years, but the cost of higher education has risen even more quickly. Between 1993 and 2005, the college wage premium rose by 27% (Mishel et al., 2007),[1] while real tuition and fees at public and private four-year colleges rose by 63% and 43%, respectively (*Trends in College Pricing*, 2005, Table A1).[2] These rising costs have made financial aid more important. The proportion of full-time, full-year undergraduates receiving financial aid rose from 58.7% in 1993 to 76.1% in 2004 (Snyder et al., 2006, Table 320).

As aid packages have grown, so has the importance of student loans. The proportion of students on aid who take out at least some loans rose from 55% in 1993 to 65% in 2004; over the same period, the proportion receiving grant aid fell slightly, from 83 to 82% (authors' calculations based on Snyder et al., 2006, Table 320). As a result, college graduates' debt burdens have risen. The average college graduate in 1993 had incurred $8462 in student debt. In 2004, this had risen to $13,275. Among those with positive debt, the average rose from $12,565 in 1993 to $20,386 in 2004.[3]

Some argue that the looming need to make loan payments leads students with debt to major in career-oriented fields or to choose more lucrative post-graduation jobs than would be otherwise optimal.[4] They also argue that educational debt deters individuals from purchasing homes or getting married, or assuming other responsibilities typically associated with full-fledged adulthood (Chiteji, 2007).

The traditional economic view of borrowing and saving rules out these sorts of effects. In a standard life-cycle model, student debt has only an income effect – proportional to the ratio of debt to the present discounted value of total lifetime earnings – on career and other post-college decisions. As debt is unlikely to represent more than one percent of a college graduate's lifetime earnings, we expect any such effects to be small.

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[1] http://www.epi.org/datazone/06/wagebyed_a.pdf.

[2] All figures in this paper are inflated to 2005 dollars using the CPI-U. The figures for college costs are enrollment-weighted.

[3] Student debt dwarfs another oft-cited source of indebtedness, credit card debt. The average senior in 2004 owed $512 in credit card debt (the median was $0; the mean and median among those with positive credit card debt were $2874 and $1654, respectively). These figures and those in the text are computed from the 1992–1993 and 2003–2004 *National Postsecondary Student Aid Surveys* (NPSAS; see Loft et al., 1995; Cominole et al., 2006).

[4] See, for example, Kamenetz (2006). A nationwide survey conducted by the Nellie Mae Corporation in 2002 found that 17% of student loan borrowers reported the loans had significantly impacted their career plans (Baum and O'Malley, 2003). Minicozzi (2005) analyzes data from the NPSAS and finds that graduates with more educational debt take jobs with higher initial wages and lower rates of wage growth than do those with less debt.

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One reason debt may have larger effects is that young people – particularly those from disadvantaged backgrounds – may be "debt averse" (see, e.g., Burdman, 2005; Callender and Jackson, 2005; Field, 2009). If holding debt reduces utility independent of any effects on consumption, recent graduates may attempt to repay loans quickly or otherwise act as if debt payments are more constraining than they really are.[5]

A second potential source of substantial debt effects on post-graduation decisions is a failure of capital markets. While much of the literature in education focuses on students' access to credit before and during college (see the discussion below), credit constraints after college graduation can also affect decisions. Young workers' current annual incomes are typically much lower than their permanent incomes, and many may prefer to borrow to finance current consumption. If recent graduates are unable to do this, student debt will have first-order effects on early-career consumption, and recent graduates may attempt to minimize these effects through their job choices.

There is suggestive evidence from non-educational contexts that many consumers are unable to borrow at reasonable interest rates. For example, Warner and Pleeter (2001) find that a majority of members of the Armed Forces selected a lump sum separation payment over an annuity even though the internal interest rate of the annuity option exceeded 20%. Gross and Souleles (2002) report that well over half of households with credit cards regularly roll over debt, with the median revolving account equal to about $7000 and a typical interest rate around 15%. They also find that credit card debt rises, immediately and significantly, when credit limits are increased, especially for those who were already close to their limits. Crossley and Low (2005; see also Japelli, 1990) find that one quarter of Canadian job losers report being unable to borrow at any rate. Finally, Souleles (1999) and Johnson et al. (2006) find that consumption increases after families receive income tax refunds, where the standard model predicts that predictable income shocks should have no effect on consumption.

In educational settings, the evidence on imperfections in capital markets is mixed and inconclusive. Cameron and Taber (2004) study the impact of borrowing constraints on educational decisions by exploiting the fact that the direct and opportunity costs of education have different effects on credit constrained individuals. They find little evidence that constraints limit otherwise optimal educational investment. Heckman and Lochner (2000; see also Carneiro and Heckman, 2002) also argue that borrowing constraints during the college-going years are not important determinants of college attendance, and that family income affects attendance primarily through its effect on students' academic preparedness. In contrast, Ellwood and Kane (2000) and Belley and Lochner (2007) argue that differences in college attendance by family income are partly explained by credit constraints. Stinebrickner and Stinebrickner (2008) conclude that some college students are credit constrained, though they argue that this does not account for family income differences in college persistence.

Our focus in this paper is on the effect of educational debt on students' early-career decisions. We take advantage of a unique natural experiment. In the early 2000s, a wealthy, highly selective university (hereafter referred to as Anonymous University, or "Anon U") phased in a "no-loans" policy, under which the loan component of financial aid awards was replaced with expanded no-strings-attached grants. Several other elite universities have subsequently followed suit. Our empirical strategy combines control function and instrumental variables strategies to identify the effect of debt on academic outcomes and career choices from changes across cohorts in the debt assigned to otherwise identical financial aid recipients (in particular, to students with the same financial need and family resources). We use data on students who did not receive financial aid to control for unobserved factors (such as the state of the macroeconomy) that might have led to different outcomes for students in the pre- and post-reform cohorts even in the absence of the policy change.

The Anon U reforms were not explicitly motivated by a desire to influence students' post-graduation plans. Rather, the intent was to eliminate financial concerns from the decision to apply to or attend the school and to increase the number of low-income students matriculating. To the extent that the policy change was successful in this regard, it may have led to changes in the composition of matriculants along both observed and unobserved dimensions. If pre- and post-reform aid recipients differ in their propensity to pursue low-income careers, this might bias our analyses. We take two strategies to minimize this. First, our main specifications control extensively for students' observed characteristics, particularly their families' financial resources. Second, we present estimates that exclude students from classes that matriculated at Anon U after the new policies were announced, identifying the effect of student debt solely from students who were already enrolled at Anon U at that time. This substantially reduces our statistical power, as students from these cohorts were only partially treated. To recover some of this lost power, we add back to the sample students from the first post-announcement class, who had already made application decisions when the policy was announced but had not yet made final commitments. We use estimates from Linsenmeier et al. (2006) of the effect of the no-loans policy on the matriculation rates of admitted students to assess the potential sample selection bias in this specification.

We find evidence, consistent across several specifications, that debt leads graduates to choose higher-salary jobs. Much or all of this effect is across occupations, as debt appears to reduce the probability that students choose low-paid "public interest" jobs. Debt effects are most notable on the propensity to work in the education industry. We also find suggestive (though imprecise) evidence that financial constraints affect students' academic choices during college.

Our impacts on career choices are too large to be entirely attributable to income effects. To help us distinguish between explanations based on credit constraints and on aversion to debt, we analyze data on alumni pledges and donations. One would expect debt aversion to be reasonably constant over the life cycle, and for college seniors to anticipate its effects on their future choices. By contrast, college students may not anticipate the degree to which credit constraints will bind in the period after graduation.[6] Although our estimates are imprecise, debt seems to have a larger effect on recent graduates' actual gifts to Anon U than it does on the pledges they make during their senior year. This appears to support the credit constraints hypothesis. Because the effect is small and because we can only speculate about the degree to which agents anticipate post-graduation constraints and debt aversion, this interpretation should be taken as suggestive rather than conclusive.

2. The Anon U policy reform

Anon U is one of the most selective, expensive colleges in the country, and it admits only the most academically qualified students. It prides itself on the diversity of its students, and it competes with other elite colleges to enroll the relatively few high school graduates of modest means with top academic credentials (Hill and Winston, 2006).

In 1998 and again in 2001, Anon U announced reforms of its financial aid program to reduce the role of student loans in aid

[5] We focus on students who are inframarginal to the college attendance decision, so aversion to taking on debt (as distinct from continuing to hold previously incurred debt) is unlikely to be an important factor.

[6] Students have access to a variety of government and third-party loans on relatively good terms. Access to this sort of credit dries up after graduation, and recent graduates are likely to have to rely on forms of borrowing – e.g., credit cards – that offer substantially worse terms than those they faced during college.

packages. Grants were expanded to keep total nominal aid awards – the sum of the face value of loans, grants, and campus work – approximately unchanged. As the present value of a grant is much higher than that of even a subsidized loan, these reforms represented a substantial increase in the value of Anon U's aid packages.

To fully understand the implications of this reform for students, it is useful to consider how Anon U determines a student's financial aid package. Except as noted below, the procedure did not change with the policy reform. Students are admitted to Anon U without regard to their financial circumstances.[7] Along with her admissions application and in every academic year thereafter, a student may apply for financial aid. The aid application solicits detailed information about the income and assets of the student and her parents. The Anon U financial aid office uses this information to develop an assessment of the family's ability to pay. The primary determinants of the "expected family contribution" (EFC) are parental income and assets, though student savings and summertime (but not term-time) earnings also enter the calculation.

