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Student loans and the labor market: Evidence from merit aid programs

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Student Loans and the Labor Market: Evidence from Merit Aid Programs

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Abstract

Student loans are a growing part of the college funding equation in the US, while merit aid scholarship programs have become a popular avenue for states to subsidize higher education. I use merit aid program eligibility in the thirteen states that have sharp test score cutoffs for eligibility to evaluate the effects of college funding on the early labor market outcomes of college graduates. I examine the heterogeneity of the effects with respect to both ability and family income. I demonstrate that qualifying for a merit aid program lowers the loan burden of students by $7200, and has little impact on other outcomes while in school. However, employed students who qualify for merit aid programs have $6400 lower annual income one year after graduation, and a different occupational profile four years after graduation than those who just missed qualifying for the programs. Because merit aid eligibility changes little of the college experience other than the funding package, it functions as an instrument for loans. This implies that exogenously increasing the loan burden of a college graduate by $1000 increases her annual income by $600-$800 one year after graduation. Examining the heterogeneity of these results by both ability and family income suggests that these effects stem from credit constraints when individuals leave school. Together these results demonstrate that while merit aid scholarships may provide students with more flexibility to seek out jobs with non-pecuniary rewards, there is no detrimental financial impact of instead financing college with loans.

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1 Introduction

How higher education should be funded is a major source of policy discussion in the US today. Many states have implemented large merit-based scholarship programs, while the federal government has heavily invested in subsidized loans and need-based grants to students attending college. Little research has been done to empirically evaluate the effects of either of these policies on college graduates as they leave school and enter the labor market. This paper will to fill this gap by exploiting sharp eligibility cutoffs in state merit aid programs to evaluate the impact of the programs on students as they enter the labor market and to determine the extent to which those effects operate through changes in loan burden. I demonstrate that merit aid programs primarily offset the amount of loans students take on with minimal impact on academic outcomes. I find that students who qualified for merit aid seek out jobs after graduation that are lower paying, but presumably have higher non-pecuniary benefits, than their peers who did not have access to the scholarships. Because the primary effect of the merit aid programs is to offset loans, and not to measurably changes anything else about the college experience, I interpret this as an effect of changing the funding package directly. Furthermore, by examining the heterogeneity of the results with respect to individual ability and family income, I conclude that the results are driven primarily by short term credit constraints after graduation.

Over the past twenty five years 20 states have implemented merit-based aid programs. Merit aid programs have largely been marketed as a way to stem "brain drain," keeping smart students in their home states for college and their subsequent careers. In practice these programs represent a large transfer of funds from states to students pursuing higher education. These programs determine eligibility based on state residence and on measures of high school performance, such as national exam scores, high school GPA, or high school class rank. The scholarships typically involve no selective application process. To take one of the largest programs as an example, the Bright Futures program in Florida covers at least 75% of the cost of public in-state school tuition[1]. To qualify for the program, students must score at least 20 on the ACT[2], graduate high school in the state of Florida with at least a

[1]The scholarship also provides at least 75% of the average of public in-state school tuition to students who qualify and choose to attend a private in-state school

[2]Or at least 970 on the SAT.

3.0 GPA and complete a number of service hours. Surveying the programs across all twenty states, there is a great deal of heterogeneity in terms of requirements and benefits. They contribute anywhere from 20% to 110% of public in-state tuition per year, representing a large reduction in the cost of attendance. Of the 20 states in the US offering merit aid programs to their high school graduates, 13 have ACT[3] test score cutoffs that either significantly or entirely inform eligibility for the scholarships, providing a sharp discontinuity in the amount of available scholarship assistance.

Federal policy towards the majority of students during this same time span has taken the form of subsidized student loans. Loans have quickly become a large fraction of the college funding equation for undergraduates: 60% of undergraduate degree recipients in 2012 graduated with debt, averaging $26,500 for each indebted student (College Board, Trends in Student Aid 2013). There has been a very vocal policy discussion over the past few years as the total stock of outstanding loan debt surpassed credit card debt in 2012, and recently exceeded $1.3 trillion (www.finaid.com). Current presidential candidate Bernie Sanders has even suggested making tuition at public colleges and universities free in response to the growing discontent with the loan balances students are taking on.

Despite the vocal policy discussion, there is little empirical understanding of the effect of college funding on students as they leave school. This is the motivating focus of this paper. To address this broad question of interest, I will answer three specific questions. First, how do state merit aid programs affect the college funding decisions and post-graduation outcomes of eligible students? Second, how much of these effects can be attributed directly to differences in student loans? Finally, how do these effects vary by student ability and family income and how does this inform the channels at work driving the observed main effects?

This project fits into a broader conversation about the impacts of college funding on student outcomes. I find that qualifying for merit aid significantly changes the amount of loans with which students graduate but changes little else about the academic experience, which suggests merit aid as a plausible instrument for loans. While the permanent income hypothesis would suggest that borrowing against future income (with student loans) should not affect behavior, several frictions may be acting on recent college graduates. Loan repayments start within six months of graduation, during which credit or liquidity constraints may cause students to

[3]All programs allow qualification based on ACT or SAT score interchangeably.

accept earlier job offers than they otherwise might without the specter of loan repayments overshadowing their search. Similar forces may induce loan takers to search for different *types* of jobs if they are concerned about the magnitude of debt they have to repay. For instance, a graduate $30,000 in debt may search exclusively for high paying jobs in order to begin paying back his debt. By contrast, a graduate who paid for his education largely with scholarships may be able to seek out jobs that have non-pecuniary benefits such a flexible work environment, or jobs with long term benefits such as unpaid internships that potentially lead to lucrative careers.

A straightforward regression of salary on loans produces the correlation about which policy-markers are concerned[4]. As can be seen in figure 1, there is a negative correlation between loans and the salary of full-time employed graduates one year after graduation. There is a similar negative correlation between loans and the probability of enrollment in post-baccalaureate education one year after graduating with a BA degree. These correlations may, however, be the result of significant unobserved selection. For instance, income and post-baccalureate enrollment are positively correlated with family wealth, which is also negatively correlated with loan burden. To consider an extreme example: it is unobservable whether a student has a rich grandfather who will fund her college education and also offer her a lucrative job in his company after graduation. However, in this and the more general case of families offering assistance with both college costs and job searches after graduation, loans are not to blame for the negative correlation with salary. Rather, the underlying effect of family networks and wealth is responsible. Using sharp discontinuities in eligibility for state merit aid programs circumvents these selection issues and allows me to identify the causal effect of student loans on post-college outcomes.

For both the evaluation of merit aid programs and the causal estimates of the effect of loans on outcomes after graduation, a data source is required that links information on students' initial eligibility for merit aid programs to their college experiences and outcomes after graduation. The Baccalaureate and Beyond restricted-use dataset administered by the National Center for Education Statistics meets these criteria by collecting detailed high school and college information on the 2007/08 cohort of bachelor's degree graduates, then following those graduates as they enter the labor force over the subsequent four years.

[4]Author's calculations using Baccalaureate and Beyond 2008/2012 data.

Using both regression discontinuity and difference in difference approaches, I find that students who are eligible for merit aid programs accumulate $7200 less debt than otherwise observationally equivalent students, and earn $6400 less in annual income one year after graduation than those who funded their education with higher amounts of loans. A reasonable explanation for the observed differences in income might be that students who benefited from merit aid programs are more likely to be enrolled in post-baccalaureate education (and thus have little or no income) because they are less financially constrained by spending on their undergraduate degree. However, there are few differences between merit aid-eligible and ineligible students in their rates of post-baccalaureate enrollment and employment. Students who are eligible for merit aid are somewhat more likely to be unemployed, but the finding that graduates earn less one year after graduation persists and is in fact stronger when the sample is restricted based on employment and enrollment status. While the difference in income disappears by four years after graduation, stark differences in occupations emerge. Students who benefitted from merit aid are far less likely to be employed in business occupations four years after graduation, but are somewhat more likely to be employed in education occupations. These results are suggestive of the broader employment choices of college graduates, with education being the classic example of a low paying but fulfilling career.

Since merit aid eligibility changes primarily college funding and nothing else about the college experience, I use merit aid as an instrument for loans to identify the causal effect of loans on outcomes after graduation. This analysis implies that an increase of $1000 in student loans generates a causal increase of $600 - $800 more in annual salary for graduates one year after graduation. Applying the same approach to occupational choice four years after graduation demonstrates that an increase of $1000 in student loans results in students being 2 percentage points more likely to be employed in business and 1 percentage point less likely to be employed in education. These results suggest that students with loans are not nearly as adversely impacted, in a financial sense, by their loans in the labor market as the popular press would have us believe, and furthermore that they are financially able to offset the amount of their loans relative to their non-indebted peers within two to three years of graduation.

Finally, I explore the heterogeneity of these results to ability by taking advantage of the wide variety of ACT cutoff scores required for eligibility in different states, and to family income by

taking advantage of the cross-section of family backgrounds represented in the sample. While other projects in the literature have collectively studied all merit aid programs (Dynarski, 2004; Sjoquist and Winters, 2012; Hawley and Rork, 2013; Cowan and White, 2015), this is the first to my knowledge to exploit both the sharp eligibility cutoffs for identification of both the main effects and of heterogeneous effects by ability, as well as the first paper to examine the heterogeneity of the results with respect to family income prior to graduation. I find that while the main results are broadly consistent across different groups of students, the main results on income are stronger up the ability distribution and down the family income distribution, suggesting that the results may indeed stem from credit constraints. We might image that the most intelligent students have the most choice of occupations, and thus would be most able to react to the credit constraints discussed, while students from the highest family income levels would be the least subject to credit constraints as they would be able to rely on family resources to bridge the gap between graduation and employment.

Together these results suggest that the method by which a college education is funded, be it by merit scholarships or by loans, has a significant impact on the initial labor market experiences of college graduates, and that these effects should be considered when contemplating changes to merit aid programs or to federal student loan policies. More generally, they suggest that credit constraints may play a large role in the job search process, and in individuals' assessment of the relative importance of pecuniary and non-pecuniary benefits of different jobs. As a result, this research is significant both for developing an understanding of how current financial aid policy impacts college graduates in the long run as well as what frictions may be salient in job searches more generally.

This paper proceeds as follows. Section 2 reviews the literature on student loans and state merit aid programs, and describes how this paper contributes to both literatures. Section 3 describes the data used in this study in greater detail. Section 4 presents the methodology used for evaluating the merit aid programs, as well as the results of that program evaluation. Section 5 extends the methods outlined in section 4 and describes the assumptions required to use merit aid eligibility as an instrument for loans, then presents the results of that analysis. Section 6 examines the heterogeneity of results with respect to family income and differences in student ability. Section 7 concludes by discussing policy implications and avenues for future

research.

2 Literature Review

Despite the public debate over the costs of student loans the literature on the topic is limited. The three most rigorous empirical papers assessing loans are Field (2009), Rothstein and Rouse (2011) and DesJardin and McCall (2010). They all find evidence that students with debt select higher paying occupations more often than their peers without debt, though none can examine income after graduation directly[5]. While convincing, these studies are all conducted on highly selected samples so it is unclear whether their results would generalize to a more representative population. Field (2009) studies an experiment randomizing financial aid packages offered to law students at New York University. Although the packages are constructed to be financially equivalent, she finds that packages framed as loans rather than grants induce graduates to more often take jobs in corporate law rather than public interest law fields. This study highlights the possible psychological impact of loans on students. DesJardin and McCall (2010) examine a sample of Gates Millennium Scholars, high-ability students from specifically low-income minority backgrounds using a regression discontinuity approach. They find that the scholarship does increase the grades and further educational aspirations of recipients, but they are unable to examine outcomes beyond graduation as I will be able to do in this study.

Rothstein and Rouse (2011) conduct the most closely related analysis to mine by using a change in the financial aid policy of a highly selective, elite private college to determine the effect of loans on the outcomes of graduates. They find that students with more loans seek out higher paying jobs, and are less likely to donate to the school within in the first few years of graduation. I will add to this burgeoning literature on student loans by using broad, state-based merit aid programs that have significant variation in their requirements. This will allow me to address whether the results found in these papers are generalizable across both the ability and family income distributions.

Earlier inquiries rely largely descriptive analyses of differences between graduates with and without loans. These studies paint a bleak picture as they imply that graduates with student loans have far lower lifetime wealth and asset accumulation than their peers without loans

[5]Rothstein and Rouse report effects on the income of accepted jobs that will be taken after graduation.

