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 at the same time merit aid scholarship programs have become an increasingly popular avenue for states to subsidize higher education. I use merit aid program eligibility in thirteen states with sharp test score cutoffs to evaluate the effects of college funding on the early labor market outcomes of college graduates. I also examine the heterogeneity of the effects with respect to ability and family income. I demonstrate that the primary causal effect of qualifying for a merit aid program is to lower the loan burden of students by $5800-$7200. Eligibility has little impact on other outcomes while in school. However, students who qualify for merit aid programs have $2300-$6000 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 $400-$800 one year after graduation. 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 offering loans to students attending college. Little research has been done to empirically evaluate the effects of either of these policies on college graduates as they enter the labor market. This paper will begin to fill this gap by exploiting sharp eligibility cutoffs in state merit aid programs to both evaluate the impact of the programs on students as they enter the labor market, and also to determine whether 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. After graduation I find that students who qualified for merit aid seek out jobs that are lower paying, but presumably have higher non-pecuniary benefits, than their peers who took on loans for school.

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" and keep smart students from moving away for college and their subsequent careers. In practice they represent a very large transfer of funds from states to students pursuing higher education. These programs determine eligibility based on state residence and measures of high school performance, such as scores on the ACT or SAT, 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 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 test score cutoffs that either significantly or entirely inform eligibility for the scholarships, providing a sharp discontinuity in the amount of available scholarship

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

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 student with a positive amount of loans (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 surpassed $1.3 trillion (www.finaid.com).

The motivating questions being addressed in this paper are twofold. First, what are the effects of merit aid programs on college funding and on outcomes after graduation? Second, to the extent that merit aid eligibility is a good instrument for loans, what effect do student loans have on graduates as they enter the labor market? To address these broad questions of interest, this paper will answer three specific main 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?

This project fits into a broader conversation about the effect of the way in which students fund their college education. I find that qualifying for merit aid discretely changes the amount of loans with which students graduate, which makes merit aid 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 plausible 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 accept earlier job offers than those 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 look 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 and unpaid internship that leads to a lucrative career.

A straightforward regression of salary on loans produces the correlation about which policy-markers are concerned[3]. 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, as can be seen in figure 2. These correlations may, however, be subject to significant unobserved selection. For instance, income and post-baccalureate enrollment are positively correlated with family wealth, which is also negatively correlated with loan burden. In 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 helping out 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 wealth is responsible. To address these selection issues I use eligibility for state merit aid programs 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.

Using both regression discontinuity and difference in difference approaches, I find that students who are eligible for merit aid programs accumulate \$5800 - \$7200 less debt than otherwise observationally equivalent students, and earn \$2300 - \$6000 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.

[3]Author's calculations

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 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. I also find little evidence that students are induced to remain in their home states one year after graduation, though these results are quite imprecise.

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. Carrying out this analysis reveals that an increase of \$1000 in student loans generates a causal increase of \$400 - \$800 more in annual salary for graduates one year after graduation. Applying the same approach to the results on occupation 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 four years after graduation. These results suggest that students with loans are not nearly as adversely impacted - at least in a financial sense - by their loans in the labor market as the popular press would have us believe, and 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 my 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. Furthermore, it is the first paper to my knowledge that examines 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, interesting differences arise in college major and subsequent occupational choices when examining how students differ across both the test score and family income distributions.

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. This project contributes to the literature by providing a comprehensive analysis of the effect of merit aid programs on the short to medium run labor market outcomes of college graduates, and by providing a quasi-experimental analysis of the effect of student loans on outcomes after college graduation. I consider outcomes that have been understudied in the literature thus far, and the design of both the empirical strategy and the data allow me to examine heterogeneity of the effects by student ability and family income. As a result, this research is significant for developing an understanding of how current financial aid policy impacts college graduates in the long run.