Whenever the EFC falls short of the total annual costs of attendance – tuition, room, board, and an estimate of additional living expenses – the student is judged to need aid. Anon U puts together a personalized aid package that closes the gap. An aid offer has two "self help" components: a campus job during the term and, before the reform, a student loan. Both of the self help components are capped — students are not expected to work more than about 10 hours per week during the term, and even before the no-loans policy they were not expected to incur more than about $4500 in debt per year of enrollment (or approximately $18,000 over four years). Any remaining demonstrated need after reaching the self help limits is met through unconditional grants. The formula thus awards grants only to students whose demonstrated need exceeds the sum of the loan and campus work caps.[8]

Anon U student loans are offered on terms similar to those of the federal Perkins Loan. These are subsidized in two ways: the interest rate is below the market rate, and no interest accrues while the student is in school.[9] Repayment begins a few months after the student leaves school, and is amortized on a ten year schedule.[10]

Students may take up their offered aid packages in whole or in part. Not surprisingly, take-up on the grant component – free money – is high. But students commonly substitute among the self help components, working more during the term and taking on less debt or vice versa. Even the total quantity of self help is elastic: some students take on additional debt in order to relieve the burden on their parents, while others reduce their need by saving more than is expected from summer earnings, consuming less during the year than the aid office budgets, or drawing on more assistance from their parents than was indicated by the aid formula.[11]

Under the new Anon U policy, the loan cap was reduced, eventually to zero. As neither the formulae for computing expected family contributions[12] and demonstrated need nor the cap on expected term-time earnings changed, this entailed more generous grants for nearly all students on aid. Students benefiting from the policy remained free to take out loans in order to reduce term-time work or parent contributions or to permit more consumption during college but were offered an aid package that did not require debt.

The no-loans policy was implemented in two stages. First, beginning in 1998, loans were eliminated for new matriculants from the class of 2002 – that is, students who entered in Fall 1998 and were expected to graduate in Spring 2002[13] – and beyond with low family incomes, defined as below about $40,000 in nominal dollars. Approximately 18% of freshman aid recipients qualified for this in 1998–1999. Students from moderate income families (between $40,000 and $57,500) received partial loan reduction. Students from pre-2002 classes remained under the old regime and continued to receive loans regardless of their family incomes, as did higher-income students (approximately 61% of aid recipients) from later cohorts.

In January 2001 – after the application deadline for the class of 2005 – Anon U announced that the no-loans policy would be extended to cover all students on aid. This applied to all students, regardless of cohort, beginning in Fall 2001. Thus, a non-low-income student from the class of 2002 was required to take out loans for her first three years on campus but was covered by the no-loans policy during her senior year and therefore was asked to take on 75% as much debt over her college career as was a similar student from the class of 2001.

Fig. 1 illustrates these changes in policy by showing the total loans (over four years of attendance) offered to students in each cohort at three different family income levels. One can see that low-income students were required to take out fewer loans than higher-income students even before the no-loans policy was introduced. Low-income students in the classes of 2002 and beyond, however, were covered by the first phase of the no-loans policy and were assigned zero loans. Middle-income students were partially covered by the first phase and fully covered by the second phase. Thus, those in the 2002–2004 cohorts saw dramatic loan reductions, while those from the 2005 and 2006 cohorts were given zero loans. Finally, higher-income students with need were unaffected by the first phase of the policy but covered under the second phase. Those in the 2005–2006 cohorts were given zero loans, while those in the 2002–2004 cohorts were treated in proportion to the fraction of their college careers that came after the no-loans policy was fully implemented in 2001–2002.

Fig. 2 offers another view of the program, graphing the average total debt assigned by the Anon U formula (in the left panel) and the average actually taken (right panel), cumulated over four years, against the family contribution in the freshman year. As we discuss below, family contributions are consistently measured only at values below $37,443, so the figure shows local linear estimates fit to observations below that point; dots show means in the remaining data. The first two series, describing the 1999 and 2001 cohorts, illustrate the pre-reform policy. The third series indicates that debt was substantially reduced, both in theory and in practice, for the partially-treated 2003 cohort. The final series shows that the 2005 cohort was not required to take any debt at all, but that low-income students typically took out as much as $5000 in loans anyway. Finally, note that in all four cohorts students with family contributions above $37,443 were assigned approximately zero debt and took out less than $2500, on average.

[7] Anon U, like many other highly endowed universities, promises "need blind" admissions. Anon U and its competitors may in fact give small *preferences* to needy applicants, as a mechanism for maintaining economic diversity. Bowen et al. (2005) find that any such preferences are quite small.

[8] Students who are eligible for federal Pell Grants or who win external scholarships can use these to displace self help. Moreover, conversations with the Anon U financial aid staff indicate that most students with positive demonstrated need are given at least a token institutional grant. Anon U is unusual in this regard — less wealthy universities will typically give grants only to students with larger need, and are not always able to provide enough aid to meet the full demonstrated need.

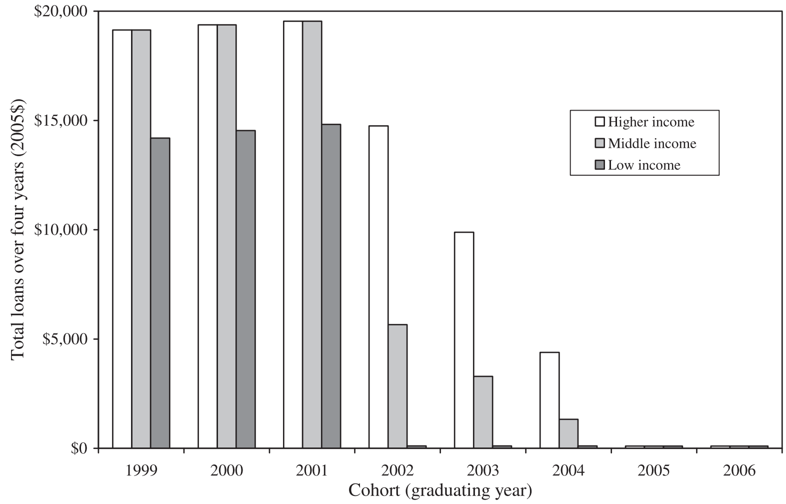
[9] Estimates of the present value of the subsidy range from about $0.25 (Gladieux and Hauptman, 1995) to $0.60 (Feldstein, 1995) per dollar of loans.

[10] Payment can be deferred in some situations, such as when students enroll in graduate school. When Anon U students qualify for federal Stafford Loans (available only to low-income families), they may take those instead. Stafford Loans have somewhat different terms, and borrowers may choose repayment periods of longer than ten years. Perkins and Anon U loans do not offer this option.

[11] Parents may themselves take on debt, through federal "PLUS" loans or unsubsidized loans from Anon U or third-party lenders, but the terms on these are usually worse than those on student debt.

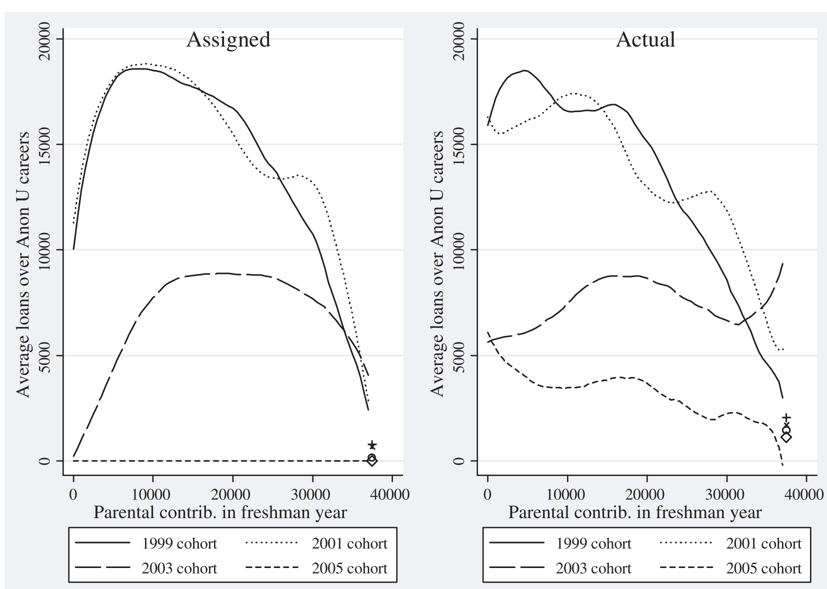
[12] Many colleges contract with the College Board to compute expected family contributions, but Anon U does not. Although the College Board formula changed several times during the period that we study, Anon U's formula was unchanged but for inflation adjustments.

[13] Throughout this paper, we refer to student cohorts by the intended year of graduation, the fourth year after the year of matriculation. Of course, some students graduate sooner or later than that.



Notes: Figure shows the loans indicated by the Anon. U. formula for students from three illustrative families who attend for four consecutive years, by cohort. The "higher income" family has nominal income $120,000 in each year but qualifies for $10,000 in aid. The "middle income" family has nominal income $48,000 and calculated parental contribution $4,000 in each year. The "low income" family has nominal income $38,000 and calculated parental contribution $1,000 in each year.

Fig. 1. Expected student loans, by cohort and family income.



Note: Parental contribution is measured in 2005 dollars, and is censored at $37,473; this value is imputed for students who did not apply for aid. Markers show averages for censored / imputed observations. Lines show local linear fits estimated only from uncensored observations.

Fig. 2. Assigned and actual loans over four years, by freshman year parental contribution and cohort.

Fig. 3 shows the fraction of students in each cohort who applied for aid at any point in their careers, who were ever found to need financial aid, and who ever took out loans. While the fractions applying for and receiving aid remained relatively stable between the 1999 and 2006 cohorts, the share taking out loans decreased from 46% in 2002 to 23% in 2006. Fig. 4 provides a similar look at the dollar amounts, totaled over a student's time at Anon U and averaged over all students in the cohort with positive need (again, in any year). The typical amount of student loan debt at graduation among those with positive need fell 67%, from over $15,000 in 1999 to $4000 in 2006. The sharp decline began with the 2002 cohort, the first cohort eligible for either portion of the no-loans program. The reduction in loans was more than offset by increases in grant aid, with the difference reflecting increasing average total need.[14] Work awards actually fell slightly over the period. This decline, along with the continued presence of loans even after the no-loan policy was fully implemented, suggests that beneficiaries of this policy consumed some of their reduced self help requirements by cutting back on campus work.