(Hiltonsmith 2013, Elliot, Grinstein-Weiss and Nam 2013, Chapman and Lounkaewa 2010). The largest weakness in these papers, however, is that they are correlational studies of loans and outcomes over the lifetime of graduates that suffer from the same selection issue outlined in figure 1. Both Minicozzi (2005) and Zhang (2013) use large nationally representative samples, though the data largely predate the expansions in student loans in the 1990s and 2000s that are of greatest concern to policy makers. Minicozzi finds that greater loan amounts are correlated with higher initial wages after leaving school, but lower four-year wage growth. Her study is based on a control-variables strategy only, however, so it does not address the potential selection issues that are addressed by the quasi-experimental variation that is used in this paper. Zhang address the potential selection issues by using an instrument for loans based on the generosity of a college's aid policies. While this instrument is promising, the amount that a particular school invests in financial aid could plausibly be related to its broader investment in students, violating the exclusion restriction. For instance, a school that offers a great deal of financial aid may also invest heavily in the job search process for their students. This type of concern may also arise with my instrument (state-based merit aid), though the mechanism in this paper would need to be operating at a broad scale across many schools to violate the exclusion restriction, rather than at the level of individual school. Zhang finds limited effects of loans on outcomes other than the probability of starting graduate school. Gicheva (2012) also uses a large nationally representative sample, derived from the Survey of Consumer Finances and a panel data set of GMAT test takers, to study the effects of loans on family formation specifically. She uses an instrument based on the rate of expansion of federal student loans to find that greater student loan debt is associated with a lower probability of being married. This is interpreted as evidence that students with debt are constrained in their choices after college, a conclusion that the results of this paper support.

The instrument used to identify exogenous differences in loans in this paper is based on merit aid programs. Program evaluation of these programs is independently interesting and is the subject of a broad literature focusing on a wide variety of outcomes realized while in college, such as enrollment rates, persistence rates, and even consumption outcomes such as car sales (Cornwell and Mustard, 2007) or binge drinking (Cowan and White, 2015). Most of this literature is limited to case studies of particular programs, both those in states with merit

aid programs such as Georgia (Dynarski 1999, 2000; Sjoquist and Winters 2015; Cornwell, Lee and Mustard 2006; Cornwell and Mustard, 2007), Tennessee (Pallais, 2009; Ness and Noland, 2007; Bruce and Carruthers, 2011), West Virginia (Scott-Clayton, 2011) and Massachusetts (Goodman, 2008; Cohodes and Goodman, 2014) and programs that are not state-based, such as the Kalamazoo Promise in Michigan (Andrews, DesJardins and Ranchhod, 2010; Bartik and Lachowska, 2012) and the Gates Millennium Scholarship (Malguizo, 2010; DesJardins, McCall, Ott and Kim, 2010; DesJardins and McCall, 2014).

To highlight a few papers of this literature, Dynarski (2004) and Cornwell, Mustard and Sridhar (2006) examine the largest and most well-known of the merit aid programs, the HOPE program in Georgia. They both find that there are increases in enrollment at four year institutions because of the scholarship, though Cornwell, et. al. highlight that the enrollment effects are largely driven by changes in the composition of enrolled students within the state, rather than students being induced to enroll in school. That is, they find suggestive evidence that the scholarship changed where students attended college, but likely not the choice of whether to attend college.

Several other papers have examined features and outcomes of particular programs, such as Pallais (2009) examining the effect of the Tennessee HOPE program on student performance on the ACT test and on enrollment in Tennessee schools. She finds that there is evidence the program caused students to exert greater effort on the ACT and achieve higher scores. I will test whether this sort of "gaming" of the test is evident in my sample by showing that the characteristics of students on either side of the score cutoffs are balanced in the aggregate. While there does seem to be a mass of students above the centered score cutoff there is a smooth transition exactly over the cutoff implying that the test is not *precisely* manipulable, which is sufficient for my purposes.

Few papers have leveraged all merit aid programs together to evaluate either the effects of the programs or related first stage variables such as loans. Sjoquist and Winters (2012) examine the college completion effect of merit aid programs in all merit aid adopting states and find no evidence that merit aid programs change college completion rates of young people, while Hawley and Rork (2013) examine the migration patterns of students over time to conclude that the programs succeeded somewhat in their stated goals of keeping bright students in their

home state for college. Cowan and White (2015) examine the consumption effects of such scholarships by examining their effects on the rates of binge drinking among students affected by the programs. Despite the large literature on this topic, and the growing literature that takes advantage of the full set of states that have implemented these programs, none of the papers in this literature has yet taken advantage of the sharp ACT score cutoffs across multiple programs as I will be able to do in this study, nor have there been in-depth analyses of the effects the programs have on outcomes after college completion.

3 Data

In order to examine the impacts of merit aid programs and student loans on outcomes after graduation, data is required that matches students' outcomes prior to college enrollment with measures of their college funding as well as with their outcomes after graduation. This data must then be matched with eligibility requirements for the various state merit aid programs across the country. In this section I describe the data on merit aid programs exploited as well as the Baccalaureate and Beyond data set used for this project.

3.1 State Merit Aid Programs

For this study I use the existence of large state merit aid programs and the eligibility for those programs both as a treatment variable of interest as well as an instrument for the cumulative loan total of a student in that state. To make use of this variation, I compiled a database of state merit aid programs and their features from several different sources. Eligibility requirements, application process and potential awards of the programs were distilled from state government websites, then combined with a dataset on all state aid programs available from the National Association of State Sponsored Grant Aid Programs (NASSGAP). This dataset includes information on the existence and size of all aid programs across all states and DC from 2003-2013. This information was then matched the data from each state to enrollment records from the IPEDS database on institutions of higher education in the US. From these data I produced a measure of the percent of flagship tuition[6] the award represented in the

[6]Total tuition and fees for the full academic year of 2007/08 at the in-state, flagship institution.

2007/08 academic year observed in my dataset (program generosity) and the percent of the enrolled student population at public institutions receiving the award (program selectivity).

A program is deemed "eligible" for my analysis if it meets the following criteria:

1. Eligibility is based on measures I am able to observe in the dataset of individual students (test scores and state of residence). This excludes some states with large merit aid programs that are based primarily high school GPA or class rank. The most notable of those are Georgia and South Carolina. Although they have very large and generous merit aid programs, I cannot identify which students were eligible for the programs, thus they will not be included as "program states" in this analysis[7].

2. No selective application process is required to earn the scholarships, and all qualifying students are funded. Enrollment in these programs is very close to automatic. As long as students meet the eligibility criteria and attend one of the schools designated by the scholarship program, there are typically no barriers (such as interviews or specialized applications) to receiving the scholarship beyond those required of any student applying for financial aid. Importantly, none of these programs are selective in the sense that a subset of qualifying students are chosen to receive the scholarship. Rather, if all students who qualify for the program elect to attend a school designated by the scholarship, all students who qualify would receive the promised grant support.

3. Students in the graduation cohort of 2007/08 were exposed to the program. States must have enacted the program early enough for the students observed in my sample to be affected. In practice this only disqualifies the most recent program adopted, the Hathaway scholarship in Wyoming.

This leaves me with a set of 13 states with merit aid programs, listed in table 1. A map showing the geographic dispersion of the used and unused programs appears in figure 2. There is significant geographic variation in the states that have passed such programs, though the programs are more common in the South.

A sense of the variety of the programs can be gained from looking at figure 3, which shows the ACT score cutoff relative to the program generosity. The programs have ACT score

[7]As a robustness check I drop these states from the analysis entirely and re-run the results. The results are unchanged by this alternative definition of "control" states.

cutoffs that range from 15 to 30. This variation will allow me to examine the differences in the treatment effects across the ability distribution, since students in different states will qualify at different scores. A student who just qualifies for a merit aid program in Mississippi or Kentucky, with an ACT score of 15 that over 80% of the national test taking population will achieve, is very different from a student who just qualifies in Missouri, with a program that by definition restricts the scholarship to the top scoring five percent of test takers in that state. The amount awarded by each program also varies widely across states. Some make awards that approach the full cost of tuition, such as Tennessee, West Virginia and New Jersey, while the maximum award available in some states, such as Idaho and South Dakota, falls short of even 20% of tuition at the in-state, flagship institution. Note that this relationship does not follow a standard "budget constraint" relationship, that is, the selectivity of the program does not seem to be related at all to the generosity of the award.

In this analysis I will exclusively be using ACT score to identify the effects, for several reasons. First, it is a finely grained measure of academic achievement that is exactly the measure states have used all or in part to determine eligibility status for merit aid programs. Second, because it is a nationally based exam with infrequent sittings and the score based on a single morning's effort, the score is less manipulable than measures that students can more readily observe in real time, such as high school GPA. There is a concern that students may be able to retake the test in order to become eligible for the programs, though I will address this concern by examining the data for evidence of this behavior. Third, while some students have an SAT rather than ACT score, students in all states are allowed to qualify with either test score, and the conversion between the two is consistent with the score crosswalks published by both administrative bodies[8]. Finally, high school GPA is not used in this analysis, even though it would allow me to include more merit program states. The data I have is too coarsely binned to determine whether students qualified based on their GPA. Several of the GPA cutoffs fall in the interior of the bins defined for the high school GPA variable.

[8]Because the crosswalks are consistent, I will use the ACT scores for students with ACT scores, the converted ACT scores for students with SAT scores, and the higher of the raw ACT or the converted ACT score for students who have both scores

3.2 Baccalaureate and Beyond

In order to examine how merit aid programs influence students' take up of loans and subsequent outcomes after graduation, a dataset is required that links information on performance in high school (particularly ACT scores) to cumulative loan amounts and outcomes after graduation. The Baccalaureate and Beyond (B&B) dataset is a restricted-use panel dataset administered by the Department of Education's National Center for Education Statistics (NCES) that fulfills these requirements. It is a nationally representative sample of 11,000 bachelor's degree graduates from the class of 2007/08. It includes information on pre-college characteristics, family background, college undergraduate experiences, and two waves of survey data after graduation: one year after graduation in 2009 and four years after graduation in 2012. There are several features of this data set that make it particularly suited to my purposes. First, high school test scores from the ACT and the SAT are collected directly from the testing agencies, providing a high-quality measure of whether students would have qualified for merit aid in their state of residence. Second, characteristics about family background and family income are imported directly from the Free Application for Federal Student Aid (FAFSA), providing high quality measures of family circumstances prior to college graduation. Third, the dataset includes detailed measures on college funding, including cumulative undergraduate loans, and the amount of state merit-only grants students received in their final year of school. Fourth, students are surveyed after graduation, and detailed information on their experiences after college is collected. This includes data on annual income, occupation and graduate school enrollment. In particular the three part link between high quality testing data, college funding and experiences after college is vital for this analysis.

For the purposes of this study I restrict the sample to students who have a non-missing observation for high school ACT or SAT score, effectively restricting the sample to students between the ages of 18 and 24 upon college graduation, and who were considered dependents for the purposes of financial aid prior to graduation.

Selected summary statistics about this sample are detailed in table 2. The sample is over 80% white and close to 60% female, consistent with broader demographic trends in the population of individuals graduating from college. Average family income is quite high, though this is a highly selected sample of households, namely those with students graduating from

a four-year college or university on a traditional schedule, which could partially explain the high household income. Students have close to $15,000 of student loans on average. This figure is inclusive of the almost 40% of students in the sample who graduate with no loans, putting the average loan burden of a student with loans at roughly $24,000, consistent with national trends. A majority of students graduate from public schools in their home state[9], with a cumulative undergraduate GPA of about 3.2. One year after graduation almost 70% of individuals are employed full time, with an average annualized salary of about $35,000. Four years after graduation close to 40% have completed or are enrolled in a post-baccalaureate degree program, and annual salary among those who are employed full time has risen to exceed $45,000.

This table also gives a sense of what fraction of students are potentially exposed to merit aid programs. Just under 20% of the sample live in states with merit aid programs, with over half of those qualifying for merit aid programs in their state. This binned data masks a large amount of heterogeneity across programs and states in the fraction of students who qualify for the programs. Since there is such variation in the ACT score cutoffs across states, virtually all students in some states qualify for the programs (such as in Mississippi and Kentucky with a qualifying score of 15), whereas very few students qualify for the programs in other states (such as in Missouri with a qualifying score of 30).

4 Labor Market Impacts of Merit Aid Programs

To evaluate the effects of merit aid programs on college funding and the labor market outcomes of students after graduation, I use two different specifications. The first restricts attention to only the 13 states that have sharp eligibility cutoffs for program participation, listed in table 1 and shown in figure 2. The essence of this regression discontinuity approach is to compare students who just met the eligibility cutoff with students who just missed the eligibility cutoff. Since the ACT score with which the students qualify is not precisely manipulable these students should be randomly allocated around the cutoff.

The second specification uses graduates from all 50 states to identify the effects of interest with a difference in difference approach. This approach compares the difference between

[9]As calculated by the state of residence of their parents in their final year of school.

students just above and below the cutoff in their merit-aid program state with the difference across the same ACT score in states without merit aid cutoffs at that score. This specification has two advantages. First, it allows me to improve precision of the results by using the full sample of students available in the dataset to identify the effects of interest. Second, it allows me to compare students at the same scores in states that have merit aid programs with different cutoffs.