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 the assumptions required to use merit aid eligibility as an instrument for loans, and 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 (Rothstein and Rouse report effects on the income of accepted jobs that will be taken after graduation). These papers are well identified and convincing, but they are 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 students to take jobs in corporate law rather than public interest law fields more often than their peers offered an aid package framed as grants. 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) is the most closely related paper to mine in terms of topic and methodology. They use a change in the financial aid policy of a highly selective, elite private college to determine the effect of a change in loan burden 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. While these papers address very similar questions to mine, the samples considered are highly selected, so it is an open question whether the results from these studies will generalize to a more nationally representative population. By using broad, state-based merit aid programs that have a large amount of variation in their requirements I will be able to address whether the results found in these papers are generalizable across 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 (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. 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 couple 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 using an instrument based on the rate of expansion of federal student loans. She finds that greater student loan debt is associated with a lower probability of being married, evidence that students with debt are constrained in their choices after college.

Only one theoretical paper to my knowledge directly addresses the link between credit constraints in education and outcomes after college. Kaas and Zink (2008) study a model in which students are heterogeneous in their initial education-specific wealth endowment and in their academic ability, and in which students endogenously choose education investment. They find that graduates with loans pursue higher paying but more competitive jobs, and thus spend more time unemployed than those without loans. This result stems directly from a feature of the model that loans need not be serviced while the worker is unemployed which is analogous to the introduction of income-based repayment in recent years.

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 that primarily focuses on a wide variety of outcomes, though few are able to address outcomes beyond those realized in college, such as enrollment rates, persistence rates, and even such outcomes 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 particular states 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) and Massachusetts (Goodman, 2008; Cohodes and Goodman, 2014) and programs that are more selective, 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. 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 be concerned with this sort of "gaming" of the test in applying my identification strategy. To address it, I show that the characteristics of students on either side of the score cutoffs in the aggregate are balanced, and that while there does seem to be a mass of students above the cutoff, the test does not seem to be precisely manipulable since there is a smooth transition exactly over the cutoff.

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) examined the migration patterns of students over time to see if the programs succeeded in their stated goals of keeping bright students in the state from which they graduated high school. 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, to my knowledge, taken advantage of the sharp ACT score cutoffs across multiple programs as I will be able to do in this study.

3 Data

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 an instrument for the cumulative total of loans a student who graduated high school in that state must take on in order to complete an undergraduate degree, as well as a treatment variable of interest in its own right.

Many states have adopted merit aid programs over the past 25 years. To make use of this variation, I compiled a database of state merit aid programs and their features from several different sources. I first searched all 50 states for merit aid programs offered to resident high school graduates. I then combined the results of this search with a dataset on all state aid programs administered by the National Association of State Sponsored Grant Aid Programs (NASSGAP) that includes information on the existence of programs across all states and DC from 2003-2013. I merged the data on total number of students affected by each program with the results of my survey of merit aid programs, then matched the data from each state to state level enrollment records from the IPEDS database on institutions of higher education in the US. From these data I then produced a measure of the percent of flagship tuition[4] the award represented in the 2007/08 academic year observed in my dataset (program generosity) and the percent of the student population receiving the award (program selectivity).

I then deemed a program "eligible" for my analysis if it met all of the four 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 on high school performance in the form of 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

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

analysis. As a robustness check I drop these states from the analysis entirely and rerun the results. The results are substantively unchanged by this alternative definition of "control" states.

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 additional barriers (such as interviews or specialized applications) to receiving the scholarship other than those required of any student applying for financial aid. Particularly, none of these programs are selective in the sense that a subset of qualifying students are selected 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. At least 1% of public school enrolled students are affected by the program. This imposes a minimum take up size for a program to be considered affecting the target population.