An average college graduate in the early 2000s could expect to earn about $1.5 million (in present discounted value) over her career, and even a graduate who expected earnings each year at the 25th

[14] The increase in total need primarily reflects increasing costs of attendance; average family contributions were approximately stable.

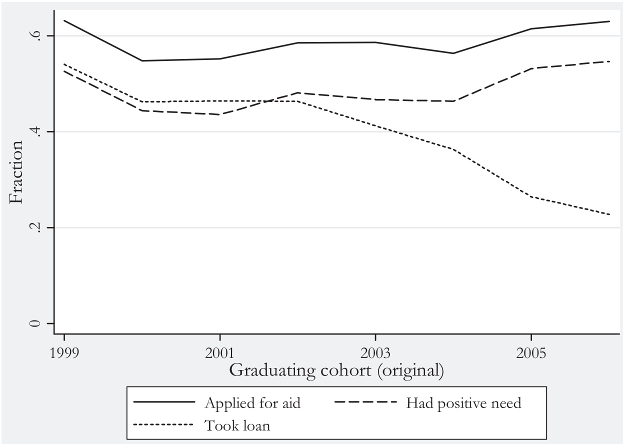


Fig. 3. Fraction of students ever applying for and receiving aid, by graduating cohort.

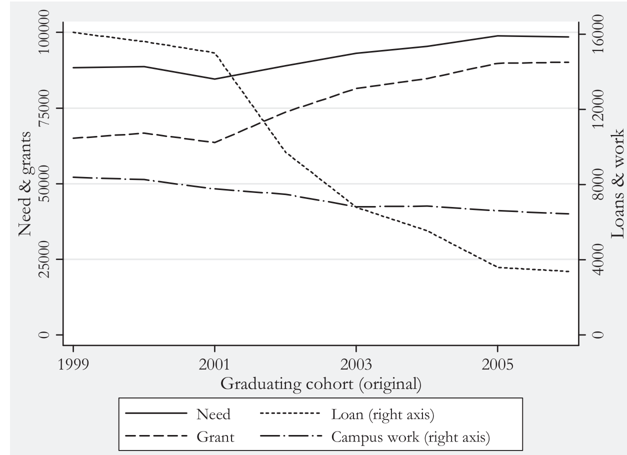


Fig. 4. Mean cumulative need, loans, and grants for students with positive need, by graduating cohort.

percentile of all college graduates her age would look forward to a lifetime income stream with present value of $500,000.[15] Graduates of Anon U earn much more than do average college graduates, so these would represent quite conservative expectations for the students in our sample.[16] As Fig. 2 indicates, Anon U's no-loans policy reduced the debt of the graduates from the lowest-income families by about a bit over $10,000, on average, with smaller effects on students from higher-income families. Thus, the policy raised affected students' expected lifetime incomes by less than two percent. In the standard life-cycle model, graduates with full access to credit markets will spend this additional wealth gradually over their entire lifetimes, with only a trivial impact on early-career consumption. Thus, there would be little reason for the program to importantly influence employment decisions.

Departures from the standard model could produce larger impacts of the no-loans policy, however, if they make it impossible or undesirable to fully smooth consumption over the life cycle.[17] Suppose, for example, that recent graduates are unable to borrow against future income to finance current consumption. Student loans are typically paid back over a relatively short period, commonly via equal monthly payments over the first ten years after graduation, when earnings are generally well below the lifetime average. A back-of-the-envelope calculation suggests that servicing $10,000 in student debt could consume five percent or more of recent graduates' earnings. Consumption changes of this magnitude might be large enough to influence career choices, motivating students to choose careers offering high initial salaries at the expense of reduced later earnings or reduced non-monetary amenities (such as hours required, flexibility of work hours, or the pleasantness of the work) in order to maintain desired consumption in the immediate post-college years.

Credit constraints are not the only possible source of this kind of impact. Debt aversion – distaste for holding debt independent of any impact on current or future consumption – could also generate a large effect of student debt on early-career consumption. Thus, the evidence presented below that the no-loans policy influenced career choices indicates that the life-cycle model with perfect credit markets does not well characterize recent Anon U graduates, but it does not distinguish between explanations based on credit constraints and those based on debt aversion. We attempt to distinguish these alternatives by exploiting the intuition that recent graduates may be surprised to face credit constraints but are unlikely to be surprised to find themselves averse to (continuing to) hold debt.

3. Estimation strategy

Our goal is to identify the effect of student debt on various outcomes, both during college and after graduation. We are interested in whether debt causes new graduates to make different choices than they would have had they reached the same decision point without debt. The Anon U reform, then, provides just the right counterfactual: students with financial need in later cohorts were given enough grant aid to meet that need without resorting to student loans. A comparison with otherwise identical students from earlier cohorts can identify the loan effect of interest, provided that pure time effects are adequately addressed.

The most straightforward way to implement this comparison is as a difference-in-differences (DID) analysis, comparing the between-cohort change in mean outcomes among financial aid recipients with the change among students not receiving aid. Aid recipients were "treated" by the no-loans policy if they were in later cohorts but not if they were in earlier cohorts. Students who did not need financial aid should not have been affected by the policy regardless of their cohort, and can therefore be used to control for business cycle and other time effects. The effect of debt on an outcome variable y can be computed as a Wald estimator, dividing the difference-in-differences in y by that in debt.

The DID strategy has several important shortcomings. First, there may have been changes in the relative characteristics of aid recipients over time, not least because the rising cost of attending Anon U shifted families who would not previously have needed aid into the aid-recipient category. Failure to control for this will result in a biased estimate of the aid effect. Second, the DID estimator does not exploit variation in the intensity of treatment. Students with very little financial need would have taken few loans in any case, so were not much affected by the no-loans policy, while those with greater need got larger benefits. Finally, the DID strategy cannot accommodate the partially-treated 2002–2004 cohorts.

In the rest of this section, we develop a regression-based version of the DID/Wald estimator that allows us to control for changing student characteristics and to exploit variation in treatment that cannot be captured in a simple DID/Wald framework. This leads us to our

[15] These calculations are based on the cross-sectional age-earnings profile of college graduates aged 25–64 in the 2003 and 2004 March Current Population Survey samples, assuming a real interest rate of 3.5% (Moore et al., 2004) and productivity growth of 1.5%. See Rothstein and Rouse (2007).

[16] Uncertainty about later-career earnings would reduce the certainty equivalent present value. However, Anon U graduates can confidently expect to earn above the 25th percentile college graduate wage in every year of their careers – the average wage among recent college graduates corresponds to roughly the 9th percentile in our Anon U sample – implying that $500,000 should represent a lower bound on this certainty equivalent.

[17] A formal model supporting the intuition outlined in this paragraph, along with the details of the calculations, is presented in Rothstein and Rouse (2007).

primary specification, which combines instrumental variables – using simulated loan offers as instruments for the actual debt level to exploit only policy-induced variation in debt – with a "control function" specification that uses a flexible parameterization of data on family financial circumstances to absorb potentially confounding variation in family background.

The DID-based Wald estimator can be seen as an IV estimate of the following equation:

$$y_{ic} = \alpha + post_c\delta + need_i\gamma + d_{ic}\beta + e_{ic}, \tag{1}$$

where y_{ic} is the outcome for student i from cohort c, $post_c$ is an indicator for whether the student comes from a treated cohort; $need_i$ is an indicator for whether the student has financial aid, and d_{ic} is the student's level of debt. The interaction of the two indicator variables, $post_c^* need_i$, serves as an instrument for d_{ic}.

To convert this to a richer specification, we need a more detailed measure of "treatment" than the simple $post_c^* need_i$ interaction. Let d_{ic}^{99} be the loan that the student would have been offered had her aid package been calculated according to the formula that applied to the pre-program 1999 cohort. As a result of the program change, she was instead offered d_{ic}^*. The intended treatment, then, is the difference between these, $d_{ic}^* - d_{ic}^{99}$. Under the Anon U aid formula, d_{ic}^{99} is a deterministic function of the student's expected family contribution, $d_{ic}^{99} = g^{99}(EFC_{ic})$. For any single cohort c, $d_{ic}^* = g^c(EFC_{ic})$ is another deterministic function, though the shape of this function varies substantially with c. For $c \geq 2005$, $g^c(EFC) = d_{ic}^* \equiv 0$, while for earlier cohorts $g^c(EFC)$ more closely resembles $g^{99}(EFC)$ (particularly for non-low-income families).

The continuous-treatment analogue to the $need_i$ control in Eq. (1) is a flexible control for the effect of the expected family contribution on outcomes, $f(EFC_{ic})$. We also generalize $post_c$ to a series of cohort dummies. Our primary estimating equation is thus:

$$y_{ic} = \delta_c + f(EFC_{ic}) + d_{ic}\beta + e_{ic}, \tag{2}$$

with first stage equation

$$d_{ic} = \theta_c + h(EFC_{ic}) + \left(d_{ic}^{99} - d_{ic}^*\right)\pi + v_{ic}. \tag{3}$$

Note that if the $f()$ and $h()$ functions are sufficiently flexibly parameterized, they will absorb all of the variation in d_{ic}^{99}. Thus, Eq. (3) can be equivalently written to use the student's own offered loan, d_{ic}^*, as the instrument, and indeed we do this in our empirical implementation. Within any single cohort, d_{ic}^* would be absorbed by a sufficiently flexible $h()$ function, but with data pooling multiple cohorts a single $h()$ function cannot absorb the variation in d_{ic}^* among students in different cohorts with the same EFC_{ic}. Thus, it is only the cross-cohort variation in the $g^c()$ function – deriving from the Anon U reform – that identifies the debt effect in Eq. (2). The central identifying assumption of our strategy is that the direct effect of family characteristics (or at least that operating through the EFC_{ic} variable) on outcomes is constant over time.