4.1 Regression discontinuity: merit program states only

The source of variation for the regression discontinuity approach is the sharp change in status of qualifying for the program across the cutoff ACT scores within each state. The intuition here is that because students cannot precisely manipulate their ACT score, those just above and below the cutoff score are randomly distributed. Thus, the approach compares students with similar ACT scores who differ only in their merit aid program eligibility status.

For this analysis I restrict the sample of individuals to students whose home state[10] is in a merit program state. To select the bandwidth for the analysis, I consider the bandwidth selection criteria presented in Lee and Lemieux (2010). Their approaches suggest a bandwidth of four to five, however the point estimates are remarkably consistent to using any bandwidth between one and ten points[11], so ten is used to maximize the power of the estimations.

I estimate a flexibly specified pooled regression with ACT score centered around the cutoff in each state. For each individual i with ACT score a in home state s, the specification is

$$\begin{aligned} y_{ias} = \alpha_0 + \alpha_1 D_{ias} + \alpha_2 D_{ias}(ACT_{ias} - c_s) \\ + \alpha_3(c_s - ACT_{ias}) + \alpha_4 X_{ias} + \alpha_5 W_{ias} + \varepsilon_{ias} \end{aligned} \tag{1}$$

where D_{ias} is an indicator for whether the student qualified for the program in her state based on her ACT or SAT score and ACT_{ias} is her ACT score. In the specification, ACT score is centered by the cutoff score in the students' home state c_s. The linear functions of centered ACT score are defined such that the slope of the effect may vary based on whether the student

[10]Home state is based on the state of residence of the student's parents as reported on the FAFSA financial aid form filed in 2006, prior to the 2007/08 academic year.

[11]Figures demonstrating the consistency of the point estimates across bandwidth choices are available upon request from the author.

falls above or below the test score cutoff. X_{ias} is a vector of exogenous baseline characteristics, while W_{ias} is a vector of potentially endogenous college performance measures that are included in some specifications. In this specification, α_1 is the program evaluation effect of merit aid programs on the outcomes of interest.

The assumption required for this approach to be valid is that the running variable (ACT score) is not *precisely* manipulable around the cutoff point. As evidence that this assumption is indeed true, I present the histogram of centered ACT scores in figure 4. While there does seem to be some heaping in the distribution, it is still smooth over the cutoff point. This suggests that while students are attempting to influence the outcome of their ACT exam in reaction to the programs either by exerting more effort on the exam or by retaking the test, they are still not able to precisely achieve the score exactly at the cutoff. Thus, the allocation of students just above and just below the cutoff should still be random.

For additional support of the validity of the assumption of random assignment over the cutoff score, I conducted an RD analysis on baseline characteristics, shown in table 3. There is minimal evidence that there are discontinuities in baseline characteristics across the cutoff score. To the extent that sorting might be related to observable characteristics, this supports the assumption that students are as good as randomly sorted across the cutoff score.

It is worth noting that direct manipulation of the running variable is not the only dimension along which selection may occur. Because my sample consists entirely of college graduates, students who receive the scholarship are also more likely to graduate than those who do not receive the scholarship, a discontinuity in the histogram could reflect students just above the cutoff being more likely to be observed. Neither this nor the concern of students directly manipulating ACT score seems to be binding, since the distribution of centered ACT score is smooth over the cutoff point.

4.2 Difference-in-difference approach: all states included

In order to make use of the full sample of data, rather than just the restricted sample in the regression discontinuity specification, I also employ a difference in difference approach. This exploits the variation of having a merit aid program across states in place or not in addition to the discontinuities induced by students falling above or below the score eligibility cutoffs.

The intuition here is to compare students within ACT score across states in which they are and are not eligible for merit programs. This goes beyond comparing students in states with and without programs, since there is substantial variation in cutoff scores across states with merit aid programs. Because of this spread, students with a qualifying score in one state may fall below the cutoff score in another state, forming part of the control group for the purposes of calculating the treatment effect. As a result I can be confident that I am in fact identifying the treatment effect separately from the effect of particular ACT scores on outcomes after graduation on the effect of simply living in a state with a merit aid program, especially since I include score fixed effects in all difference in difference specifications.

The specification used for this portion of the analysis for an individual student i with ACT score a in home state s is

$$y_{ias} = \alpha_0 + \alpha_1 D_{ias} + \gamma_a + \gamma_s + \alpha_3 X_{ias} + \alpha_4 W_{ias} + \varepsilon_{ias} \tag{2}$$

where D_{ias} is an indicator for whether students qualify for the merit aid program in their home state based on their test scores, γ_a represents ACT score fixed effects, γ_s represents state fixed effects, X_{ias} is a vector of exogenous baseline controls[12] and W_{ias} is a vector of endogenous controls representing experiences of the student while in college that might affect outcomes after college[13]. In this specification, α_1 is the treatment effect of qualifying for a merit aid program on outcomes after graduation.

The critical identifying assumption is that absent the programs, the marginal effect of obtaining a particular score on the ACT on later life outcomes would be the same in states with and without the programs. This "parallel trends" assumption can be supported by examining plots of the averages of various outcomes by state, treatment status and ACT score. As an example, the average of state merit aid by ACT score in states with and without merit aid programs is shown in figure 5. This figure shows that students who do not qualify for merit aid in states with merit aid programs look remarkably similar to students who live in states without merit aid programs, while students who do qualify and live in merit program states

[12]Baseline controls include gender, race, family income, parental marital status, parental education, parental nativity, number of siblings in college and whether the student attended a private or public high school.

[13]This vector includes GPA, college major, whether the student graduated with honors, and measures of institutional quality.

have substantially higher merit aid in their final year of school than either of the other groups.

4.3 Merit aid program evaluation

The following sections present the results of the baseline merit aid program evaluation specifications. Each table is organized with the coefficient from one specification represented in each cell of the table. Results from both regression discontinuity and difference in difference specifications are presented side-by-side, as well as specifications including no controls, exogenous baseline controls as well as adding potentially endogenous college outcome controls.

4.3.1 Effect of program eligibility on college experience

I first present the results at graduation. Does qualifying for a merit aid program, based on ACT score, significantly affect how students finance their college education, or change their academic outcomes while in school? It does, as can be seen in figure 6 and more generally in table 4. Panel A shows changes in cumulative funding outcomes over the full undergraduate career. Students who qualify for merit aid programs have $5900-$7600 less debt than their peers, which is robust to controlling for baseline characteristics, and to both the regression discontinuity and difference in difference specifications. The regression discontinuity results generate the largest gaps between qualified and unqualified students, suggesting that the differences are indeed resulting from qualifying for the programs directly. While the point estimates differ from one another somewhat, they are statistically indistinguishable from each other.

Students are only somewhat less likely to have any loans, however showing that the changes in loans are driven primarily by intensive margin changes in loans adopted to pay for school, rather than extensive margin changes in taking on loans or not. Qualifying for merit aid does somewhat offset the total amount of federal Pell grants students received by about $900 to $1300. The magnitude of this change is much smaller than the change in the amount of loans offset by qualifying for state merit grants. There is also no evidence that qualifying for a merit aid program changes students' work income in the final year of school. While these results do mask some heterogeneity in the results, this shows that the primary effect of state merit aid is to offset loans students are taking on to graduate, rather than to dramatically change other elements of the funding package.

Unfortunately, whether students are receiving state merit only grants and in what magnitudes are only available for the final year of the undergraduate degree. Students who qualify are both more likely to receive state merit only aid, and to receive a larger amount of state merit only aid than their peers in their final year of their undergraduate degree. The magnitude of these amounts, particularly the "treated on the treated" measures are consistent with the size of merit aid grants. Because the cumulative measures are unavailable, loans represent a more accurate summary representation of the funding patterns over all years of attendance.

It is possible that introducing merit aid programs would change the quality of the school students attend, or the qualify of the outcomes achieved. These possibilities are examined in table 5. Students are induced by the scholarship to attend different schools. Students who qualify for the scholarship programs are more likely to graduate from public schools in their home states as we would expect from the fact that most scholarships have the most value (or are only redeemable) at public in-state schools. These schools may be cheaper than the alternatives (either private or out of state schools) resulting in a less expensive degree overall relative to students who chose to attend schools without the additional price incentive of the merit aid scholarships. Thus, even if the scholarship did not directly offset loans, the drop in cumulative loans for students who were induced to change which school they attended could be partially a result of the changing cost of the degree. Because treatment is based on who *qualified* for the program, rather than who *redeemed* the scholarship, there may also be a bargaining effect: that students who did not redeem the scholarship were able to leverage the option of the award at other schools for a more favorable financial aid package.

Following Cohodes and Goodman (2014), there is a concern that students were induced to attend different quality schools as a result of the scholarship. The results in table **??** show that there are no discernible differences in the selectivity of the schools that students attend when they qualify for a merit aid program. Even if there is no difference in the schools students attend, it is possible that qualifying for the scholarship will change their academic performance while in school, which could impact their labor market outcomes after graduation. I find that other than being more likely to graduate from a public in-state school, the college experience is relatively unaffected by eligibility for merit aid. There are no significant treatment effectson undergraduate GPA or on the probability of a student graduating with honors. I have

also examined the effects of merit aid programs on college major choice. While I find some evidence that students change their major in response to the programs, though the results are inconsistent across specifications. Summarizing, there is evidence from the RD specifications that students are less likely to major in STEM fields and more likely to major in humanities fields (the largest category of undergraduate major). The DD results are broadly consistent with an increase in the probability of majoring in a humanities field, but show close to a zero effect on the probability of majoring in a STEM field. This may imply that the effect acts primarily close to the score margin, while students farther away from the cutoff point are not changing their marginal decision to major in one field or another.

Demonstrating that there are limited effects on outcomes at graduation beyond the changes in funding variables should allay some concerns that the effects on outcomes after graduation are driven by intermediate outcomes. To further address these concerns I report specifications including these endogenous controls[14] directly to demonstrate that the results are robust to their inclusion. Regardless, changes to choices while in school will only pose a problem for the use of merit aid eligibility as an instrument for student loans. In the program evaluation sense, these changes are a component part of the bundle of effects qualifying for merit aid will have on outcomes after graduation.

4.3.2 Effects one year post graduation

While the dire predictions of correlational studies of loans would suggest that students with lower loans (because they qualified for merit aid scholarship) would have higher income, I find exactly the opposite result: students who benefitted from merit aid programs have lower income than their peers by $2000-$5000 in the baseline specifications. This result, presented in table 6, is robust to including exogenous baseline controls, to including endogenous controls for outcomes while in college and to either the RD or DD specifications. This is consistent with the result found by Rothstein and Rouse (2011), that students with fewer loans are more likely to enter lower-paying careers, such as those in teaching or public service. My results support their conclusion but are also stronger, since they are robust to the inclusion of occupation fixed effects. The magnitude of the point estimates is reduced by including

[14]Endogenous controls included: undergraduate GPA, primary undergraduate major, institution selectivity, public in-state school indicator, months to graduation, honors graduate

occupation controls, suggesting that there is a large role that occupational sorting might play, but students without loans are still making less money one year after graduation even within occupational group.

Although these are few observable differences in employment and time use between students who did and did not qualify for merit aid programs in the first two rows of table 6, we might imagine that the results are driven by the students who are unemployed or by students who are enrolled in further education while employed. This is not the case. Moving down the table, I restrict to students who are employed and not enrolled, or employed full time[15] while not enrolled. The result that students who benefitted from the programs are making less money is robust to all of these sample restrictions, and to the inclusion of controls for undergraduate experience and institutional quality. In fact, excluding students who are not enrolled results in a larger magnitude coefficient, suggesting that students who *are* enrolled and employed may behave more like students with greater loans in terms of job selection even if they qualified for merit scholarships. One possible explanation for this could be that students who are enrolled and employed may be taking on debt for further schooling, and thus are starting to behave more like their peers who took on greater undergraduate debt.

There is some evidence that these results are at least somewhat driven by occupational choice, though the results are not consistent across specifications. These results demonstrate that students who benefit from merit aid programs are indeed somewhat more likely to sort into the "fun" occupations of the entertainment industry[16] and less likely to sort into the more practical healthcare related occupations. Results on other sectors are mixed, though there is suggestive evidence that students are less likely to find occupations in the STEM fields (science, technology, engineering and math), and are slightly more likely to sort into "office" occupations, a catch-all category that includes legal professions. While suggestive, these results are consistent with students who qualified for merit aid programs sorting into more fun and less difficult occupations.

[15]Full time is defined as 35 hours or more worked in a typical week.
[16]This industry includes art, design, sports, communications and music occupations

4.3.3 Effects four years post graduation

The importance of the results found hinge on how lasting they are, and whether the differences found persist four years after graduation, we see that students who benefitted from the program are slightly more likely to have a degree or be enrolled in a higher degree program. These results are presented in table 7. Although these results are insignificant, they are of fairly consistent magnitude and are robust to the inclusion of several controls. We also see that the effect on income has largely disappeared. The point estimates are quite small, have inconsistent signs and are highly insignificant. This could imply either that students who benefitted from the programs have converged to the same income path as those who did not benefit from programs, or it may imply that they were flexible enough to chose jobs with higher income growth than their peers without loans. Without a longer panel of data it is impossible to tell the difference between these two outcomes. Another possible confounding factor is that Income Based Repayment (IBR), a payment plan that allows students to tie their loan payments to a fraction of their monthly income, was dramatically expanded by presidential order in the summer of 2009 and became more widespread over the subsequent few years. This may have an effect of its own, causing students with loans to act more like their peers without loans by 2012, four years after graduation.