4. 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 which programs are used and which states have programs that are not used appears in figure 3. A sense of the variety of the programs can be gained from looking at figure 4 and at figure 5. The programs have ACT score cutoffs that range from 15 to 30. This variation will allow me to examine the differences in the treatment effects across the ability distribution. 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 three 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. To examine the variation in take-up across states, I calculated the number of students receiving the scholarship in 2007/08 (as documented in the NASSGAP data) and the number of full-time enrolled students at all state public degree granting institutions. The ratio of these two measures can be termed the fraction of full time enrolled students benefiting from the scholarship. This measure also varies widely across states as a result of variation in eligibility, take-up, maintenance of eligibility and in-state relative to out of state enrollments and is surprisingly unrelated to the size of the maximum award made as can be seen in figure 5.

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, which is something I will discuss in greater detail in the next section. Third, while some students have an SAT rather than ACT score, students in all states are allowed to qualify with either an SAT or ACT score, and the conversion between the two is consistent with the score crosswalks published by both the SAT and ACT administrating bodies. 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. Finally, it is worth noting that high school GPA is not used more generally in this analysis, even though it would allow me to include more merit program states. In the data I have it is too coarsely binned to be used to determine whether students qualified based on their GPA or not. Several of the GPA cutoffs fall in the interior of the bins defined for the high school GPA variable.

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), take up of loans to fund a college degree 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 of the class of 2007/08. It includes information on pre-college characteristics, family background information, information on college undergraduate experiences, and two waves of follow up survey data post graduation, one year post graduation in 2009 and four years post 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 about family circumstances prior to college graduation. Third, the dataset includes detailed measures on college funding, including cumulative loans students took out for undergraduate education, 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, including income, occupation and further school enrollment post-graduation. In particular the three part link between high quality testing data, college funding and experiences after college is difficult to construct and will prove advantageous for examining the effects of loans on outcomes in this paper.

For the purposes of this study I will restrict the sample to those students who have a non-missing observation for high school ACT or SAT score, which effectively restricts the sample to students who are between the ages of 18 and 24 upon college graduation, and 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 more or less traditional schedule. Students have close to

$15,000 of student loans on average. This figure is inclusive of the almost 40% of students who graduate with no loans, putting the average loan burden of a student with loans at roughly $23,500 - $24,000. Half to two-thirds of students graduate from public schools in their home state (as calculated by the state of residence of their parents in their final year of school), and have a cumulative undergraduate GPA of about 3.2 in both merit aid program and non-program states. One year after graduation almost 70% of individuals are employed full time, with an 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 above $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 qualifying for merit aid programs in their state. This binned data masks a large amount of heterogeneity across programs and states in the number 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 outcomes of students as they enter the labor market 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 3. The essence of this regression discontinuity approach is to compare students who just managed to meet the eligibility cutoff with students who just missed the eligibility 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 a student just above and a student just below the cutoff in their merit-aid program state with the difference between a student just above and below the same ACT score in states that do not have merit aid programs. This specification 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, while also comparing 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 behind the regression discontinuity approach is that because students cannot precisely manipulate the running variable, those just above and below the cutoff are randomly distributed around the cutoff. Thus, the approach compares students with similar ACT scores who differ only on their status as qualifying or not for the merit program in their state.

For this analysis I restrict the sample of individuals to students whose parent's residence in their final year of school is in a qualifying merit program state, and to those within ten points of the cutoff score in each 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 results are remarkably consistent to using any bandwidth between one and ten points[5], so ten is selected 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[6] 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, centered by the cutoff score in her 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 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,

[5]Robustness to bandwidth selection for cumulative loans and salary can be seen in figures 10 and 11, and similar figures for other variables are available upon request from the author.

[6]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.

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 6. 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 affect 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 a random allocation.

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, supporting 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. If 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 in my sample of students who have graduated. 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 which uses the variation across states of having a merit aid program in place or not in addition to the discontinuities induced by some students falling above or below the score eligibility cutoffs. The intuition behind this approach is to compare students within ACT score across states in which they are and are not eligible for merit programs by virtue of the programs existing or not, or by virtue of the eligibility cutoffs varying across states. This goes beyond comparing students in states with and without programs, since there is such a large spread of cutoff scores in states with qualifying 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 of this variation in cutoff scores 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, especially since it will allow me to 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 state based on their test scores, γ_a are ACT score fixed effects, γ_s are state fixed effects, X_{ias} is a vector of exogenous baseline controls (gender, race and family income) and W_{ias} is a vector of endogenous controls representing experiences of the student while in college that might affect outcomes after college, such as GPA, college major, whether the student graduated with honors, and whether the school from which the student graduated was public or private. In this specification, α_1 is the treatment effect of qualifying for a merit aid program on outcomes after graduation.