Granting this assumption – to which we return later – our IV strategy should eliminate two possible sources of bias that would arise in simple OLS estimates of the effect of debt on outcomes. The first and most important is omitted variables. Most of the variation in student debt – and all of the variation in offered debt within a single cohort – derives from differences in families' financial resources. Family background is likely to have strong effects on academic and employment outcomes. If it is excluded from the estimating equation, both d_{ic} and d_{ic}^* will be correlated with the error term. The inclusion of a flexible control function in the expected family contribution in our IV specification should eliminate the resulting bias since, as noted above, the offered loan is a deterministic function of the EFC. The possibility that EFC_{ic} does not capture all dimensions of family background does not present a problem: so long as the projection of other family background characteristics onto the expected family contribution is constant over time, these characteristics will be uncorrelated with d_{ic}^* conditional on EFC_{ic}.

A second possible source of bias is reverse causality. Empirically, there is a fair amount of variation in actual loans that is unexplained by the loan offer. This variation may reflect differences in tastes or in expectations about future earnings. In particular, those who expect high future earnings (i.e., have a high e_{ic} when y_{ic} is earnings) should consume more in college, and may take out more debt to finance this. This will bias an OLS estimate of β from Eq. (2) upward. This bias is eliminated in the instrumental variables estimate.

We construct d_{ic}^* by applying the Anon U aid formula for the student's cohort to the observed expected family contribution variable.[18] By the argument above, this is uncorrelated with the residual component of family background, so long as $f()$ and $h()$ are sufficiently flexible. We model each as a cubic polynomial. To guard against the possibility that this fails to fully capture the variation used in the loan assignment, we also control for the total financial need and, in some specifications, for the loan that would have been offered had the student been in the 1999 or the 2002 cohort (i.e., d_{ic}^{99} and d_{ic}^{02}). With these controls, the debt effect is identified solely from across-cohort variation among students who would have been offered the same aid package had they been in the same cohort.[19]

Our identifying assumption would be violated if there were differential underlying trends in the employment outcomes of students receiving and not receiving financial aid. One possible source of such differential trends might be changes in the composition of the Anon U aid population. In some specifications, we include controls for several non-aid student characteristics: indicators for whether the student was a legacy (i.e., had parents who attended Anon U), the first in the family to attend college, or a recruited athlete; a cubic in family income; and a full set of indicators for the academic and non-academic ratings given to the student's admissions application. These additional controls have essentially no effect on our estimates. We also report specifications that allow for changes in the returns to student characteristics over time by including unrestricted interactions of students' SAT scores – indicators of ability that are correlated with financial need – with cohort indicators. This, too, has no effect on the results. Finally, we estimate our model on the subsample of students whose application and matriculation decisions were made before the no-loans policy was announced. Yet again, there is no sign that endogenous matriculation decisions can account for our results.

4. Data

Our data come from Anon U's administrative records, and describe students from the cohorts that entered between Fall 1995 and Fall 2002. 91% of Anon U matriculants graduate within four years and 96% graduate within five years, and neither completion rates nor time-to-degree appear to have changed as a result of the no-loan program. Thus, the majority of these students graduated between 1999 and 2006. We merge data from several independent databases, using identifiers that are common to all of Anon U's student records. The registrar's data include 9287 students from the 1999–2006 classes. We have complete data on admissions qualifications, financial aid, and employment outcomes for 8641 students.[20]

[18] In practice, there is some evidence that the financial aid office occasionally deviates from its formula in response to student requests, particularly when Anon U is in competition with other colleges for a particular student. By using the loan offer indicated by the formula rather than the actual offer we avoid any endogeneity of the negotiated aid package.

[19] Nielsen et al. (2010) use a similar strategy.

[20] 70% of the missing observations are students who have not graduated, split approximately evenly between those who have dropped out and those (overwhelmingly from the classes entering in 2001 and 2002) who were still active as of summer 2006.

4.1. Financial aid data

Our key explanatory variables come from financial aid records. We observe the expected family contribution and the size and composition of the aid award. We convert all dollar figures to 2005 dollars. The aid data are in student-year format, while most of our analysis focuses on student-level records. We sum the student loans taken over all years that the student appears in the aid data, and average the expected family contribution over the student's (first) four years of enrollment.

Not all students apply for aid every year. This complicates our analysis, as the expected family contribution is computed only for aid applicants. We assume that any student who did not apply for aid would not have been found to have need in any case. This implies that her expected family contribution is at least as high as the cost of attendance, and we impute this value.[21]

Our data cleaning decisions were made with an eye toward maximizing the comparability of the data across cohorts. This leads us to censor variables from some cohorts to match the censoring that occurred in other cohorts. For example, a family in the 1999 cohort whose contribution exceeded the cost of attendance might not have applied for aid. Because costs rose over time, a family with the same income in the 2005 cohort might have applied for and received aid. We censor this family's contribution at the 1999 cohort's real cost of attendance, to preserve the symmetry with our treatment of the non-applying family from the earlier cohort.[22] Our loan simulation is based on this censored contribution, although results are robust to alternative censoring – e.g., using the actual costs as the censoring point – and to the use of uncensored data.

Table 1 presents estimates that relate the composition of the actual aid package, cumulated over all years in which the student was enrolled, with the simulated cumulative loan offer over the four years after the student's matriculation. Each specification includes controls for cohort (a full set of dummy variables), a cubic in the parental contribution, indicators for the number of years that the contribution is censored and the number of years that the student applied for aid, and the student's cumulative financial need. With such rich controls, the simulated loan coefficient is identified exclusively from the differential effects of across-cohort variation in the loan formula on students with different need levels. If our simulation perfectly captured offered loans and if all students perfectly complied with the "intended" treatment, the simulated loan effect on actual loans would be exactly one, that on actual grants would be +1, and the effects on other variables would be zero. In reality, the estimated effects on loans and grants are likely attenuated by imperfections in our simulation and by imperfect compliance.

Column 1 presents an analysis in which the dependent variable is the total debt incurred over a student's time at Anon U. While the simulated loan offer coefficient is significantly different from one, it is nevertheless large – the realized cumulative loan rises by about $0.77 for each additional dollar of offered loans – and quite precisely measured. Columns 2 and 3 take as dependent variables the other components of the aid award. The estimated coefficient for grant aid (Column 2) is −0.90. Again, this is significantly different from the theoretical effect of −1, but in practical terms the deviation is small.[23]

Table 1
Simulated loan and the composition of the realized aid package.

	Student loan	Grant	Campus work	Total aid
	(1)	(2)	(3)	(4)
Simulated loan	0.77	−0.90	0.08	−0.06
	(0.01)	(0.02)	(0.01)	(0.01)
R^2	0.65	0.98	0.68	0.99

Notes: N = 8893. Standard errors are in parentheses. Dependent and independent variables are cumulative over four years, and are measured in 2005 dollars. Each specification includes a full set of cohort dummies, a cubic in the cumulative (censored) parental contribution, a set of dummies for the number of years in which the parental contribution was censored, a set of dummies for the number of years that the student applied for aid, and the student's cumulative financial need.

Column 3 presents an analysis of term-time student work. For each dollar of loans offered, term-time earnings increase by 8 cents.[24] Column 4 indicates that the simulated loan is weakly (but significantly) negatively associated with total aid, the sum of grants, work, and loans. As we control for need, and as Anon U always offers an aid package that meets total need, the negative point estimate indicates that take-up of aid packages is lower when these packages include more debt, perhaps because some students substitute additional parental contributions for offered loans.

4.2. Other data

We create a variety of control variables and outcome measures from other administrative data sets.[25] We draw from admissions data the student's SAT score and legacy status, the type of high school attended (public or private), and the numerical ratings given to the application. The registrar provided information about the major, minors, grade point average (GPA), and honors received. Our richest outcome measures come from an "exit survey" conducted by the Office of Career Services during the week before graduation. The survey asks about plans during the next year, and students who have already obtained jobs are asked their occupations, industries, and annual salaries. Response rates are typically above 97%. Finally, the Development Office provided information on students' donations to the university's "annual giving" campaigns in the first several years after graduation. Anon U's solicitations start early, and students are asked during their senior year to make pledges covering each of the next five years. We observe both pledges and actual gifts, though our ability to look several years beyond graduation is limited for the most recent classes.[26]

Table 2 presents comparisons of the observable characteristics of students from the 1999–2001 cohorts, who were ineligible for the no-loans program, and those from the 2005–2006 cohorts, who entered after full implementation. For now, we exclude the partially-affected 2002–2004 cohorts. We distinguish between students with positive

[21] Some students who apply for aid have EFCs that are above the cost of attendance. We censor these at the lower level, and include dummy variables in our regressions for the number of years that the EFC was imputed or censored. Another complication arises because estimated costs vary with, for example, the distance between the student's home and Anon U. We use the modal cost in each year for all students.

[22] That is, for each student we assign $d^*_{ic} = g_c[\min(EFC_{ic}, cost99)]$ where *cost99* was the cost of attending Anon U for the class of 1999. $37,473, the threshold used in Fig. 2, is the freshman year portion of *cost99*.

[23] The most likely explanation is that we do not capture adjustments made to the formula award on a case-by-case basis by financial staff.

[24] We do not observe actual earnings, but only the assumed earnings that were included in the final financial aid offer. Upon receipt of the original offer, which might have specified $0 in loans and $1500 in term-time earnings, students were permitted to negotiate alternative divisions of the "self help" component between loans and work. Our point estimate indicates that a student receiving a $0/$1500 initial award negotiated a final award that specified $120 in debt, on average, and $1380 in work.

[25] In a few cases, students are missing from one of the component data sets. This produces missing values for some of our variables. There are also missing values for some items in each data set. Most sample size variations in the analyses below, however, reflect known sources of omission – e.g., outcomes that had not yet occurred when the data were extracted or job characteristics for graduates who had not taken jobs. We discuss the implications of this sample selection below.