While differences in income have disappeared by four years after graduation, large differences in occupation have emerged, as is demonstrated in panel B of table 7. Interestingly, they are somewhat different from the occupational differences observed one year after students graduated. Students who qualified for merit aid programs are far less likely to have an occupation in business than their peers, and are more likely to be employed in education. Results on any of the other occupational categories are either insignificant or inconsistent across specifications. This is particularly interesting since students who qualified for merit aid programs were in fact slightly less likely to major in education, and were no more or less likely to major in business. To the extent that business occupations tend to be more lucrative over the life cycle than education careers, this could also imply that the students who did qualify for merit aid have in fact sorted into careers that will pay less over their lifetime, but presumably will have higher expected non-pecuniary benefits in the long run.

5 Student Loans: Evidence from Merit Aid

While the effect of merit aid programs after graduation is independently interesting, this project is motivated by an interest in how students are affected by their loans as they leave school and enter the labor market. A naive way to address the link between loans and outcomes after graduation would be to run a regression of the form

$$y_i = \beta_0 + \beta_1 Loans_i + \beta_2 X_i + \varepsilon_i \tag{3}$$

where X_i is a vector of baseline control characteristics, y_i is the outcome of interest and $Loans_i$ is a measure of the total amount of loans a student has upon achieving a bachelor's degree for a student i. The issue with this specification is that there are many potential omitted variables included in the error term. For instance, if college funding is negatively correlated with parental wealth and networks that are unobservable to the researcher while outcomes after college are positively correlated with those things, then an estimate of β_1 would be biased downwards. The estimation would falsely attribute the lower income of graduates to their loans rather than to their family background characteristics.

5.1 Causal identification of the effect of student loans

To address this selection issue and identify the causal effect of loans on outcomes after college, I use merit aid program qualification as an instrument for the cumulative amount of undergraduate loans students have upon graduation. Because we have shown that merit aid programs have very few impacts on outcomes while in college other than the composition of college funding, eligibility for these programs can plausibly be used as an instrument for loans.

As in the previous section, I use the variation in merit aid program eligibility in two ways: first by using only program states to employ a regression discontinuity design over the test score cutoffs, then by using all states in the US to employ a difference in difference approach.

The second stage estimating equation for these specifications will closely resemble the specification in equation 3, though with the addition of an instrument for loans in the first stage, state fixed effects, controls for ACT score, and both exogenous baseline characteristic controls and potentially endogenous controls for measures of college performance (based on

the specification used). Thus the relevant second stage estimating equation will be

$$y_{ias} = \beta_0 + \beta_1 \widehat{Loans}_{ias} + \mathcal{F}(ACT_{ias}, c_s) + \gamma_s + \beta_2 X_{ias} + \beta_3 W_{ias} + \varepsilon_{ias} \tag{4}$$

where subscript i refers to the individual student, a to her ACT score, and s to her home state. The variables used in this specification are $\widehat{Loans}_{ias}$, the predicted value of cumulative undergraduate loans from the first stage, $\mathcal{F}(ACT_{ias}, c_s)$, a flexible function controlling for ACT score and program eligibility based on the ACT score and the state-specific score cutoff, γ_s, state fixed effects, X_{ias}, a vector of exogenous baseline characteristics and W_{ias}, a vector of potentially endogenous college performance measures. y_{ias} represents the outcomes of interest which include income one and four years after graduation, occupation of employment and post-baccalaureate enrollment. The first stage estimating equations will follow the specifications in the program evaluation of merit aid programs, using the cumulative amount of loans as the endogenous variable of interest.

There are two assumptions required for this instrumental variables approach to be valid. First, the instrument must predict a decrease in loan amount: changes in the student's status of qualifying for the merit aid program must induce changes in their cumulative amount of undergraduate loans. There is clearly a strong first stage, as can be seen in table 4 and more directly in figure 6. Students who qualify for the merit aid programs have substantially fewer loans than their peers.

The second requirement for the IV approach to be valid is that the exclusion restriction must hold: conditional on covariates, whether the student qualified for merit aid in his or her state is unrelated to the outcomes under examination except through the effect on loans. Even if this requirement does not hold, the program evaluation analysis in the previous section remains valid. This assumption is only required to study the link between student loans and outcomes after college. This assumption is in fact plausible as it was shown in the previous section that merit programs have no observable effect on the academic outcomes of students in the sample. While the programs do shift students towards public universities in their home states, this is exactly the effect that we would expect to see, since students are unable to redeem the scholarship unless they attend public in-state schools (for most programs). There is also no demonstrable difference in the quality of the school that students attend in

reaction to the program, even if they shift the sector of school they attend. Finally, to further address this concern, I will include the potentially endogenous characteristics of students' college experiences in the results that follow to demonstrate that they have little effect on the estimates of interest.

5.2 Effect of student loans on outcomes after graduation

As in the previous section, each table here presents the instrumental variables regressions of loans on outcomes after college, with merit aid program eligibility serving as the instrument for cumulative undergraduate loans. Both regression discontinuity and difference in difference specifications are presented, as well as variations without any controls, with exogenous baseline controls and with potentially endogenous college outcome controls. Each cell in each table represents the coefficient on cumulative undergraduate loans (represented in thousands) from the second stage regression.

5.2.1 Effects one year after graduation

Turning attention to the IV results in table 8, we see that students who have higher loans are in fact making more money, completely contradicting the concerns in the traditional policy discussion outlined in figures 1. The point estimates suggest that for each additional $1000 in loans students have when they complete an undergraduate degree, they are making an additional $600 - $800 in the first year after graduation. Depending on the interpretation of the imprecision of these results, at worst students with loans are finding jobs that are comparable to their peers without loans, and at best students are seeking out jobs that pay sufficiently more in their first few years out of school that they might completely offset the value of those loans in 2-3 years.

The upside of these results is that students with higher loans are making more money immediately after graduation, which may allow them to offset their loan burden relatively quickly. There are two potential concerns with these results, however. First, individuals with loans may be choosing occupations that have higher initial income but lower income growth over the life cycle, while their peers without loans have greater freedom to choose occupations that have low initial returns, but a steeper growth path over subsequent years.

Second, basic economic theory would imply that individuals with loans are trading off money with non-pecuniary benefits, since students who are less constrained by loans are making different choices that lead to lower income. Whether the fact that students with loans may be less happy in their occupations is of interest to policy makers is an open question, but the fact remains that students with loans must be sacrificing something in the balance.

5.2.2 Effects four years post graduation

The IV results in table 9 are again more imprecise than the reduced form results, but they suggest a pattern that students with loans being less flexible than their peers without loans. Students with higher loans are less likely to have a degree or be enrolled by 2012, even though these students were roughly similar above and below the cutoff points at college graduation.

Even if students are choosing income at jobs commensurate with their peers without loans after graduation as a result of the introduction of income based repayment, the differences in occupation selection four years after graduation are persistent and reveal interesting patterns, which are reported in panel B of table 9. Students with higher amounts of loans are far more likely to have an occupation in business four years after graduation. Particularly, for each additional $1000 of loans, students are almost 2 percentage points more likely to be employed in business four years after graduation. Students who have higher loans from their undergraduate degree are also less likely to be working in education four years after graduation, on the order of 1 percentage point per $1000 of loans. These results suggest that while the income disparity between students with and without loans has largely disappeared four years after graduation, a more dramatic occupational disparity has emerged, which could have significant implications for the lifetime income path of students beyond the survey period.

6 Heterogeneity analysis: Exploring mechanisms

The same forces may act with different strength on heterogeneous parts of the student population, so by examining the heterogeneity of the results to both ability and family income I can address mechanisms underlying the main results presented. In particular I examine the heterogeneity of the reduced form results with respect to student individual ability and family income. While the same constraints may be acting on the full distribution, underlying dif-

ferences related to the heterogeneity may generate different magnitudes of effects along the distribution. Students at the higher end of the ability distribution likely have more choice of possible employment, while students from higher income families are likely less financially constrained as they enter the labor force. If credit constraints drive the observed results, we would expect to see the effects be stronger at higher ability levels, and stronger at lower family income levels, since those populations would be most reactive to credit constraints, the former because of choices and the latter because of necessity.

The large amount of variation between program cutoff scores across states allows me to examine the differences in treatment effect by individual ability, since students qualify for the merit programs in their home states at different points in the national ACT score distribution. The marginal student in one state, for instance Mississippi, scores a 15 while the marginal student in another state, for instance Missouri, has a score of 30. These are likely students with very different underlying ability. Heterogeneity in the underlying sample and high quality information derived from the Free Application for Student Aid (FAFSA) filed in the year prior to graduation allows me to examine the heterogeneity of treatment effects by family income, reported to the schools for the purpose of calculating need-based financial aid eligibility. I find that while the main results are broadly consistent across heterogeneous groups, differences in magnitudes suggest that the main results are produced by credit and/or liquidity constraints binding when individuals leave school and enter the labor force.

6.1 Individual ability

I argue that credit constraints upon graduation drive the main results presented above, but one possible competing explanation for the main result could be that students choose to attend college based on a calculation of their individual unobserved potential income post-graduation, relative to the cost of attendance. If the cost of attendance exceeds some function of their potential income, the student would choose not to attend college. In this case, only high quality students would sort into college attendance. Introducing a scholarship program lowers the cost of attendance for all students that meet the eligibility criteria, even those who previously would have not sorted into a college education. Lowering the average unobserved quality of students among the treatment group would result in a lower average income among the treated students.

If this were the case, we would expect the effects to be stronger at lower ability levels, since marginal students at lower scores are more likely to have not planned on attending college absent the scholarship. Contrary to this expectation, I find that the effects are remarkably similar across the ability distribution and in fact potentially stronger at the higher end of the ability distribution.

Because different states have different ACT cutoff scores, I observe the treatment effect at many different possible ACT scores, reflecting a broad cross-section of the underlying ability/quality distribution. Looking at figure 7, which plots the average salary against ACT score by treatment status, we can see that there may be significant differences in the treatment effect by ability, as proxied by a student's high school score on the ACT. The gap between those who are treated in merit program states and those who live in non-program states is in fact widest towards the high end of the ACT score distribution. For reference, the middle 50% of students at top public institutions[17] fall between roughly 25 and 31 on the ACT composite score scale. This population would likely have chosen to attend college regardless of the subsidy. Furthermore, we might imagine that all else equal, higher ability students will have more employment options after graduation. As a result, high ability students should show a greater treatment effect, since those who do have loans may have a greater set of employment opportunities from which to choose. This supports my conclusion that students with lower loans in fact have lower income because they are more flexible in the job market after graduation, and are more able to take jobs that they want, rather than jobs that they must take in order to generate sufficient income to pay off their loans.

In order to estimate these effects directly, I replace the treatment indicator with three indicators in the main estimating function: interactions between whether the individual qualified for the merit aid program in a state with a "low" score cutoff (between 15 and 20), a "medium" score cutoff (between 21 and 27) or a "high" score cutoff (between 28 and 30)[18]. For ease of comparison, I report the results from a regression discontinuity regression and a difference in difference regression, both with baseline characteristic controls included, side by side in tables 10 and 11 for selected outcome variables.

[17] e.g. UNC-Chapel Hill, UVA, UC-Berkely, UM-Ann Arbor

[18] States with low score cutoffs are Mississippi, Kentucky, Tennessee, Arkansas, Florida, Louisiana and Idaho, states with medium score cutoffs are South Dakota, Michigan and West Virginia and states with high score cutoffs are New Jersey and Missouri

6.1.1 Effects by ability at graduation

Looking at table 10, there are some differences in the effects on students' offset loan burden by ability. Although the point estimates are not statistically different from one another, students qualifying at higher scores seem to have larger drops in loans as a result of qualifying for merit aid programs. They are also more likely to graduate with no undergraduate loans, while students who qualify at lower test scores alter their loan burdens entirely on intensive margins. Interestingly, students who qualify at lower test scores have more of their Pell grants offset as a result of qualifying for merit aid programs, though the magnitude of this effect is still smaller than the total amount by which loans are reduced by qualifying for the merit aid programs.

The effects on academic outcomes at graduation are also quite heterogeneous by ability levels. Students who qualify at low ACT scores are most induced to graduate from a public, in-state school. Their academic outcomes are subsequently unchanged, while students who qualify at high ACT scores have somewhat worse academic outcomes at graduation than their peers who just missed qualifying for the programs. While there is no evidence of differential school quality when students qualify for merit aid, this could reflect a potential negative impact of merit aid programs on the affected students. It is possible that this drop in academic quality could be related to the drop in income, however if this were the case we would expect the specifications including endogenous controls to wipe out the treatment effects in the main specifications.