The identifying assumption here is that absent the programs, the marginal effect of obtaining a particular score on the ACT on later life outcomes would be the same as in states with and without the program. 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 7. 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 broad merit aid programs (as they are defined in this application), while students who do qualify and live in merit program states have substantially higher merit aid in their final year of school than either of the other groups.

While we might expect the function of merit aid by ACT score to discretely increase at the score cutoff for qualifying individuals in merit aid programs states, this is not the case. Rather, it falls off both at very low and very high scores. There are two forces responsible for this trend. First, this is a plot of the average state merit aid each student receives in their final year of school only. Students who score very low on the ACT likely have greater difficulty meeting the renewal requirements than those higher up the score distribution, explaining why those with very low ACT scores may not appear to have benefited from merit aid at all even though they qualified initially based on their scores in high school. Second, students must attend eligible schools (public, in state institutions) in order to redeem the scholarship. Students with very high ACT scores are less likely to attend public in state schools, forgoing the broad scholarship available in state for higher quality private and/or out of state schools. Thus, students with very high ACT scores also do not appear to be receiving state merit only grants in their final year of school.

4.3 Results

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 8 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 $5500-$7500 less debt than their peers (depending on the specification), 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.

Students are not less likely to have any loans, though, showing that the changes in loans are driven primarily by intensive margin changes in loans adopted to pay for school. Qualifying for merit aid does offset the total amount of federal Pell grants by a statistically significant amount of between $900 and $1300, though the magnitude of the change is much smaller than the change in the amount of loans offset by qualifying for state merit grants. This shows that the primary effect of state merit aid is to offset loans students are taking on to graduate.

It may seem natural to consider whether students are more likely to receive state merit-only grants, and the magnitude of that increase. Unfortunately, these measures are only available for the final year of the undergraduate degree, and are not available as cumulative measures like student loans or Pell grant receipt. 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, though the magnitudes are rather small. This is a result of this stock vs. flow issue between comparing cumulative loans relative to grants only in the final year of school. Any students who received the scholarship for less than their full undergraduate career, either because they timed out of the scholarship[7], because they failed to maintain the necessary GPA for scholarship renewal or because they chose not to take up the scholarship in the first place by attending a qualifying school, contributes to the drop in total undergraduate loans but not to the drop in the amount of state merit only grants observed in the final year. Thus, loans represent a more accurate summary representation of the funding patterns over all years of attendance, rather than only the final year of school.

Table 4 also demonstrates that qualifying for merit aid grants does not have any impact on the amount of employment income during the final year of school implying that on average merit aid grants does not offset employment income as a means of college financing. This result does mask some heterogeneity that I will discuss later in section 6.

Students are mechanically induced by the scholarship to attend different schools, as can be see in table 5. 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 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. There may also be a bargaining effect, that students who did not redeem the scholarship were able to leverage the option of the award

[7]Many scholarships have a time limit of four years, or eight semesters, or a cap on benefits. Students who maintained eligibility for the scholarship but nonetheless took longer to complete a degree than the term of the scholarship may not appear in the data as benefitting from merit only grants in their final year of school.

at other schools for a more favorable financial aid package.

This result implies a natural question: on what margin[8] is the composition of graduates changing, and will that have an impact on students after graduation? The next two rows of results in this table shed some light on this question. The effect on public, in-state graduates is clearly composed of movement on both margins, since neither estimate is as large as the total change in public in-state graduates. It does seem, however, that the primary margin of change is to switch students from private to public schools in-state, as the amount by which the fraction of in-state graduates changes in response to qualifying for merit aid programs is smaller and statistically insignificant in all specifications. Empirically it is unclear what effect this change in composition might have on outcomes after graduation, though these results do rule out the fear that students are opting out of attending better out-of-state schools in favor of in-state schools. Rather, students who would have likely stayed in-state for their college education regardless are switching the type of institution they attend.