[26] Our GPA and alumni gift data were extracted in Fall 2006, so we do not observe gifts or GPAs for the class of 2006. We observe only one year of gifts for the class of 2005, two years for the class of 2004, and so on.

Table 2
Average characteristics of students, Anon U 1999–2001 and 2005–2006 cohorts.

	1999–2001 (pre-reform)		2005–2006 (fully treated)		Change		Difference in differences
	Without need	With need	Without need	With need	Without need	With need	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
N	1829	1634	1085	1265			
SAT	1441	1407	1468	1425	**27**	**18**	−9
	[98]	[117]	[90]	[112]	(4)	(4)	(6)
Race							
Black	0.02	0.13	0.02	0.15	−0.003	0.017	0.020
					(0.005)	(0.013)	(0.014)
Hispanic	0.03	0.09	0.03	0.09	−0.001	0.005	0.006
					(0.007)	(0.011)	(0.013)
Asian	0.13	0.15	0.14	0.15	0.006	0.001	−0.005
					(0.013)	(0.014)	(0.019)
Private HS	0.52	0.34	0.55	0.34	0.031	0.008	−0.023
					(0.019)	(0.018)	(0.027)
Financial need							
Log (family income)		11.19		11.27		**0.08**	
		[0.68]		[0.83]		(0.03)	
Family contribution (per year)		15,333		16,201		**868**	
		[10,652]		[12,023]		(429)	
Total need		86,642		99,015		**12,373**	
		[45,640]		[51,243]		(1830)	
Total loans	1145	15,485	496	3448	**−649**	**−12,038**	**−11,389**
	[4180]	[8311]	[2785]	[6169]	(129)	(269)	(298)

Notes: Need/no-need categorization is based whether the student ever had positive need during her time at Anon U. Family income is measured in the freshman year, only for students who had positive need in that year. Family contribution is averaged over a student's time at Anon U, censored in each year at the typical cost of attendance in that year and imputed at the censoring point in years that the student did not apply for aid. Standard deviations (for non-binary variables) are shown in square brackets; standard errors are in parentheses. Changes and differences-in-differences in bold are significant at the 10% level.

financial need, who would have been affected by the no-loans policy, and students without need, who would not. Columns (1) and (2) show the average characteristics of “no-need” and “need” students in the earlier cohorts, while columns (3) and (4) show the same groups in the later cohorts. Columns (5) and (6) show the change over time for each group, and column (7) shows the difference between these.

Students with need have lower average SAT scores than those without need, are less likely to be white, and are much less likely to have attended private high schools (although even so, a third of students with need attended such schools). There were few changes between the two cohorts in racial composition or the type of school attended. Elite college admissions became much more competitive over this period, however, and SAT scores rose dramatically in both the need and no-need subgroups.

The remaining rows of the table show financial characteristics, which we observe only for students who apply for financial aid and which are therefore shown only for students with need. Real family incomes of students with need rose about 8% between cohorts, and mean expected family contributions rose by a bit less than \$900. Average need rose by over \$12,000. This primarily reflects the increasing cost of attending Anon U — tuition, fees, and room and board collectively rose by \$12,122 over this period.[27] Despite the increase in need, average student loans fell dramatically, from \$15,485 in the 1999–2001 cohorts to less than \$3500 in the 2005–2006 cohorts. Some students without demonstrated need took loans as well, but the average amounts are quite small.[28] The difference-in-differences estimate of the effect of the no-loans program on the total debt that students incur is −\$11,389.

5. Results

5.1. Difference-in-differences analyses

Table 3 presents DID analyses of our primary outcome measures, grouped into “academic,” “career,” and “alumni gifts” categories. Aid recipients have lower GPAs and rates of honors receipt than non-aid recipients. GPAs rose for both groups of students in the later cohorts, with a slightly larger increase for aid recipients. There were no meaningful changes in honors receipt.

The intuition outlined above indicates that in the presence of debt aversion or credit constraints debt will lead students to substitute toward higher-salary jobs with lower levels of job amenities, particularly in the years immediately after graduation when constraints are likely to be most binding. We find little relative change in aid recipients' propensities to plan employment or graduate school during the year after graduation, nor in the probability that they will have a job lined up as of graduation. When we look at the subset of students who have jobs lined up, however, there do appear to be shifts in the *types* of jobs that they take as measured by the industry. We consider two groups of high-salary and two groups of low-salary industries. Our first group of high-salary industries includes consulting,[29] banking, and finance jobs, which collectively account for over half of Anon U students with jobs on graduation. Our second group adds to these a group of industries – like pharmaceuticals and computer products and services – that also offer high mean salaries. On the low-salary side, we first consider the nonprofit, government, and education sectors (together 19% of our sample), then expand this to include other low-salary industries like publishing and architecture.

Aid recipients shifted out of industries with high average salaries and into lower-salary industries, while there was little change in the industry composition of jobs taken by students not on aid. While there was no relative decline in the share of aid recipients taking jobs

[27] The increase in costs may explain the increase in the family incomes of students with need (about \$6000), as families whose expected contributions fell between the old and new tuition levels would have been judged to have need in the later period but not in the earlier period.

[28] We have been told that some students who apply for aid but are found not to have need are nevertheless offered token aid packages composed primarily of “self help.” The decline in loans among no-need students may indicate that loans were removed from these packages when the no-loans policy was implemented.

[29] We also include in this category individuals who listed other industries (e.g., health care) but reported “consulting” as their occupation.

Table 3
Anon U academic and employment outcomes, by cohort and financial need.

	1999–2001		2005–2006		Change		Difference in differences
	Without need	With need	Without need	With need	Without need	With need	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Academic							
GPA (N=4419)	3.43	3.30	3.48	3.37	**0.050**	**0.064**	0.015
	[0.34]	[0.41]	[0.31]	[0.36]	(0.016)	(0.018)	(0.024)
Honors (N=5528)	0.49	0.40	0.49	0.40	0.000	0.002	0.002
					(0.020)	(0.019)	(0.027)
Career							
Post-graduation plans (N=5421)							
Plans graduate school	0.19	0.21	0.19	0.20	−0.004	−0.009	−0.005
					(0.016)	(0.016)	(0.022)
Plans employment	0.61	0.63	0.58	0.60	**−0.035**	−0.028	0.006
					(0.020)	(0.019)	(0.027)
Has a job	0.41	0.39	0.40	0.38	−0.007	−0.010	−0.003
					(0.020)	(0.019)	(0.027)
Industry/occupation (if has a job; N=2048)							
Consulting/banking/finance	0.58	0.51	0.61	0.53	0.034	0.016	−0.018
					(0.031)	(0.032)	(0.045)
Any high-salary industry	0.70	0.67	0.66	0.60	−0.039	**−0.071**	−0.032
					(0.030)	(0.031)	(0.043)
Nonprofit/govt./education	0.18	0.17	0.15	0.22	−0.029	**0.048**	**0.078**
					(0.023)	(0.026)	(0.035)
Any low-salary industry	0.25	0.23	0.24	0.32	−0.006	**0.085**	**0.091**
					(0.028)	(0.029)	(0.040)
Salary (if has a job; N=1689)							
Observed	50,086	51,025	52,568	50,849	**2482**	−175	**−2657**
	[15,111]	[16,243]	[17,014]	[13,242]	(1169)	(1036)	(1562)
Salary below $41,395 (25th percentile)	0.26	0.24	0.20	0.23	**−0.057**	−0.004	0.052
					(0.030)	(0.030)	(0.042)
Alumni gifts (N=4531)							
Pledge for 1st year after graduation	15.2	11.4	16.3	12.9	1.1	**1.6**	0.5
	[31.0]	[12.6]	[46.2]	[18.4]	(2.2)	(0.8)	(2.3)
Gift in 1st year after graduation	25.1	14.6	25.3	19.0	0.2	**4.4**	4.3
	[58.3]	[26.8]	[55.8]	[36.9]	(2.8)	(1.7)	(3.3)

Notes: Standard deviations (for non-binary variables) are shown in square brackets; standard errors in parentheses. Changes and differences-in-differences in bold are significant at the 10% level. GPAs and alumni gift data are missing for students in the 2006 cohort. Reported sample sizes refer to observations in the 1999–2001 and 2005–2006 cohorts.

in the consulting, investment banking, and finance sectors, there was a notable increase in the share taking jobs in the nonprofit, government, and education sectors.[30] Consistent with this shift, we see that while non-aid-recipients' mean salaries rose by nearly $2500, aid recipients' mean salaries were flat. Effects of debt are most likely at the lower end of the salary distribution, and indeed we see that the fraction of non-aid students with salaries below $41,395 (the 25th percentile salary in our data) fell substantially while there was no corresponding drop among students on aid.

The last rows of the table show mean alumni pledges and gifts for the year immediately following graduation, when alumni are perhaps most likely to be financially constrained. We see significant increases in both pledges and gifts from aid recipients. The change for non-aid-recipients – who both pledge and give more than recipients in all cohorts – is smaller and not significant.

All of these changes are consistent with the presence of debt effects on students' career choices and on the constraints that they face after graduation. These estimates may be confounded, however, by changes in the characteristics of students who receive aid. The IV and control function strategies outlined above are designed to eliminate such confounding variation.

5.2. OLS and IV estimates of effects on employment outcomes

Table 4 presents regression estimates of the effect of student debt (in $10,000s) on the employment outcomes from Table 3. Column 1 shows sample sizes; these are smaller for outcomes that are only available for students who have accepted jobs and who report annual salaries than for other outcomes. Columns 2–4 present OLS results, without any controls (column 2), with a cubic in the expected family contribution and a linear control for the total need (column 3), and with those controls plus a vector of other family background and academic quality controls (indicators for being a first-generation college student, a legacy at Anon U, or a recruited athlete, plus academic and non-academic admissions ratings and the simulated loan under the rules that applied to the 1999 and 2002 cohorts) in column 4.[31] The next three columns repeat these specifications but instrument for the observed loan with our simulated loan offer. The first stage coefficients in the three specifications – estimated on the full sample – are shown in the first row. As in Table 1, the coefficients suggest that the instrument is strongly related to the actual loan amount and that a dollar in simulated loans increases a student's actual debt by about 77 cents. Coefficients are similar for each of the subsamples used in the table.