6.1.2 Effects by ability after graduation

While insignificant, the results in table 11 are suggestive of there being a stronger treatment response one year after graduation for students who qualify at higher ability levels. Particularly looking at the results for annualized salary and the variations thereof, the largest treatment effects are demonstrated by the students who qualify for the programs at the highest ability levels. This pattern of results is inconsistent with the explanation presented that students who would not otherwise have gone to college driving the negative results, since we imagine students scoring above a 28 on the ACT, at the 90th percentile of the national test score distribution, would have indeed attended college regardless of receipt of the scholarship.

The results four years after graduation, presented in table 12, show similar patterns in the

treatment effects. Although there is no effect on graduate school attendance in the aggregate, students who qualified for the merit aid programs at medium or high scores are indeed far more likely to have a degree or actively be enrolled in education beyond a bachelor's degree than those who just missed qualifying for the merit aid programs. This suggests that states setting high score cutoffs for their merit aid programs may be indirectly subsidizing post-baccalaureate education, rather than undergraduate degrees directly.

While there is still no significant difference in annual salary between students who did and did not qualify for those programs, the magnitude are suggestive of there being a persistent income gap among students who qualified at the highest ACT scores. The results on occupation choice four years after graduation, presented in panel B of table 12 demonstrate that students who qualified for merit aid programs at high cutoff scores are the least influenced by the scholarships to change their occupational track. While they are less likely to have a job in business, they are not more likely to be employed in education or STEM fields. Students who qualify at medium or low scores are also less likely to be employed in business, but it is these individuals who are more likely to be employed in education fields. Individuals qualifying at low scores are even slightly more likely to be employed in STEM fields. These results are suggestive of students qualifying at higher ability levels having different reactions to the constraints than their peers who qualify at lower scores. They paint a picture of students who qualify at higher levels having primarily financial constraints relieved, as they are more likely to attend graduate school, and potentially still have lower annual income four years after graduation. Students who qualify at lower scores, however, seem to have constraints of access relieved, as they sort into different sorts of occupations more frequently than their higher scoring peers. This may still be a financial constraint, but operates in a different way. For instance, perhaps having the scholarship gives a somewhat struggling student the flexibility to retake classes and ultimately sort into their preferred career that otherwise would have been unattainable.

6.2 Family Income

The broad results reported in this paper thus far are remarkably similar to those reported in Rothstein and Rouse (2011), particularly considering that their sample is exclusively drawn

from a highly selective, elite private school. In comparing to their paper and other research, in assessing the value of merit aid programs more generally and in determining the possible mechanisms at work generating the main results, it is valuable to assess the extent to which the effects vary by family income bracket. In order to examine these differences, I have split the sample into four equal quartiles by family income in 2006, and reported the results of the main analyses interacted with membership in each of the income quartiles. Each coefficient in this specification is the direct estimate of the treatment effect at each income quartile.

6.2.1 Effects by family income at graduation

The results on funding are reported in table 13. The effect of merit aid eligibility on having any merit aid in the final year, the amount of merit aid received in the final year, and on the cumulative amount of student loans students accumulate is statistically indistinguishable across the four income quartiles. This is particularly promising because it demonstrates that merit based programs do seem to be independent of family background, supporting the validity of my identification approach.

There are some differences expressed for students from the lowest family income quartile. Loan accumulation is somewhat decoupled from merit aid eligibility, though this likely occurs because students from the lowest income quartile have greater access to need-based aid in the form of grants and work study, somewhat decoupling their loan accumulation from their merit aid eligibility. Further evidence in this direction can be seen in columns 5 and 6 of panel A of this table, which show the effect of qualifying for merit aid on the cumulative amount of federal Pell grants each student received. Students from the middle 50% of the distribution have their Pell grant dollars offset somewhat by merit aid eligibility and students at the top end show little effect, presumably because they are very unlikely to meet the requirements for Pell grant eligibility. However, students in the bottom quartile of family income are in fact more likely to receive federal Pell grants if they qualify for merit aid scholarships. Students from the lowest family income quartiles are also the only students who show any evidence of merit aid eligibility reducing the amount of income they gain from working while in school. Together these results demonstrate that merit aid programs are impacting the way students from the lowest family income brackets fund their college degree differently than students from

families with higher income.

Students from lower income families are also differentially affected by merit aid eligibility on outcomes realized at graduation, shown in table 14. Students from the top half of the income distribution are more likely be induced by the merit aid programs to graduate from a public school in their home state. This likely occurs because students from the lower half of the family income distribution are already largely attending public, home state institutions, while students from the upper half of the income distribution might be sufficiently enticed by the price change implied by the introduction of the merit aid program to change their choice of where to attend college. This is consistent with the results found in Dynarski (2000), that the Georgia HOPE program largely shifted the decisions of middle and upper class students.

6.2.2 Effects by family income after graduation

The goal of looking at differences by family income particularly is to assess whether credit constraints might explain the results observed. Turning attention to the results on outcomes one year after graduation with respect to family income in table 15, we see the exact pattern that we would expect: that the effect of merit aid eligibility diminishes moving up the family income distribution, particularly in the regression discontinuity results. This is consistent with a credit or liquidity constraint driving the results observed, since students from families with relatively high income would likely have access to family resources to smooth temporary constraints. This pattern is not entirely replicated in the difference in difference results, however, indicating that students from high income families away from the score discontinuity may in fact be behaving differently from those at the score discontinuity.

The results four years after graduation show similar patterns, though the strength of the pattern is somewhat diminished. This may suggest that students are establishing themselves well enough that their outcomes are becoming less tightly associated with family background. The point estimates of students who have completed or are pursuing a post-baccalaureate degree are remarkably consistent across family income quartiles, though it is only significant for students from the lowest quartile. The results on income four years after graduation are quite inconsistent in that the regression discontinuity and difference in difference specifications disagree on the sign of the treatment effects. There is however a clear ordinal pattern that

students from higher income families show a more positive result of qualifying for the scholarship: either a smaller decrease or even in fact an increase over the income of those who missed qualifying than their peers from lower income families. This may suggest that students form the highest income families were able to capitalize more fully on the opportunities afforded by having fewer loans than their peers who came from lower income families. It may also suggest an additional outcome: that absent the credit constraints imposed by lower family income, demoing the constraint of student loans may in fact accelerate income growth.

The result that students who qualify for merit aid scholarships are less likely to be employed in business four years after graduation is surprisingly consistent across family income levels, though it is the weakest for students from the wealthiest families. The result that students are more likely to be employed in education is primarily concentrated on the middle 50% of the family income distribution. This may suggest that students from the middle of the distribution are less constrained both financially and with respect to family expectations in the short and long run: they are able to take the job they want without the weight of family expectation constraining their choices.

7 Policy Implications and Future Research

In summary, the results of this analysis demonstrate that the way in which students fund their college education has a significant impact on their short-run labor market outcomes. Graduates with large amounts of loans seek out jobs that have greater financial compensation, whereas graduates with a higher amount of scholarships seek out jobs that have lower financial compensation but (presumably) greater non-pecuniary benefits. Taken together, this provides evidence that loans are not financially disadvantaging students who have recently graduated from college. In fact, students with loans are pursuing occupations and jobs within occupations that provide higher compensation than their peers without loans. These results are largely consistent with those reported in Rothstein and Rouse (2011), which is remarkable considering their study focused on students from a highly selective elite private institution, which the results here are derived from a nationally representative cross-section of students.

There is clear evidence, however, that students who have loans are less likely to have careers in education after four years. Taking education as a proxy for public service careers in general,

it may be the case that relying heavily on student loans for college financing could be distorting students away from starting careers in public service. Current policy seems to be taking this into account in the form of the Public Service Loan Forgiveness program, which gives students the option to discharge their debt after working for ten years in designated public service occupations. This should reduce the total cost of federal loans, though graduates still must make payments on their loans for ten years after graduation. The results of this paper suggest that the spirit of this program is correct, that students with loans need greater incentives to be induced to enter public service careers, but this program may take too long a view. Students with greater amounts of loans are changing their behavior immediately after they graduate from school, so this sort of intervention may be too little, too late. Furthermore, the results in Field (2009) would suggest that even if students are aware of the option to have their debt forgiven, they might take that option less often than rationality would predict. Her behavioral results suggest that having loans while in school changes the way students view their job opportunities after college. While her study considers a highly selected group of law students, my results suggest that similar effects may be at work in this nationally representative sample. Students with loans are clearly opting into occupations that provide higher compensation, even though they might have other options available to them to reduce their loans.

There are limitations of this paper that must be noted, chief among them that the sample considered here is one of graduates from four year degree programs. As the recent paper by Looney and Yannelis (2015) would suggest, the student loan "crisis" is largely being borne by students at for-profit institutions that offer degree and certificate programs much shorter than four years. This paper cannot speak to the experience of those students, nor can it speak to the experience of individuals who take on student loan debt and do not complete a degree. Future research should seek to examine the effects of student loans on those individuals who do not successfully complete a degree. Similarly, there are few studies of how merit aid programs might impact persistence once students are induced to enroll in school. While the data used in this project is not ideally suited to studying that question since it is a sample of students who have already graduated from school, the Beginning Postsecondary Study available from NCES could potentially be used to examine questions of that nature using many of the same techniques employed in this analysis.

More subtly, this paper considers an instrument for loans that precedes the in-school decisions of how much debt to accumulate, what major to choose and how much effort to expend in classes and other investments in human capital while in school. To circumvent this issue, the large expansion of income based repayment in 2009 could be exploited. The announcement of this expansion occurred as the cohort of graduates examined in this paper were leaving college, after all decisions about major and effort in college were made, but potentially before their initial labor market outcomes are set. This generates variation in loan repayment options (rather than in loans themselves) that could be used to examine in greater depth how students respond to credit and liquidity constraints with respect to their early labor market decisions.

Despite the limitations of this paper, the results do provide guiding evidence for policy makers evaluating the impacts of various college funding regimes on college students and college graduates. Loans do distort the behavior of recent college graduates, and seem to do so through short-term credit and liquidity constraints that affect the graduates' assessment of the value of pecuniary and non-pecuniary benefits of different jobs. These unanticipated consequences of college funding method highlighted in evaluations of existing programs should be taken into account as new programs and reforms to old programs are being considered.

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Figure 1: Loans regressed on salary of full time employed graduates