Examining other measures of the undergraduate experience in table 5, we see 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 effects on months between post-secondary enrollment and degree completion (months to graduation), 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, and find some evidence that students change their major in response to the programs, though the results are inconsistent across the two types of 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. There is also some evidence that students are slightly less likely to major in education-related fields when they benefit from merit aid programs, though this effect is only significant in the DD specification, and is much smaller in magnitude in the RD specification.

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

[8]The public vs. private school margin or the in-state vs. out-of-state margin

are driven entirely by changes in choices while in school. To further address these concerns I report specifications including these endogenous controls as well as controls for the final primary undergraduate major directly to demonstrate that the results are robust to their inclusion. In any case, any changes to choices while in school will pose a problem only for the identification of student loans as an instrument for merit aid. In the program evaluation sense, they 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

Table 6 shows the reduced form effects of qualifying for merit aid on time use one year following graduation. Students who qualified for the merit aid programs are not significantly different from their peers who just missed qualification cutoffs (RD estimates) or in states without merit aid programs (DD estimates) in their rates of being employed full time, part time, or out of the labor force. Specifically, students who qualified for merit aid are not any more likely to be enrolled in post-baccalaureate education. However, they are more likely to be unemployed. Since this survey represents a snapshot at one date in the year after graduation, and students are more likely to be unemployed but not more likely to be out of the labor force, this could imply that students who benefitted from merit aid programs are more likely to take risks and quit a job that is not a good match to search for a better one.

There are also significant differences between students in income generated from their employment activities, which are presented in table 7. Students who benefitted from merit aid programs have lower income than their peers. This result is robust to including endogenous controls for outcomes while in college (beyond the method of funding) and to using 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 my results are smaller in magnitude but still robust to the inclusion of occupation fixed effects. The magnitude of the point estimates is reduced by including 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.

Despite the largely null results on employment and time use in table 6, we might imagine that students are perhaps enrolled while employed, and that this result is coming entirely from students have lower income while splitting their attention between school and work, or that the results are driven by the students who are unemployed. This is not the case. Moving down the table, I restrict to students who are employed, employed and not enrolled, employed full time (35 hours or more worked in a typical week) and employed full time 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 occupational fixed effects. 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. One possible explanation for this could be that students who are enrolled and employed are at that time taking on debt for further schooling, and thus will start to behave more like their peers who took on greater undergraduate debt.

Table 8 addresses the potential conclusion that students who qualified for state merit aid are sorting into different occupations, particularly occupations that pay less. There is some support for this conclusion, especially since we see that including occupation fixed effects in table 7 draws the estimates of the coefficients down towards zero, though all are still negative and several still significant. Table 8 shows that students who benefit from merit aid programs are indeed more likely to sort into the “fun” occupations of the entertainment industry, which includes art, design, sports, communications and music occupations, 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. Note that these results are consistent even after controlling for primary undergraduate major, in columns 3 and 6 of the table.

4.3.3 Effects four years post graduation

Turning to the reduced form effects of programs four years post graduation in table 9, 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. 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 positive and negative, quite small, 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 and are in the process of surpassing the income of their peers. 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 table 10. Interestingly, they are somewhat different from the occupational differences demonstrated in table 8, one year after graduation. 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 will have higher 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 also 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. That is, the measure of $Loans_i$ is not orthogonal to the error term ε_i. For instance, if outcomes after college are correlated with parental wealth and networks that are unobservable to the researcher, but parental wealth and networks have an impact on both the total amount of loans a student has (a negative impact) and the student's outcomes after college (a positive impact), then an estimate of β_1 in the specification above would be biased downwards, since 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 amount of loans students have accumulated when they complete a BA degree. Because we have shown in the previous section 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. I discuss in greater detail the assumptions required to satisfy the exclusion restriction for this estimation below.