In the simplest specifications without controls, we estimate that debt has no effect on the likelihood that an individual plans to attend graduate school and a small positive effect on the likelihood of planning employment.[32] When we add controls for family financial

[30] There is no clear pattern in the industries from which these students are drawn.

[31] Columns 2 and 3 also include controls for the number of years that the family contribution was censored or imputed. We present only linear probability models. Results are similar (though more difficult to interpret in IV specifications) when we instead use probit models.

[32] About 20% of Anon U students report other plans, including travel, internships, and fellowships. The predicted effect on whether the student plans to attend graduate school is ambiguous, as loan payments can be deferred while a student is in school. Still, undergraduate loans may deter debt-averse students from taking on further debt for graduate school.

Table 4
Estimates of effects of $10,000 in loans on employment outcomes.

	N	OLS			IV		
		Cohort dummies	+ controls for financial status	+ additional controls	Cohort dummies	+ controls for financial status	+ additional controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First stage	9287				**0.85**	**0.77**	**0.77**
					(0.01)	(0.01)	(0.01)
Post-graduation plans							
Plans graduate school	8672	−0.002	**−0.017**	−0.009	0.009	−0.006	−0.009
		(0.006)	(0.008)	(0.008)	(0.009)	(0.015)	(0.015)
Plans employment	8672	**0.012**	**0.019**	0.009	0.002	0.011	0.011
		(0.007)	(0.010)	(0.010)	(0.010)	(0.018)	(0.019)
Has a job	8672	**−0.025**	**−0.016**	−0.012	**−0.025**	0.006	0.011
		(0.007)	(0.009)	(0.009)	(0.010)	(0.018)	(0.018)
Industry/occupation (if has a job)							
Consulting/I-banking/finance	3020	**−0.036**	−0.003	0.008	**−0.063**	0.006	0.015
		(0.012)	(0.017)	(0.017)	(0.018)	(0.036)	(0.036)
Any high-salary industry	3020	−0.018	−0.010	0.008	−0.020	0.014	0.014
		(0.012)	(0.016)	(0.016)	(0.018)	(0.034)	(0.034)
Nonprofit/government/education	3020	−0.012	**−0.022**	**−0.030**	−0.004	**−0.052**	**−0.058**
		(0.010)	(0.013)	(0.013)	(0.015)	(0.028)	(0.028)
Any low-salary industry	3020	−0.001	−0.007	−0.020	−0.006	**−0.057**	**−0.058**
		(0.011)	(0.015)	(0.015)	(0.017)	(0.032)	(0.032)
Salary (if has a job)							
Observed	2441	−84	−472	30	978	**2263**	**2011**
		(399)	(558)	(563)	(618)	(1202)	(1199)
Salary below $41,395 (25th percentile)	2441	−0.006	−0.005	−0.017	−0.018	**−0.059**	**−0.062**
		(0.012)	(0.016)	(0.016)	(0.018)	(0.035)	(0.035)

Notes: Columns 2 and 5 include a full set of cohort dummies. Columns 3 and 6 add controls for the student's cumulative total need, a 3rd order polynomial in the avg. parental contribution (censored at the budget that applied to the 1999 cohort), and sets of indicators for the number of years the student applied for aid and the number of years the contribution was censored. Columns 4 and 7 add controls for legacy status, first-generation college student, recruited athlete, SAT score, admissions ratings of academic and personal qualifications (as sets of dummy variables), a cubic in family income, and the simulated loan under the rules that applied to the 1999 and 2002 cohorts. In columns 5–7, the instrument is the simulated loan for the actual cohort, assuming that the student enrolled in 4 consecutive years. Standard errors are in parentheses; bold coefficients are significant at the 10% level.

circumstances, the effect on whether the student plans employment is largely unchanged but that on whether the student plans to attend graduate school becomes significantly negative, suggesting that the original estimate was confounded by differences between students with and without financial need. Effects on both graduate school and employment decrease and become statistically insignificant (partly because standard errors rise substantially) in IV specifications although the point estimates indicate small positive effects on employment and negative effects on graduate school. We similarly find no debt effects in IV specifications for the probability that a student has found a job by the time of the survey.

The next rows examine the distribution of accepted jobs across industries. We see little impact of debt on the probability that a student takes a job in the high-salary groups of industries. By contrast, we find negative, statistically significant effects of debt on employment in the low-salary occupations. Specifically, in our preferred specifications in columns 6 and 7, we estimate that an extra $10,000 in student debt reduces the likelihood that an individual will take a job in nonprofits, government, or education by about 5 to 6 percentage points. Further investigation (not reported in Table 4) indicates that this is driven primarily by the education sector: in the specification from Column 6, $10,000 in student debt reduces the probability of taking a job in education by 3.3 percentage points (standard error 0.20).[33]

We next look at the impact of college loans on a student's actual starting salary.[34] Our preferred specifications indicate that an additional $10,000 in debt leads students to accept jobs that pay about $2000 (p-value 0.06–0.10) more in annual salary, on average, and reduces the likelihood that the salary is below $41,395 by about 6 percentage points (p-value 0.09).

Overall, it appears that college debt affects post-graduation employment decisions: students with more debt are less likely to accept jobs in low-paying industries and accept higher-paying jobs more generally.[35] These effects are quite large. The current Stafford loan interest rate is 6.8%, suggesting that a $10,000 loan paid off over ten years (the typical repayment period) would have annual payments of $1380. Our estimated salary effects thus imply that after-debt-payment income rises with debt. This may be explained by taxes: with a 33% tax rate, our point estimates imply that after-tax earnings rise approximately one-for-one with debt payments.

[33] 19% of Anon U students with education jobs teach in private schools; 26% work for Teach for America or for other similar organizations. Point estimates give no indication that either of these types of jobs accounts for the effect, though our samples are too small to permit precise analyses of the distribution of students *within* the education sector.

[34] We have examined the sum of the starting salary and the anticipated annual bonus, with qualitatively similar results. The 21% of students who have jobs but do not report their salaries are excluded from our salary analyses. We have explored imputing salaries for these students, using observed salary responses from other students working for the same firms. Estimates using the imputed data are similar to those reported in Table 4.

[35] An alternative potential explanation is a change in the selection of students into the subsample with jobs at graduation. If aid recipients prone to high-wage jobs were less likely to have jobs at graduation (relative to non-recipients with similar propensities) in the post-policy era, this could produce the results with no change in the relative preference for high-wage jobs. To assess this, we estimated for each student a predicted probability of taking a low-salary job or a job in a low-salary industry, using data from before the policy change and admissions and aid variables as explanatory factors. There is no indication that the association between this propensity and the student's financial need changed over time, either in the full sample or the employed-at-graduation subsample. We cannot fully rule out bias from changing unobservables in the employed subsample, but the absence of changes in observables is suggestive that such bias is unlikely to be large.

Table 5
Alternative specifications.

	Base	Using sum of student and parent loans	Exclude 2005 and 2006 cohorts and all low-income students	Exclude 2006 cohort and all low-income students	Control for SAT-cohort dummy interactions
	(1)	(2)	(3)	(4)	(5)
1st stage	**0.77**	**0.83**	**0.80**	**0.79**	**0.76**
	(0.01)	(0.08)	(0.02)	(0.02)	(0.01)
Plans employment	0.011	0.008	0.014	−0.018	0.018
	(0.018)	(0.019)	(0.027)	(0.022)	(0.019)
Has a job	0.006	0.005	0.006	−0.017	0.012
	(0.018)	(0.018)	(0.027)	(0.022)	(0.018)
Industry: Nonprofit/government/education	**−0.052**	**−0.043**	−0.028	**−0.062**	**−0.060**
	(0.028)	(0.026)	(0.043)	(0.035)	(0.028)
Salary	**2263**	2099	**3364**	**2819**	**2580**
	(1202)	(1327)	(1812)	(1519)	(1205)

Notes: Base specification in column 1 is from column 5 of Table 4. Other specifications change the endogenous variable (column 2) or the sample (columns 3 and 4), or add additional control variables (column 5). Standard errors are in parentheses; bold coefficients are significant at the 10% level.

Turning to our estimates of effects on occupational choices, one way to understand their magnitude is to consider graduates for whom credit constraints are perfectly binding. For these individuals, loans with annual payment p are equivalent to a reduction of the salary offered in each available occupation by p. An upper bound on the supply response to low-salary occupations can be obtained by imagining that salaries in these jobs are reduced by p, with all other occupational salaries held constant. The proportional supply reduction would be $(p/y_0)*\eta_0$, where y_0 is the baseline salary and η_0 is the elasticity of supply to these occupations. A back-of-the-envelope calculation indicates that our estimates are consistent with η_0 in the range of four or five. This is somewhat higher than estimates from the literature – estimates of the elasticity of supply to teaching are in the range of two or three (Manski, 1987; Dolton and Makepeace, 1993) – but is not unreasonable, particularly given the substantially better non-teaching opportunities available to high-ability Anon U students relative to typical teachers.

5.3. Robustness of employment results

Table 5 presents several alternative specifications – using different loan measures, samples, and control variables – meant to gauge the robustness of our employment, industry, and salary results. Column 1 repeats the estimates from Column 5 of Table 4 for several key outcomes. Column 2 adds to the endogenous debt measure the sum of any loans that students' parents have taken from Anon U, either via the PLUS program or from Anon U's unsubsidized parental loans office.[36] This has little effect on the estimates.