Figure 2: States with merit aid programs and their ACT score cutoffs

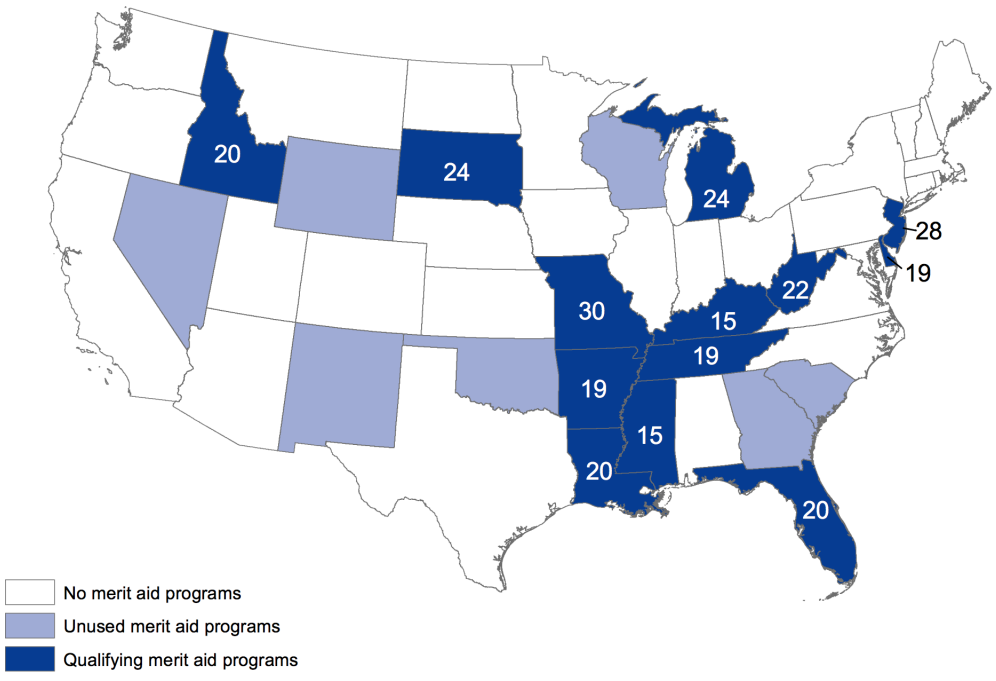

Figure 3: Program Award Size by Cutoff Score

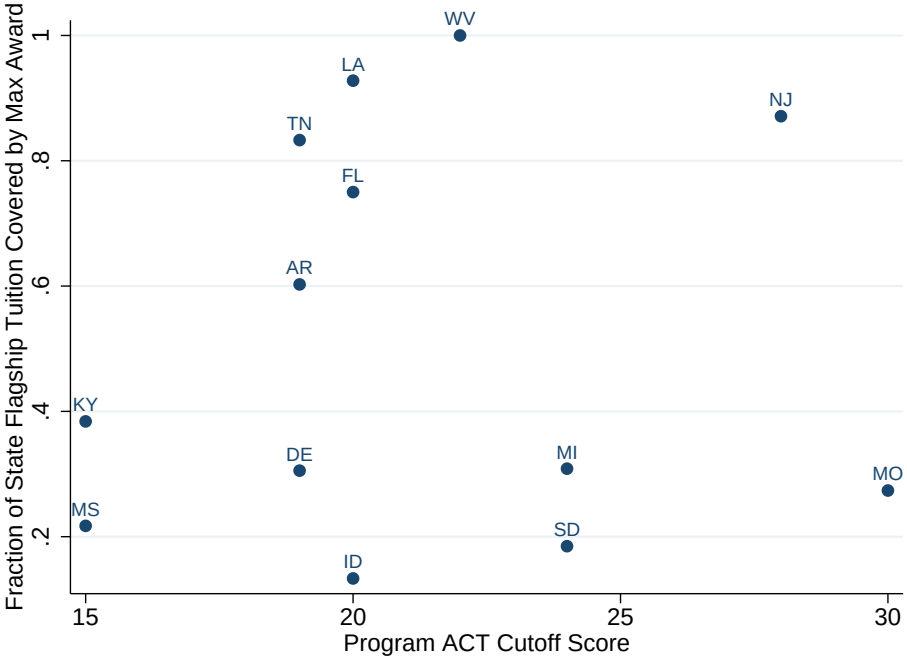

Figure 4: Centered ACT Score Histogram

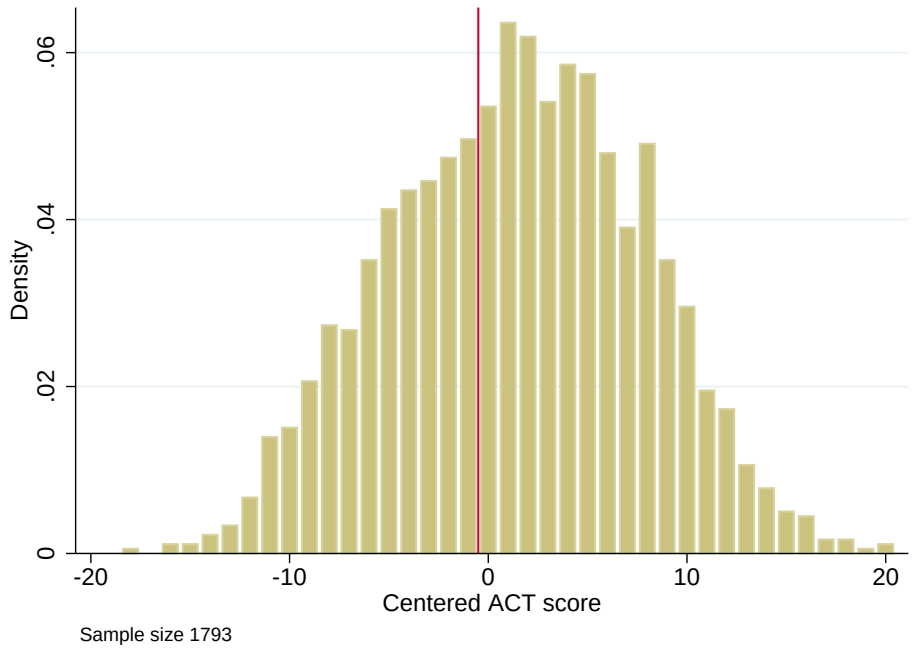

Figure 5: Amount of state merit aid by ACT Score (final year only)

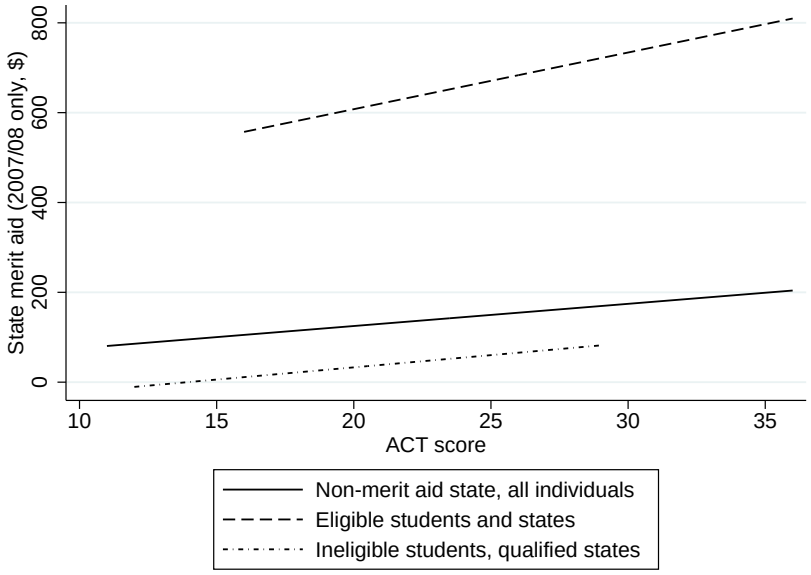

Figure 6: Total undergraduate loans by centered ACT score

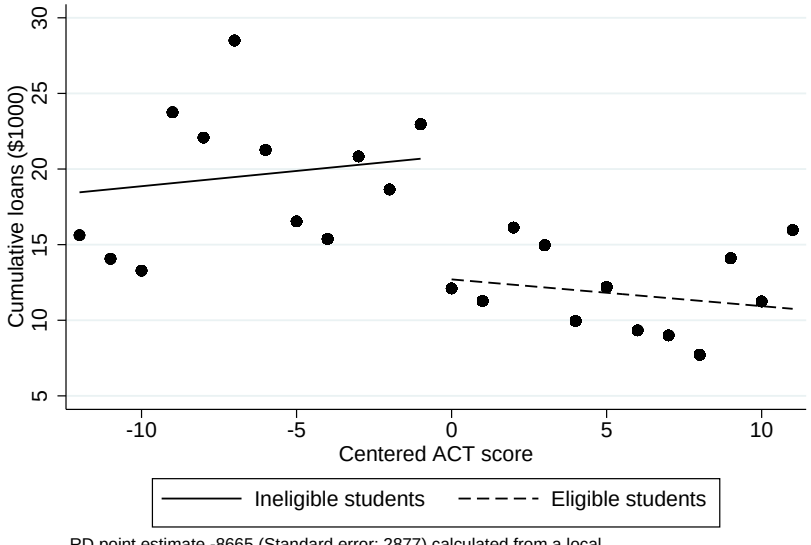

Figure 7: Salary by ACT score and treatment status

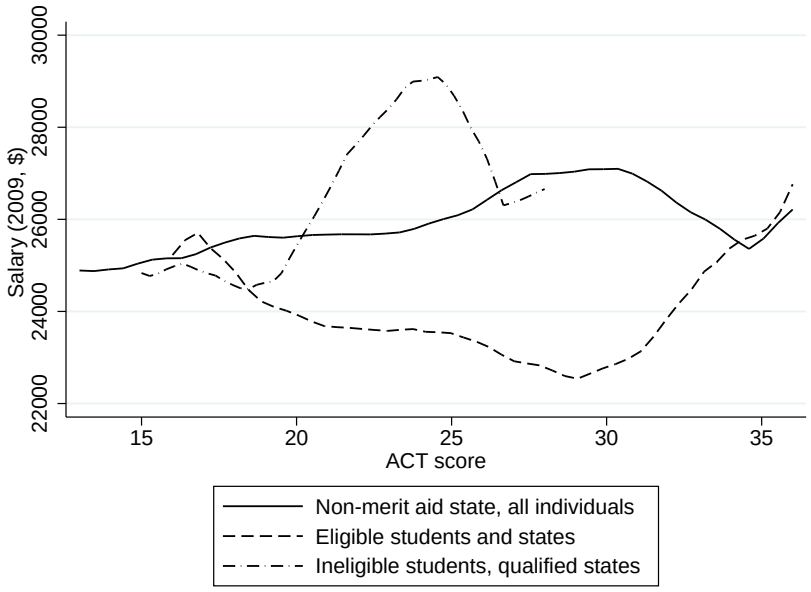

Table 1: Merit Aid Programs

Programs Used

Program start	State	Program name	ACT cutoff
1992	Arkansas	Academic Challenge Scholarship	19
2006	Delaware	Delaware SEED Program	19
1997	Florida	Florida Bright Futures Scholarship Program	20
2001	Idaho	Idaho Promise Category B Scholarship	20
1999	Kentucky	Kentucky Educational Excellence Scholarship	15
1998	Louisiana	Taylor Opportunity Program for Students	20
2000	Michigan	Michigan Merit Award	24
1996	Mississippi	Mississippi Resident Tuition Grant	15
1987	Missouri	Higher Education Academic Scholarship	30
1989	New Jersey	Edward J. Bloustein Distinguished Scholars	28
2004	South Dakota	South Dakota Opportunity Scholarship	24
2004	Tennessee	HOPE Scholarship	19
2002	West Virginia	PROMISE Scholarship	22

Notable Programs Not Used

Program start	State	Program name	Eligibility criterion
1993	Georgia	HOPE Scholarship	High school GPA
2000	Nevada	Governor Guinn Millennium Scholarship	High school GPA
1997	New Mexico	Legislative Lottery Scholarship	Residency only
1996	Oklahoma	Oklahoma's Promise - OHLAP	High school GPA
1998	South Carolina	LIFE Scholarship	HS GPA and class rank
1998	Wisconsin	Academic Excellence Scholarship	High school class rank

Table 2: Selected summary statistics

	States without programs	States with programs	
		Non-qualifying students	Qualifying students
Baseline characteristics			
Female	0.591	0.599	0.570
	(0.492)	(0.491)	(0.495)
White	0.815	0.820	0.820
	(0.389)	(0.385)	(0.385)
Family income (2006)	101442	87279	93351
	(68494)	(64157)	(60273)
Outcomes at graduation (2007/08)			
Cumulative undergraduate loans	14892	20529	11696
	(18909)	(22637)	(16418)
Any undergraduate loans	0.619	0.727	0.559
	(0.486)	(0.446)	(0.497)
Graduate of public, in-state school	0.538	0.575	0.655
	(0.499)	(0.495)	(0.476)
Cumulative undergraduate GPA	3.288	3.172	3.273
	(0.460)	(0.462)	(0.512)
Outcomes one year after graduation (2009)			
Employed full time (2009)	0.693	0.693	0.677
	(0.461)	(0.461)	(0.468)
Full time employed salary	36887	35317	35264
	(14819)	(12444)	(14418)
Outcomes four years after graduation (2012)			
Degree or enrolled	0.327	0.280	0.369
	(0.469)	(0.450)	(0.483)
Annual salary, full time employed	48225	45755	46128
	(23880)	(20864)	(23477)
Observations	7750	630	940

[1] Standard errors in parentheses.
[2] Means and standard errors in columns 2 and 3 calculated within ten points of the ACT cutoff scores in each state, the same bandwidth used in the regression discontinuity specifications.
[3] Observation counts rounded to the nearest 10 for data security purposes

Table 3: RD Analysis of baseline characteristics

	Own characteristics			Parents' characteristics	
	RD	DD		RD	DD
Female	-0.020	0.004	Family income	7534	-4922
	(0.082)	(0.054)	(2006)	(9240)	(5773)
White	-0.105*	-0.071*	Parents married	0.067	0.063**
	(0.055)	(0.039)		(0.049)	(0.032)
Private high school	0.012	-0.069*	At least one parent	0.084	0.044
	(0.058)	(0.036)	foreign-born	(0.053)	(0.039)
Number of siblings	-0.000	-0.028	Dad's education	0.046	-0.006
in college (2007-08)	(0.076)	(0.047)	BA or greater	(0.067)	(0.045)
English native	-0.033	-0.023	Mom's education	-0.005	-0.119**
language	(0.032)	(0.023)	BA or greater	(0.079)	(0.049)
Observations	1450	8970	Observations	1450	8970
ACT Controls	Linear	FE	ACT Controls	Linear	FE
State FE	X	X	State FE	X	X

[1] $*p < 0.1$ $**p < 0.05$ $***p < 0.01$

[2] Each cell of the table represents a separate, single regression.

[3] Standard errors are in parentheses. All standard errors are clustered at the ACT score by state level.