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.

For both of these approaches, I calculate whether a student qualified for his/her state merit aid program based on ACT, SAT score and a rough measure of high school GPA, then use ACT score exclusively as the running variable for the regression discontinuity specifications and as fixed effects for the difference in difference specifications. In order to plausibly employ this design, I only include program states that use test scores primarily or exclusively to calculate eligibility, as was outlined in the previous section.

The second stage estimating equation for these specifications will closely resemble the specification in 3, 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). The relevant second stage estimating equation will then 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, based on the residence of her parents in the final year of her undergraduate degree. 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 control for ACT score based on the ACT score and the state-specific score cutoff for merit aid program eligibility, γ_s, state fixed effects, X_{ias}, a vector of exogenous baseline characteristics and W_{ias}, a vector of potentially endogenous college performance measures, such as time to graduation and undergraduate GPA. 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 8. Students who qualify for the merit aid programs have between \$6000 and \$8000 fewer loans than their peers, depending on the specification used. In fact, there is a stronger effect of qualifying for the programs on student loans than on having any state merit only grants. This result stems from the fact that student loans are a cumulative measure of college financing choices over the full undergraduate career, whereas the state merit only grant amount is measured only in the final year of school, making the cumulative amount of loans a more appropriate measure to use in examining the total effect of college funding on outcomes after college.

The second requirement for the instrumental variables 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 its effect on loans. If this requirement does not hold all, 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. In order to establish that this assumption is plausible, I examine the effects of merit aid programs on outcomes while students are in college that might feasibly affect outcomes after graduation, such as major selection, final undergraduate GPA, whether the student was an honors graduate, and whether the student graduated from a public school in her home state. Merit programs have no demonstrable effects on students in my sample, other than shifting them 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), and it lends support to the assumption of the exclusion restriction that other measures of the college experience seem to be unchanged by the merit aid programs. Furthermore, this is consistent with the results in Cornwell et. al. (2006), that the programs primarily shifted the choice of where, not whether, to attend college. 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 Results: Effect of student loans on outcomes after graduation

5.2.1 Effects one year post graduation

Turning attention to the IV results in table 11, we see that students who have higher loans are in fact making more money, completely contradicting the concerns in the traditional policy discussion as outlined in figures 1 and 2. While the results are imprecise, the point estimates suggest that for each additional $1000 in loans students have when they complete an undergraduate degree, they are making an additional $400 - $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 the loans in 2-3 years.

Table 12 shows that the results shown in table 8 are consistent in the IV analysis. Students with $1000 more loans are 0.3 - 0.7 percentage points more likely to have an occupation in healthcare, and 0.5 - 0.7 percentage points less likely to have an occupation in the entertainment industry. At a within-sample average of roughly $24,000 of loans, this translates to being 7.2-16.8 percentage points more likely to have a job in a healthcare occupation, and 12.0 - 16.8 percentage points less likely to have a job in the entertainment industry. This suggests that students with higher amounts of loans are selecting into more "practical" occupations, that may have higher immediate returns.

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 are sacrificing something in the balance.

5.2.2 Effects four years post graduation

The IV results in table 13 are again more imprecise than the reduced form results, but they suggest a pattern that speaks to 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, and they have held fewer jobs between achieving their degree and being interviewed in 2012. A student with the average amount of loans in the sample (approx $24,000) will have held roughly one fewer job by 2012 than an observationally equivalent student with no debt.

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 related to the cumulative amount of loans students take on. These results are reported in table 14. 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 4 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

In this section I examine the heterogeneity of the reduced form results with respect to three different dimensions: student individual ability and family income. 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 very different points in the national ACT score distribution. 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, interesting patterns arise when examining the effects on major and occupational selection.