One concern with the earlier results is that we include students who might have applied or enrolled as a result of the program, and these students may differ in their career intentions from other Anon U students. Non-low-income students in the classes of 2002–2004 were grandfathered into the program after they had already enrolled at Anon U, so the program could not have affected their application and matriculation decisions. Column 3 of Table 5 excludes all low-income students and all students from the 2005 and 2006 cohorts from the sample, thus identifying debt effects solely from the partially-treated classes of 2002–2004. The occupational choice effect is smaller in this sample and is insignificantly different from zero, while the salary impact is larger than in our main sample. This sample, however, provides little power, as it discards all of the fully-treated students.

To probe the selection bias question further, we can add back a group of students for whom we can measure the impact of endogenous selection. Students from the 2005 cohort had already submitted their applications when the no-loans program was announced, so any endogenous selection in this group would come solely from decisions to accept admissions offers that would otherwise have been rejected. Column 4 includes non-low-income students from the class of 2005 in the sample. Estimated debt effects on both occupational choices and salaries are larger than in the main sample, and standard errors are about midway between those in columns 1 and 3. To assess the potential bias from endogenous matriculation decisions in the 2005 cohort, we use estimates from Linsenmeier et al. (2006), who find that the earlier elimination of loans for low-income students increased yields by at most 3 percentage points. Anon U admits about 500 students per year with financial need, so the effect of debt on yields accounts for no more than 15 students per cohort. Even if all of these students took jobs in low-salary occupations, this would increase the fraction going to such occupations by about two percentage points, less than one third of the estimated program effect. We thus conclude that application and matriculation responses to the no-loans program can explain no more than a small portion of our estimates.[37]

The macroeconomy weakened somewhat over our sample period, and this may have had differential effects on students with financial need. As a first attempt to evaluate this source of potential bias in our estimates, in Column 5 of Table 5 we add controls for interactions between the student's SAT score and cohort indicators. This specification will absorb changes in the labor market returns to ability. The estimated debt effects are essentially unchanged.

Of course, macroeconomic changes might have induced changes in the relative outcomes of students on aid that cannot be attributed to changes in the return to SAT scores. To fully evaluate this, we would have liked to difference out any such changes by comparing Anon U data with data from a peer institution with similar students but no change in its aid formula. Unfortunately, we have not been able to obtain such data. As an alternative, we use data from the National Longitudinal Survey of Youth 1997, a nationally representative sample that spans approximately the same cohorts covered by our Anon U data. We use the respondent's family income at age 17 to simulate the loan that Anon U would have assigned to the student and investigate the "effect" of this simulated loan on her salary at her first post-college job. In the Anon U data, the effect of the simulated loan on salaries (from the reduced form of the specification in Column 1 of Table 5) is 1532 (SE 809). In the NLSY data, an analogous regression of salaries on

[36] Our data on parental loans extend only through the end of the 2004–2005 academic year. We thus exclude students from the 2006 cohort in this column.

[37] There are two reasons to expect that this calculation overstates the impact of selection. First, it is based on an estimate of the sensitivity of yields to loans for low-income students; we would expect non-low-income students to be less sensitive. Second, our control function strategy will absorb whatever portion of the endogenous selection appears as differences in students' observed characteristics.

Table 6
Estimates of effects of $10,000 in loans on major and academic performance at Anon U.

	N	OLS			IV		
		Cohort dummies	+ controls for financial status	+ additional controls	Cohort dummies	+ controls for financial status	+ additional controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Division							
Social sciences	9166	0.000	**0.026**	0.011	**−0.034**	−0.003	−0.012
		(0.007)	(0.009)	(0.009)	(0.010)	(0.018)	(0.018)
Humanities	9166	−0.009	**0.016**	**0.018**	**−0.017**	−0.008	0.000
		(0.006)	(0.008)	(0.008)	(0.009)	(0.015)	(0.015)
Physical sciences	9166	0.005	−0.012	−0.010	0.012	−0.005	−0.010
		(0.005)	(0.007)	(0.008)	(0.008)	(0.014)	(0.015)
Engineering	9166	0.004	**−0.029**	**−0.018**	**0.039**	0.016	0.022
		(0.005)	(0.007)	(0.007)	(0.008)	(0.013)	(0.013)
Specific majors/certificates							
Econ. or engineering major or finance certif.	8792	0.001	**−0.033**	**−0.020**	**0.039**	0.020	0.025
		(0.006)	(0.009)	(0.009)	(0.010)	(0.017)	(0.017)
Non-renumerative major	8792	−0.001	**0.047**	**0.029**	**−0.042**	−0.019	−0.023
		(0.007)	(0.010)	(0.010)	(0.011)	(0.018)	(0.018)
Public affairs or teaching certificate	8792	−0.005	−0.005	0.000	−0.006	0.004	0.009
		(0.004)	(0.005)	(0.005)	(0.006)	(0.010)	(0.010)
Non-renumerative certificate	8792	0.001	0.005	0.006	−0.002	0.002	0.006
		(0.004)	(0.005)	(0.005)	(0.005)	(0.010)	(0.010)
GPA	7773	**−0.098**	**−0.088**	**−0.048**	**−0.091**	**−0.036**	−0.015
		(0.005)	(0.007)	(0.006)	(0.008)	(0.014)	(0.012)
Honors	8893	**−0.082**	**−0.075**	**−0.040**	**−0.071**	−0.028	−0.013
		(0.007)	(0.009)	(0.009)	(0.010)	(0.018)	(0.017)

Notes: See notes to Table 4.

simulated loans, controlling for cohort indicators and a cubic in the family income, yields a coefficient of −2346 (SE 15,457). Of course, the NLSY sample has a very different ability distribution than does the Anon U student body. When we limit the NLSY sample to the highest-SAT-score students (whose scores are nevertheless substantially lower than those at Anon U), the coefficient becomes even more negative. Thus, although the NLSY analyses are extremely imprecise, they offer no indication that macroeconomic trends could account for our Anon U results.

Finally, we have explored specifications that allow debt effects to vary with students' predetermined characteristics. Debt seems to have the largest effect on the salaries and employment choices of high-SAT students with low financial need, though these estimates are imprecise. There does not seem to be any substantial variation of debt effects with gender.[38]

5.4. Effects on educational outcomes at Anon U

The estimates presented above indicate that debt reduces students' probabilities of taking low-salary jobs. It may also have effects on students' job qualifications. Students who are or expect to be more financially constrained may study more (if alternative activities are expensive) or less (if term-time employment tightens the time constraint) than those with more disposable funds. On the other hand, students anticipating a desire to obtain a high-paid job after graduation may make different choices while at Anon U. Thus, we also examine the effects of debt on students' academic performance.

Results are presented in Table 6. Specifications are parallel to those in Table 4. The first rows show effects on the student major, as measured by the broad academic division (social sciences, humanities, physical sciences, or engineering). OLS estimates show that debt is associated with higher probabilities of majoring in the social sciences and humanities, at the expense of engineering. IV estimates without controls show the opposite effects. When we add our control variables in columns 6 and 7, both effects become insignificant, although the point estimates indicate a small shift toward engineering.

The next rows show effects on specific majors. Debt has a positive but insignificant effect on the probability of choosing an economics or engineering major, both of which are associated with access to high-salary jobs. It has a negative, imprecisely estimated effect on choosing a major from within a group that might be categorized as non-remunerative.[39] We find no indication of debt effects on academic minors ("certificates" at Anon U), including the public policy and teaching minors most closely associated with the employment outcomes seen earlier.

The final rows show models for students' GPAs and for whether they graduate with honors. In our basic IV-control function specification (column 5), debt seems to have large negative effects on each. However, when we control for students' entering academic credentials in column 6, these effects shrink substantially and become indistinguishable from zero.

On the whole, there is no indication that the debt-induced shift toward higher-paid jobs might derive from a positive effect on students' employability. Debt appears to have small effects on the choice of major, at most inducing a small shift toward majors that might be seen as oriented toward employment and away from "consumption"-type majors, and zero or small negative effects on academic performance.[40] It seems reasonable to interpret the earlier estimates of debt effects on employment outcomes as reflecting students' preferences rather than constraints imposed by their academic performance.

[38] There are too few black and Hispanic students at Anon U to permit meaningful analysis of these groups.

[39] We classify all of the humanities, history, history of science, anthropology, political science, and sociology as "non-remunerative." We include in the "economics or engineering" category students with other majors who earn minors in finance, many of whom are bound for financial industry jobs.

[40] When we exclude the partially-treated classes of 2002 and 2003, who might have chosen majors before the announcement of the no-loans policy, results are qualitatively similar, with one exception: The negative effect of debt on GPA is more robust in this sample and persists across all of the specifications in Table 6.

5.5. Effects on alumni giving

Finally, we consider the effect of student loans on annual alumni giving to Anon U. There are three reasons to expect effects of debt relief on alumni gifts. First, pure income effects may lead to slight increases in donations. Second, if students perceive debt relief as a gift from the University, increased donations may be a way to show their gratitude. Third, if debt causes students to be constrained after college, it will also increase the shadow cost of early-career contributions and therefore debt relief would increase their level. Data on pledges can also inform our analysis. Students approaching graduation seem likely to be able to anticipate their desire to give to the university, but may not anticipate the constraints that they will face in the "real world." We compare actual gifts with those pledged during the senior year, when the Anon U Development Office asks students to commit to their annual gifts for the next five years. Differences between what students pledge and what they actually give can be seen as evidence of unanticipated financial difficulty (or bounty).

We present estimated effects in Table 7 for five measures of alumni gifts. Each specification uses the IV-control function specification from Column 6 of Tables 4 and 6. In Column 2, the dependent variable is an indicator for whether the student pledged a donation. In Column 3, it is an indicator for actually donating. Columns 4 and 5 examine the amount of the pledge or donation, assigning zero for students who did not participate. Finally, Column 6 examines an indicator for whether or not the gift fell short of the pledge — this occurs about one quarter of the time in the first year after graduation. The rows of the table examine gifts in different years: the first row presents results for gifts during the first year after graduation (62% participation, unconditional mean gift $21) and for pledges concerning gifts during that year; the second row results for gifts during the second year (64% participation, mean gift $27); etc. Note that the sample sizes are notably smaller in the bottom rows. We have data only through the summer of 2006, so cannot observe gifts during the 4th year for the class of 2003, 3rd year for the class of 2004, 2nd year for the class of 2005, or any year for the class of 2006.