[4] RD regressions based on a bandwidth of 10

[5] Observation counts rounded to the nearest 10 for data security purposes

Table 4: Funding changes as a result of merit aid

	(1) RD	(2) RD	(3) DD	(4) DD
***Panel A**: Cumulative undergraduate funding variables*				
Total undergraduate loans	-7590***	-7200***	-5857***	-6326***
	(2675)	(2649)	(1621)	(1560)
Any undergraduate loans	-0.046	-0.029	-0.080**	-0.090**
	(0.058)	(0.062)	(0.040)	(0.040)
Total federal Pell grant amount	-1305*	-1353**	-846*	-1071***
	(686)	(590)	(463)	(394)
***Panel B**: Final year funding outcomes*				
Any state merit grant	0.155***	0.163***	0.214***	0.209***
	(0.053)	(0.049)	(0.036)	(0.035)
State merit grant amount	289***	315***	446***	436***
	(109)	(106)	(86)	(84)
Employment income while in school	215	156	-737	-901
	(1188)	(1177)	(671)	(655)
Observations	1450	1440	8970	8880
ACT Controls	Linear	Linear	FE	FE
State FE	X	X	X	X
Exogenous controls		X		X

[1] $*p < 0.1$ $**p < 0.05$ $***p < 0.01$

[2] Each cell of the table represents a separate, single regression.

[3] Standard errors are in parentheses. All standard errors are clustered at the ACT score by state level.

[4] Exogenous controls: race, gender, private high school, number of siblings in college, English primary language growing up, at least one parent foreign, dad/mom has a BA or greater degree

[5] Observation counts rounded to the nearest 10 for data security purposes

Table 5: Changes at graduation as a result of merit aid (2008)

	(1) RD	(2) RD	(3) DD	(4) DD
***Panel A**: Academic outcomes at graduation*				
Graduate of public, in-state school	0.127**	0.129**	0.100**	0.084**
	(0.062)	(0.064)	(0.040)	(0.040)
Undergraduate GPA	-0.041	-0.032	-0.026	-0.027
	(0.055)	(0.052)	(0.035)	(0.035)
Honors graduate	-0.028	-0.026	-0.008	-0.014
	(0.041)	(0.044)	(0.030)	(0.030)
***Panel B**: Institutional selectivity*				
Most selective	0.064	0.057	0.007	0.011
	(0.059)	(0.058)	(0.038)	(0.037)
Most or somewhat selective	0.029	0.013	-0.023	-0.024
	(0.041)	(0.042)	(0.031)	(0.031)
Not selective	-0.029	-0.013	0.023	0.024
	(0.041)	(0.042)	(0.031)	(0.031)
Observations	1450	1440	8970	8880
ACT Controls	Linear	Linear	FE	FE
State FE	X	X	X	X
Exogenous controls		X		X

[1] $*p < 0.1$ $**p < 0.05$ $***p < 0.01$

[2] Each cell of the table represents a separate, single regression.

[3] Standard errors are in parentheses. All standard errors are clustered at the ACT score by state level.

[4] Exogenous controls: race, gender, private high school, number of siblings in college, English primary language growing up, at least one parent foreign, dad/mom has a BA or greater degree

[5] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, institutional selectivity, indicators for majors in 5 categories

[6] Observations rounded to the nearest 10 for data security purposes

Table 6: Changes in salary as result of merit aid (2009)

	(1) RD	(2) RD	(3) RD	(4) DD	(5) DD	(6) DD
***Panel A**: Time use outcomes*						
Enrolled	0.044	0.037	0.018	0.023	0.021	0.014
	(0.057)	(0.057)	(0.058)	(0.037)	(0.037)	(0.038)
Full time employed	-0.026	-0.013	0.007	-0.004	-0.006	0.006
	(0.058)	(0.058)	(0.060)	(0.043)	(0.042)	(0.043)
***Panel B**: Salary outcomes*						
Annualized salary	-5279**	-5253**	-4400**	-2666	-2868	-2018
	(2251)	(2242)	(2241)	(1889)	(1833)	(1801)
Employed, not enrolled salary	-6638***	-7070***	-6391***	-4106*	-4512**	-3833**
	(2242)	(2357)	(2354)	(2144)	(2061)	(1943)
Full time employed, not enrolled salary	-4901**	-5471***	-4886**	-3175*	-3669**	-3655**
	(2088)	(2064)	(2077)	(1877)	(1756)	(1640)
Log full time income	-0.153**	-0.162**	-0.149**	-0.127**	-0.134**	-0.124**
	(0.066)	(0.067)	(0.071)	(0.061)	(0.060)	(0.059)
Observations	690-1330	690-1330	690-1330	4280-8330	4280-8330	4280-8330
ACT Controls	Linear	Linear	Linear	FE	FE	FE
State FE	X	X	X	X	X	X
Exogenous controls		X	X		X	X
Endogenous controls			X			X

[1] $*p < 0.1$ $**p < 0.05$ $***p < 0.01$

[2] Each cell of the table represents a separate, single regression.

[3] Standard errors are in parentheses. All standard errors are clustered at the ACT score by state level.

[4] Exogenous controls: race, gender, private high school, number of siblings in college, English primary language growing up, at least one parent foreign, dad/mom has a BA or greater degree

[5] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, institutional selectivity, indicators for majors in 5 categories

[6] Observations rounded to the nearest 10 for data security purposes

Table 7: Changes in employment outcomes due to merit aid (2012)

	(1) RD	(2) RD	(3) RD	(4) DD	(5) DD	(6) DD
***Panel A**: Employment and enrollment outomes*						
Degree or enrolled	0.126*	0.111	0.111	0.044	0.041	0.042
	(0.071)	(0.069)	(0.072)	(0.045)	(0.045)	(0.044)
Annual salary, employed	1142	452	1207	-2054	-1999	-910
	(4830)	(4559)	(4303)	(3302)	(3197)	(3065)
Annual salary, full time employed	-156	-1236	-514	-2077	-1987	-1104
	(4913)	(4481)	(4207)	(3375)	(3231)	(3188)
***Panel B**: Occupational choice*						
Business occupation	-0.184**	-0.178**	-0.182***	-0.111**	-0.109**	-0.112***
	(0.080)	(0.079)	(0.070)	(0.046)	(0.045)	(0.042)
Education occupation	0.123**	0.125**	0.093**	0.065*	0.066*	0.082**
	(0.058)	(0.056)	(0.043)	(0.039)	(0.040)	(0.036)
STEM occupation	0.022	0.016	0.042	-0.003	-0.005	-0.004
	(0.042)	(0.044)	(0.043)	(0.029)	(0.029)	(0.030)
Observations	870-1210	870-1190	870-1190	5250-7550	5200-7480	5200-7480
ACT Controls	Linear	Linear	Linear	FE	FE	FE
State FE	X	X	X	X	X	X
Exogenous controls		X	X		X	X
Endogenous controls			X			X

[1] $*p < 0.1$ $**p < 0.05$ $***p < 0.01$

[2] Each cell of the table represents a separate, single regression.

[3] Standard errors are in parentheses. All standard errors are clustered at the ACT score by state level.

[4] Exogenous controls: race, gender, private high school, number of siblings in college, English primary language growing up, at least one parent foreign, dad/mom has a BA or greater degree

[5] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, institutional selectivity, indicators for majors in 5 categories

[6] Observations rounded to the nearest 10 for data security purposes

Table 8: Changes in salary due to student loans (2009)

	(1) RD	(2) RD	(3) RD	(4) DD	(5) DD	(6) DD
***Panel A**: Time use*						
Enrolled	-0.005	-0.004	-0.002	-0.003	-0.003	-0.002
	(0.007)	(0.006)	(0.008)	(0.006)	(0.006)	(0.007)
Full time employed	0.003	0.002	-0.001	0.001	0.001	-0.001
	(0.007)	(0.007)	(0.008)	(0.007)	(0.006)	(0.007)
***Panel B**: Salary outcomes*						
Annualized salary	640*	631*	583	411	416	342
	(344)	(339)	(384)	(322)	(296)	(337)
Employed, not enrolled salary	768*	747**	728*	614	636*	641
	(435)	(381)	(411)	(410)	(378)	(434)
Full time employed, not enrolled salary	1230	1020	1170	492	546	613
	(1242)	(823)	(1227)	(369)	(355)	(399)
Log full time income	0.041	0.031	0.031	0.021	0.021*	0.022
	(0.038)	(0.022)	(0.030)	(0.013)	(0.012)	(0.015)
Observations	690-1330	680-1320	680-1320	4280-8330	4240-8250	4240-8250
ACT Controls	Linear	Linear	Linear	FE	FE	FE
State FE	X	X	X	X	X	X
Exogenous controls		X	X		X	X
Endogenous controls			X			X

[1] $*p < 0.1$ $**p < 0.05$ $***p < 0.01$

[2] Each cell of the table represents a separate, single regression.

[3] Standard errors are in parentheses. All standard errors are clustered at the ACT score by state level.

[4] Exogenous controls: race, gender, private high school, number of siblings in college, English primary language growing up, at least one parent foreign, dad/mom has a BA or greater degree

[5] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, institutional selectivity, indicators for majors in 5 categories

[6] Observations rounded to the nearest 10 for data security purposes

Table 9: Changes in employment outcomes due to student loans (2012)

	(1) RD	(2) RD	(3) RD	(4) DD	(5) DD	(6) DD
***Panel A**: Employment and enrollment outcomes*						
Degree or enrolled	-0.018	-0.016	-0.017	-0.007	-0.006	-0.008
	(0.013)	(0.012)	(0.013)	(0.008)	(0.007)	(0.009)
Annual salary, employed	-206	-77	-244	318	302	148
	(864)	(769)	(783)	(526)	(486)	(493)
Annual salary, full time employed	26	194	41	338	313	179
	(821)	(703)	(645)	(570)	(514)	(513)
***Panel B**: Occupational choice*						
Business occupation	0.034	0.032	0.037	0.019*	0.017*	0.019**
	(0.028)	(0.026)	(0.028)	(0.010)	(0.009)	(0.010)
Education occupation	-0.022	-0.023	-0.020	-0.011	-0.010	-0.014**
	(0.017)	(0.015)	(0.012)	(0.007)	(0.007)	(0.007)
STEM occupation	-0.004	-0.003	-0.007	0.001	0.001	0.001
	(0.008)	(0.008)	(0.009)	(0.005)	(0.005)	(0.005)
Observations	870-1210	870-1190	870-1190	5250-7550	5200-7480	5200-7480
ACT Controls	Linear	Linear	Linear	FE	FE	FE
State FE	X	X	X	X	X	X
Exogenous controls		X	X		X	X
Endogenous controls			X			X

[1] $*p < 0.1$ $**p < 0.05$ $***p < 0.01$

[2] Each cell of the table represents a separate, single regression.

[3] Standard errors are in parentheses. All standard errors are clustered at the ACT score by state level.

[4] Exogenous controls: race, gender, private high school, number of siblings in college, English primary language growing up, at least one parent foreign, dad/mom has a BA or greater degree

[5] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, institutional selectivity, indicators for majors in 5 categories

[6] Observations rounded to the nearest 10 for data security purposes

Table 10: Ability heterogeneity results, outcomes at graduation

Panel A: *Cumulative funding outcomes*						
	Total undergraduate loans		Any undergraduate loans		Total federal Pell grant amount	
	(1) RD	(2) DD	(3) RD	(4) DD	(5) RD	(6) DD
Qualified at low cutoff	-7104**	-4823**	-0.038	-0.084	-1838**	-2048**
	(3243)	(2162)	(0.096)	(0.061)	(863)	(909)
Qualified at medium cutoff	-7522**	-6810**	-0.017	-0.045	-761	-785*
	(3286)	(2697)	(0.089)	(0.075)	(599)	(418)
Qualified at high cutoff	-10891**	-7177**	-0.058	-0.130**	-381	-494
	(4302)	(2807)	(0.095)	(0.062)	(691)	(539)
Observations	1440	8880	1440	8880	1440	8880
R^2	0.157	0.101	0.164	0.124	0.415	0.315

Panel B: *Academic outcomes at graduation*						
	Graduate of public, in-state school		Undergraduate GPA		Honors graduate	
	(1) RD	(2) DD	(3) RD	(4) DD	(5) RD	(6) DD
Qualified at low cutoff	0.202*	0.148**	0.015	0.083	0.089	0.055
	(0.109)	(0.071)	(0.087)	(0.059)	(0.072)	(0.045)
Qualified at medium cutoff	0.085	0.030	-0.036	-0.022	-0.091	-0.001
	(0.065)	(0.059)	(0.056)	(0.049)	(0.069)	(0.062)
Qualified at high cutoff	0.062	0.072	-0.121	-0.121**	-0.186***	-0.079*
	(0.085)	(0.065)	(0.078)	(0.061)	(0.062)	(0.047)
Observations	1440	8880	1440	8880	1440	8880
R^2	0.203	0.182	0.190	0.172	0.135	0.124

[1] $*p < 0.1$ $**p < 0.05$ $***p < 0.01$

[2] Each column of the table (within panels) represents a single regression with interactions for whether students qualified for a merit aid program in a state with a low (15-20), medium (21-27) or high (28-30) cutoff score.