6.1 Individual ability

One possible explanation for the main results outlined above is 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, so only high quality students sort into a college education. Introducing a scholarship program would lower 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. I address this possible sorting effect by examining the treatment effect across the distribution of ACT scores, and find that the effects are remarkably similar across 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 9, 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 (e.g. UNC-Chapel Hill, UVA, UC-Berkely, UM-Ann Arbor) 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. 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 have to take in order to generate sufficient income to pay off their loans.

In order to estimate these effects directly, I include two additional indicators in the main estimating function: an interaction between whether the individual qualified for the merit aid program in a state with a "low" score cutoff (between 15 and 20) or a "high" score cutoff (between 28 and 30)[9]. 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 15 and 16 for selected outcome variables. The total effect of qualifying for a program in a state with a low (high) cutoff will be the sum of the main effect of qualifying for a merit aid program and the specific interaction effect of qualifying for a merit aid program in a state with a low (high) score cutoff.

6.1.1 Effects by ability at graduation

The results in table 15 demonstrate that there is not much heterogeneity based on ability in the amount by which cumulative loans are decreased, though there are differences in how quickly students graduate from school. Students graduating from schools with very high cutoffs take longer to graduate by about a semester. This may be a result of students with very high ACT scores being more able in general, and could operate in a number of different ways. They may be more likely to have college credit when starting school, and thus would be able to finish a degree more quickly than their peers with lower ACT scores who may not have performed as well in their high school careers. They also may be more able to maintain eligibility for the merit aid scholarship throughout their undergraduate career. Combining these two things, students who qualify for merit aid programs at very high cutoffs may have an incentive to stay in school longer to reap the benefits of the scholarship, whereas their peers who just missed qualifying for the scholarship who are still capable of graduating a semester early will do so, containing their college costs along a different margin.

Looking at panel B in the same table shows that the mixed effects on major choice in the main specifications mask large amounts of variation at different points in the ability distribution. Students who qualify for the scholarship at high cutoffs are far less likely to major in STEM fields, while individuals who qualify at other points of the ability distribution are

[9]States with low score cutoffs are Mississippi, Kentucky, Tennessee, Arkansas, Florida, Louisiana and Idaho, states with high score cutoffs are New Jersey and Missouri

unaffected. Those who qualify at low score cutoffs are no more likely to major in humanities fields, while those who qualify at mid-range or high scores are far more likely to major in humanities. Likewise, students who qualify at low score cutoffs are more likely to major in education, despite the main effect of merit aid programs being to discourage students from majoring in education.

6.1.2 Effects by ability one year after graduation

Turning to the results of merit aid one year after graduation in table 16, we see that there is in fact an overall negative effect of the efficacy of merit aid programs keeping individuals in state one year after graduation, but that students who qualify at with high score cutoffs may be more likely on net to stay in their home state as a result of merit aid programs. Since this is the stated goal of many merit aid programs - keep strong college graduates in state - it is interesting to note that the more selective merit aid programs may be doing a better job of achieving this goal than the more broad-based programs. This could have significant implications for the design of these programs, particularly since the cost of more broad-based programs can be quite a bit higher than more targeted scholarships. The results on time use also mask a great deal of heterogeneity by ability. Individuals who benefited from merit aid programs at high score cutoffs are more likely to be employed part time and less likely to only be enrolled in post-baccalaureate education, which may imply that they are more likely than their peers to be working part time while attending school. Overall, however, students who benefit from merit aid are less likely to be employed full time and more likely to be enrolled one year after graduation. The results in panel B, exploring the differences in salary outcomes across individuals, suggest that the differences in salary are primarily driven by individuals who qualify for programs at very high score cutoffs, which is consistent with the patterns evident in figure 9. The estimated treatment effect for those from states with very low treatment ACT cutoff scores have salaries indistinguishable from those of their peers who missed the cutoff scores for eligibility.