The results suggest that college debt has no effect on whether students pledge donations (column 2). Debt does appear to have negative effects on whether students actually give (on the order of 3 percentage points per $10,000 in loans), though these are only marginally statistically significant (p-value 0.07 for year 1, 0.13 for year 2). We see similar patterns for amounts: effects on pledges are small (except in year 4, for which the sample includes only the classes through 2002), while there are somewhat larger effects on actual gifts. Column 6 indicates that debt has significant positive effects on the probability of falling short on a pledge, at least in the first year after graduation.

Table 7
IV-Control function estimates of effects of $10,000 in loans on alumni giving.

	N	Participation (0/1)		Amount		Fall short of pledge
		Pledge	Gift	Pledge	Gift	
	(1)	(2)	(3)	(4)	(5)	(6)
Year 1	7768	−0.001	**−0.035**	−0.5	**−3.4**	**0.047**
		(0.017)	(0.019)	(0.9)	(1.9)	(0.017)
Year 2	6629	0.000	−0.034	−1.1	−1.6	0.029
		(0.020)	(0.023)	(1.3)	(2.5)	(0.023)
Year 3	5516	0.009	−0.028	−1.3	**−7.9**	0.031
		(0.026)	(0.028)	(2.0)	(3.6)	(0.029)
Year 4	4402	−0.036	0.027	**−8.1**	−0.6	−0.023
		(0.037)	(0.041)	(4.8)	(6.0)	(0.044)

Notes: Samples exclude cohorts for which gift data are not yet available (class of 2006 in row 1, 2005–2006 in row 2, 2004–2006 in row 3, and 2003–2006 in row 4). Bold coefficients are significant at the 10% level.

We interpret Table 7 as providing further, indirect evidence that students do not follow the life-cycle model in the first years after college graduation. The effects are small in absolute magnitude but are reasonably large relative to the average gift from a recent graduate.[41] Moreover, the estimates of larger effects on actual gifts than on pledges and of negative effects of debt on the probability of fulfilling a pledge offers suggestive evidence that can help to distinguish credit constraints from debt aversion as explanations for the failure of the life-cycle model: this result suggests that recent graduates are surprised by the effect that debt has on them, a reaction that seems more consistent with unexpected credit constraints than with foreseeable debt aversion.

6. Generalizability of findings

An important question concerns the generalizability of our results. Our analysis derives from a sample of students at a particular school, and our results might not extend to typical college students. There is at least good reason, however, to suspect that debt effects should be *larger* for typical students than for Anon U graduates. Table 8 presents comparisons of academic and financial characteristics of Anon U students from the 1999–2001 cohorts with those of nationally representative samples of aid recipients in various categories.[42] We consider three comparison samples of institutions. The narrowest category consists of private four-year institutions that are classified (according to the Carnegie taxonomy) as Research I and II, PhD granting I and II, comprehensive I and II, or liberal arts I and II.[43] Column 3 adds 4-year public institutions in the same Carnegie classifications. Finally, Column 4 includes students from all 4-year schools. Students receiving need-based financial aid represent approximately 40% of Anon U's seniors, 67% of 18–24-year-old seniors at comparable private institutions, and 48% of students at comparable public and private institutions or at all four-year institutions.

Anon U is one of the most selective schools in the country, and in an academic sense its students are clearly unrepresentative. They have much higher SAT scores and are more likely to have attended private high schools than their counterparts in any of the comparison samples. Aid recipients at Anon U are wealthier than students on aid nationwide, but generally are reasonably comparable to aid recipients at private colleges. For example, the mean family income among students receiving need-based financial aid at Anon U is approximately $93,000 (in 2005 dollars), whereas private college aid recipients have average family incomes of $84,000 and aid recipients overall have average incomes of $74,000. Approximately 30% of Anon U aid recipients and 36% of those at comparable private schools have family incomes below $60,000 (which corresponds roughly to the upper threshold for the "middle-income" category in Fig. 1). Aid recipients at Anon U thus come from somewhat wealthier families than do those at less selective institutions, though the differences are not large.

Table 8 also shows statistics for student debt. Students receiving financial aid at Anon U before the no-loans program incurred an average of $16,597 in educational debt over their college careers. Cumulative debt levels were nearly double that amount in

[41] Median nominal contributions from the class of 1985 over the first 20 years were 23.5 times the four-year median for that class. If debt effects grow at the same rate, the total effect of $10,000 in debt reduction will be to increase donations by $318, only a fraction of the cost to Anon U of replacing $10,000 in loans with grants. Of course, donations are highly skewed, and a single large donation could overturn this calculation, as could increases in mean donations in the years beyond 20.

[42] The comparison samples consist of dependent students aged 18–24 from the 2000 NPSAS (Riccobono et al., 2002) who were enrolled full-time in 1999–2000 and graduated in that year. We select in both the NPSAS and the Anon U data on receiving aid during the senior year. Characteristics of Anon U students from later cohorts are similar in all dimensions except average student loans.

[43] Anon U is a Carnegie Research I school.

Table 8
Comparison of Anon U and nationally representative aid recipients.

	Anon U: aid recipients from the classes of 1999–2001	1999–2000 NPSAS: full-time, 18–24-year-old dependent seniors with positive aid		
		Comparable private	Comparable public and private	All four-year Institutions
	(1)	(2)	(3)	(4)
SAT	1407	1102	1089	1087
	[116]	[177]	[174]	[173]
Race				
White	60%	80%	75%	74%
Asian	16%	3%	6%	6%
Black/Hispanic	24%	14%	18%	18%
Private HS	33%	21%	15%	15%
Family Income	93,251	83,859	74,427	74,278
	[52,092]	[49,149]	[46,337]	[46,264]
Family income <$60,000	30%	36%	43%	43%
Cumul. federal student loans	16,597	30,479	26,985	27,039
	[7877]	[21,884]	[21,260]	[21,072]
N	1341	1332	2491	2588

Notes: Means; standard deviations in brackets. "Comparable" schools include public and private not-for-profit colleges and universities in the Carnegie categories Research I and II, PhD Granting I and II, Comprehensive I and II, and Liberal Arts I and II. NPSAS sample is restricted to full-time dependent seniors aged 18–24 with positive need-based aid in their senior years, and is weighted by the study weight. Anon U sample is similarly restricted to students with positive need in the senior year. All dollar figures are in 2005 dollars.

comparable private colleges and universities and slightly less for graduates from all institutions.

Table 8 offers several reasons to expect that debt effects will be at least as large for typical students as for Anon U students. First, debt levels at Anon U are relatively low, and Anon U students are for that reason less likely to reach any given debt ceiling than are students from other schools. Second, Anon U students earn higher salaries after graduation than typical college graduates. An analysis of Current Population Survey data indicates that college graduates aged 21–24 and employed full-time throughout 2001 had mean salaries of $36,800 (in 2005 dollars), far below the average of around $50,000 for employed Anon U students (Table 3). Even if access to credit is independent of earnings, high salaries might reduce the utility cost of constrained consumption and thereby reduce the effect of debt on job choices. Third, Anon U students' parents have relatively high incomes, and may be able to offer intra-family loans that permit consumption smoothing without employment distortions. Finally, Anon U's students' higher SAT scores may indicate that they make better decisions, which again might reduce the effect of debt on choices.

7. Conclusion

There is widespread concern about the level of debt incurred by those acquiring a post-secondary education. Among the concerns is that debt burdens distort graduates' post-schooling decisions. But in standard economic models, with well-functioning credit markets, student debt should have only income effects on career and consumption decisions of life-cycle optimizers; since student debt composes a small portion of an average college graduate's lifetime earnings, these effects should be quite small. In this view, debt is the ideal mechanism for financing college education, as it permits a student to internalize the full costs of her human capital investment decisions. There is no reason to think that high levels of student debt represent a market failure that warrants intervention.

In the standard model, Anon U's no-loan program should have had very small effects on its beneficiaries' career choices. This is not borne out by the data. When students were relieved from the need to incur debt, they shifted toward lower-salary jobs in public service industries. The point estimates indicate that changes in employment choices were large enough to entirely offset the effect of student debt on average after-tax, after-loan-payment earnings in the first years after graduation. The standard model cannot rationalize a response of this magnitude.

Our paper adds to an existing body of evidence that consumer behavior is poorly characterized by the life-cycle model. The most plausible explanations for our results are that recent college graduates are averse to holding debt or that they face constraints on their ability to borrow against future earnings, either of which could lead to non-trivial effects of student debt on occupational choices. We have limited ability to distinguish between these competing explanations. We find suggestive evidence that debt reduces students' donations to Anon U in the years after they graduate and increases the likelihood that a graduate will default on a pledged gift, indicating that seniors do not fully anticipate the effects of debt. We believe this finding is more consistent with credit constraints than debt aversion, as it seems likely that seniors will be able to anticipate their future debt aversion and less likely that they will correctly forecast constraints on their ability to borrow. This conclusion is necessarily tentative, however.

There are many outstanding questions about the role of debt in decision-making that we do not address. We have no direct evidence, for example, that student loans crowd out other forms of borrowing. If student debt prevents graduates from obtaining home mortgages – which are typically taken out several years after college graduation – effects on utility could be larger than those captured by our employment analyses. Similarly, we do not know whether the debt effects on immediate post-college employment that we observe will persist throughout graduates' careers. Another important avenue for further investigation concerns the effect of post-graduation credit constraints on pre-college decisions. If young people anticipate that taking on debt will constrain their consumption choices early in their careers, even free access to student loans will not lead to optimal educational investment. Optimal design of college financing mechanisms will require a deeper understanding of the role of debt in decision-making and a better characterization of the availability of affordable debt to young people today, both during college and beyond.

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