[3] Standard errors are in parentheses and all standard errors are clustered at the ACT score by state level.

[4] Regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects.

[5] Observation counts rounded to the nearest 10 for data security purposes.

Table 11: Ability heterogeneity results, outcomes one year post graduation

	Enrolled		Full time employed		Annualized salary	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
Qualified at low cutoff	0.123	0.027	0.003	0.045	-1331	-742
	(0.094)	(0.058)	(0.084)	(0.069)	(3499)	(2209)
Qualified at medium cutoff	-0.017	-0.001	0.058	0.031	-1096	-1690
	(0.073)	(0.072)	(0.096)	(0.076)	(3369)	(3413)
Qualified at high cutoff	-0.064	0.014	-0.024	-0.047	-6268	-3336
	(0.125)	(0.064)	(0.119)	(0.068)	(5189)	(3270)
Observations	1320	8250	1320	8250	1320	8250
R^2	0.166	0.097	0.116	0.068	0.163	0.104

	Employed, not enrolled salary		Full time employed, not enrolled salary		Log full time income	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
Qualified at low cutoff	-2498	-2071	-342	-2272	-0.090	-0.126
	(3870)	(2474)	(3381)	(2331)	(0.137)	(0.086)
Qualified at medium cutoff	-2735	-4186	-856	-4048	-0.001	-0.015
	(3360)	(3549)	(3322)	(3329)	(0.102)	(0.091)
Qualified at high cutoff	-4766	-4989	-5646	-4546	-0.241	-0.209*
	(5277)	(3582)	(4711)	(2822)	(0.183)	(0.115)
Observations	820	5210	680	4240	860	5390
R^2	0.231	0.189	0.325	0.238	0.193	0.147

[1] $*p < 0.1$ $**p < 0.05$ $***p < 0.01$

[2] Each column of the table (within panels) represents a single regression with interactions for whether students qualified for a merit aid program in a state with a low (15-20), medium (21-27) or high (28-30) cutoff score.

[3] Standard errors are in parentheses and all standard errors are clustered at the ACT score by state level.

[4] Regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects.

[5] Observation counts rounded to the nearest 10 for data security purposes.

Table 12: Ability heterogeneity results, outcomes four years post graduation

***Panel A**: Employment and time use outcomes*

	Degree or enrolled		Annual salary, employed		Annual salary, full time employed	
	(1) RD	(2) DD	(3) RD	(4) DD	(5) RD	(6) DD
Qualified at low cutoff	0.019	-0.080	-186	46	-1571	-2596
	(0.130)	(0.094)	(9025)	(6093)	(9725)	(6558)
Qualified at medium cutoff	0.219**	0.097	6393	3670	6855*	5604
	(0.101)	(0.083)	(3994)	(3509)	(3749)	(3481)
Qualified at high cutoff	0.274*	0.095*	-2045	-5709	-5861	-6242
	(0.144)	(0.052)	(8354)	(5442)	(8354)	(5655)
Observations	1190	7480	920	5680	870	5200
R^2	0.136	0.131	0.263	0.182	0.277	0.197

***Panel B**: Occupation choice*

	Business occupation		Education occupation		STEM occupation	
	(1) RD	(2) DD	(3) RD	(4) DD	(5) RD	(6) DD
Qualified at low cutoff	-0.245***	-0.181**	0.161**	0.106**	0.057	0.071**
	(0.092)	(0.078)	(0.065)	(0.046)	(0.060)	(0.032)
Qualified at medium cutoff	-0.203**	-0.082	0.130*	0.135**	0.026	-0.040
	(0.095)	(0.087)	(0.067)	(0.065)	(0.057)	(0.050)
Qualified at high cutoff	-0.213**	-0.082*	0.050	0.024	0.036	-0.033
	(0.104)	(0.047)	(0.094)	(0.064)	(0.085)	(0.056)
Observations	1090	6750	1090	6750	1090	6750
R^2	0.203	0.130	0.327	0.224	0.241	0.224

[1] $*p < 0.1$ $**p < 0.05$ $***p < 0.01$

[2] Each column of the table (within panels) represents a single regression with interactions for whether students qualified for a merit aid program in a state with a low (15-20), medium (21-27) or high (28-30) cutoff score.

[3] Standard errors are in parentheses and all standard errors are clustered at the ACT score by state level.

[4] Regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects.

[5] Observation counts rounded to the nearest 10 for data security purposes.

Table 13: Family income heterogeneity results, funding outcomes at graduation

Panel A: *Cumulative funding outcomes*						
	Total undergraduate loans		Any undergraduate loans		Total federal Pell grant amount	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
$< 25\%$	-6594	-3997	0.054	-0.011	2805***	2484***
	(4185)	(2970)	(0.084)	(0.061)	(910)	(849)
$25\% - 50\%$	-9718***	-7483***	-0.058	-0.111*	-2268***	-2049***
	(3093)	(2169)	(0.077)	(0.061)	(671)	(499)
$50\% - 75\%$	-9052***	-6931***	-0.071	-0.113**	-2629***	-2210***
	(2654)	(1837)	(0.076)	(0.049)	(544)	(394)
$\geq 75\%$	-7774***	-5856***	-0.050	-0.083	-1096**	-561
	(2944)	(1739)	(0.085)	(0.051)	(507)	(401)
Observations	1440	8880	1440	8880	1440	8880
R^2	0.158	0.101	0.167	0.124	0.490	0.331

Panel B: *Final year funding outcomes*						
	Any state merit grant		State merit grant amount		Employment income while in school	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
$< 25\%$	0.157**	0.222***	265	454***	-1462	-2242**
	(0.074)	(0.055)	(179)	(137)	(1122)	(924)
$25\% - 50\%$	0.153***	0.190***	246*	391***	337	-481
	(0.055)	(0.051)	(147)	(125)	(1242)	(940)
$50\% - 75\%$	0.165***	0.212***	292**	446***	-520	-1086
	(0.055)	(0.044)	(141)	(111)	(1056)	(721)
$\geq 75\%$	0.133***	0.212***	184	445***	22	-462
	(0.051)	(0.040)	(121)	(96)	(1099)	(756)
Observations	1440	8880	1440	8880	1440	8880
R^2	0.311	0.341	0.296	0.331	0.096	0.056

[1] $*p < 0.1$ $**p < 0.05$ $***p < 0.01$

[2] Each column of the table represents a single regression with separate treatment indicators for students in each family income bracket. Family income quartile based on within-sample quartiles of family income. Note that the within-sample distribution of family income lies significantly to the right of the national distribution of household income.

[3] Standard errors are in parentheses. All standard errors are clustered at the state by score level.

[4] Regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects.

[5] Observation counts rounded to the nearest 10 for data security purposes.

Table 14: Family income heterogeneity results, graduation outcomes

	Graduate of public, in-state school		Undergraduate GPA		Honors graduate	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
$< 25\%$	0.002	-0.035	-0.009	-0.009	-0.070	-0.017
	(0.088)	(0.070)	(0.072)	(0.057)	(0.068)	(0.055)
$25\% - 50\%$	0.133*	0.075	0.010	0.007	0.001	0.048
	(0.077)	(0.061)	(0.076)	(0.064)	(0.061)	(0.045)
$50\% - 75\%$	0.170**	0.122**	-0.074	-0.062	-0.098*	-0.049
	(0.066)	(0.048)	(0.058)	(0.043)	(0.050)	(0.039)
$\geq 75\%$	0.128*	0.089*	-0.103	-0.017	-0.083	-0.008
	(0.068)	(0.049)	(0.067)	(0.050)	(0.065)	(0.046)
Observations	1440	8880	1440	8880	1440	8880
R^2	0.208	0.183	0.192	0.171	0.133	0.124

[1] $*p < 0.1$ $**p < 0.05$ $***p < 0.01$

[2] Each column of the table represents a single regression with separate treatment indicators for students in each family income bracket. Family income quartile based on within-sample quartiles of family income. Note that the within-sample distribution of family income lies significantly to the right of the national distribution of household income.

[3] Standard errors are in parentheses. All standard errors are clustered at the state by score level.

[4] Regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects.

[5] Observation counts rounded to the nearest 10 for data security purposes.

Table 15: Family income heterogeneity results, outcomes one year post graduation

	Enrolled		Full time employed		Annualized salary	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
$< 25\%$	0.031	0.008	0.051	0.022	-2194	-1773
	(0.075)	(0.058)	(0.092)	(0.069)	(3115)	(2525)
$25\% - 50\%$	0.008	0.023	0.052	0.020	-3586	-2954
	(0.077)	(0.055)	(0.083)	(0.064)	(2966)	(2552)
$50\% - 75\%$	0.046	-0.011	0.020	0.036	-1728	-1168
	(0.070)	(0.050)	(0.082)	(0.055)	(2944)	(2261)
$\geq 75\%$	0.027	0.036	-0.030	-0.040	-1093	-2493
	(0.075)	(0.048)	(0.082)	(0.052)	(3370)	(2090)
Observations	1320	8250	1320	8250	1320	8250
R^2	0.164	0.097	0.117	0.068	0.163	0.104

	Employed, not enrolled salary		Full time employed, not enrolled salary		Log full time income	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
$< 25\%$	-4265	-5018*	-4157	-4988*	-0.089	-0.156*
	(3371)	(2655)	(3030)	(2679)	(0.105)	(0.092)
$25\% - 50\%$	-5669*	-4884*	-3338	-4226*	-0.152	-0.124
	(3156)	(2935)	(3050)	(2338)	(0.105)	(0.083)
$50\% - 75\%$	-1210	-2613	-777	-3018	-0.038	-0.112
	(2973)	(2427)	(2731)	(2263)	(0.094)	(0.087)
$\geq 75\%$	-1985	-4155*	1865	-3541*	-0.055	-0.124*
	(3486)	(2280)	(3117)	(2111)	(0.118)	(0.073)
Observations	820	5210	680	4240	860	5390
R^2	0.235	0.189	0.331	0.239	0.192	0.146

[1] $*p < 0.1$ $**p < 0.05$ $***p < 0.01$

[2] Each column of the table represents a single regression with separate treatment indicators for students in each family income bracket. Family income quartile based on within-sample quartiles of family income. Note that the within-sample distribution of family income lies significantly to the right of the national distribution of household income.

[3] Standard errors are in parentheses. All standard errors are clustered at the state by score level.

[4] Regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects.

[5] Observation counts rounded to the nearest 10 for data security purposes.

Table 16: Family income heterogeneity results, outcomes four years post graduation

***Panel A**: Employment and time use outcomes*

	Degree or enrolled		Annual salary, employed		Annual salary, full time employed	
	(1) RD	(2) DD	(3) RD	(4) DD	(5) RD	(6) DD
$< 25\%$	0.172*	0.039	-484	-2737	-1574	-3318
	(0.098)	(0.080)	(5771)	(4578)	(5855)	(4747)
$25\% - 50\%$	0.150	0.059	2532	-65	671	-715
	(0.093)	(0.065)	(4786)	(3638)	(4947)	(3843)
$50\% - 75\%$	0.125	0.022	3547	-1044	2493	-1604
	(0.093)	(0.057)	(5133)	(3559)	(5441)	(3652)
$\geq 75\%$	0.168	0.058	4172	-618	5512	-37
	(0.102)	(0.059)	(5685)	(3880)	(5971)	(3969)
Observations	1190	7480	920	5680	870	5200
R^2	0.134	0.130	0.262	0.181	0.275	0.196

***Panel B**: Occupation choice*

	Business occupation		Education occupation		STEM occupation	
	(1) RD	(2) DD	(3) RD	(4) DD	(5) RD	(6) DD
$< 25\%$	-0.229**	-0.105*	0.083	0.019	0.000	-0.043
	(0.089)	(0.064)	(0.058)	(0.050)	(0.052)	(0.042)
$25\% - 50\%$	-0.227***	-0.140**	0.157**	0.124**	0.029	-0.027
	(0.079)	(0.057)	(0.069)	(0.055)	(0.049)	(0.035)
$50\% - 75\%$	-0.268***	-0.159***	0.148***	0.101**	0.061	0.006
	(0.074)	(0.046)	(0.052)	(0.045)	(0.057)	(0.043)
$\geq 75\%$	-0.134*	-0.036	0.112*	0.055	0.063	0.016
	(0.081)	(0.054)	(0.066)	(0.046)	(0.062)	(0.041)
Observations	1090	6750	1090	6750	1090	6750
R^2	0.210	0.132	0.328	0.225	0.243	0.224

[1] $*p < 0.1$ $**p < 0.05$ $***p < 0.01$

[2] Each column of the table represents a single regression with separate treatment indicators for students in each family income bracket. Family income quartile based on within-sample quartiles of family income. Note that the within-sample distribution of family income lies significantly to the right of the national distribution of household income.

[3] Standard errors are in parentheses. All standard errors are clustered at the state by score level.

[4] Regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects.

[5] Observation counts rounded to the nearest 10 for data security purposes.