6.1.3 Effects by ability four years after graduation

Interesting patterns are evident in the effects four years after graduation, shown in table 17. There is a positive effect of qualifying for merit aid on having completed or being enrolled in a post-baccalaureate degree program four years after graduation, but this effect is operating primarily for students at the mid- to high score ranges of the ACT. This result is intuitive, in that we wouldn't expect low-ability students to necessarily benefit from pursuing graduate education, but also may imply that the roles of merit aid programs in different states may serve very different purposes. States with programs that set very low score cutoffs may directly be subsidizing students to complete bachelor's degrees, while states with programs that set very high score cutoffs may be indirectly subsidizing students who would have completed bachelor's degrees anyway to pursue post-baccalaureate degrees. Interestingly these results - while very imprecise - imply that the effects on income four years after graduation that are fairly precisely zero in the aggregate results, may in fact be zero for students from the low end of the ACT score distribution, positive in the middle of the ACT score distribution, and negative at the high end of the ACT score distribution. These differences in results on income are further supported by heterogeneity on occupational choice four years after graduation, explored in panel B. While students seem to consistently be less likely to have an occupation in business after graduation, students are in fact more likely to have an occupation in a STEM field or in education if they qualify at low score cutoffs, and are decreasingly likely to have either occupation moving up the ability distribution.

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, and in assessing the value of merit aid programs more generally, 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. This is different from the preceding specification in that each coefficient 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 18. The effect of merit aid eligibility is statistically indistinguishable across the four income quartiles, though the lowest income quartile does seem to be somewhat less affected by merit aid eligibility than the upper three quartiles, despite the fact that students are equally likely to benefit from merit aid grants in their final year of school across the income distribution. This may occur 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.

Some evidence in this direction can be seen in columns 5 and 6 of this table, examining the effect of qualifying for merit aid on the cumulative amount of federal Pell grants that each individual has benefitted from over their undergraduate career. While 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, 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.

6.2.2 Effects by family income one year after graduation

Students from lower income families are also differentially affected by merit aid eligibility on outcomes realized at graduation, shown in table 19. 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. Students from the lower half of the family income distribution are more likely to be dissuaded from a STEM major if they qualify for a merit aid scholarship and more likely to graduate with honors, though these result is inconsistent across the different specifications and so should be taken with a grain of salt.

Turning attention to the results on outcomes one year after graduation with respect to family income in table 20, we see in the regression discontinuity results in column 5 the exact pattern that we would expect to see: that the effect of loans diminishes as we move up the family income distribution. 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 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. Similar patters are evident in the results on whether students are living in their home state after graduation. The results for whether students are unemployed are also consistent with credit or liquidity constraints, in that students from the lowest income families are the least affected by merit aid in their employment status after graduation, implying that they may yet be constrained even after benefitting from the merit aid scholarship.

6.2.3 Effects by family income four years after graduation

There are also some differences in the results four years after graduation by family income, as can be seen in table 21. Individuals from the lowest family income quartiles have held the most jobs as a result of qualifying for merit aid, which could again be symptomatic of credit constraints being relieved for those individuals. Since students from the lowest income families would have the least ability to smooth income shocks, we would expect to see the largest reaction from these individuals when credit constraints are relieved by qualifying for a scholarship program. Occupational outcomes vary somewhat along family income lines as well, though the effects are not statistically significantly different from one another. Individuals from the highest income families are least affected in their propensity to go into business occupations, while individuals from the lowest income families are least affected by merit aid eligibility in their propensity to go into education occupations. The effect on education seems to be mainly composed of graduates from the middle 50% of the family income distribution. Interestingly, students from low income families who benefit from merit aid programs are more likely to go into production occupations.

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 initial labor market outcomes. Graduates with a higher proportion of loans seek out jobs that have greater financial compensation, whereas graduates with a higher proportion 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. 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 seems to be the case that relying heavily on student loans for college financing could be distorting students away from starting careers in public service. 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. These results demonstrate that their findings are generalizable to a wider population of students, and in fact that the results they found may have been muted as a result of the population they were studying.

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 the student still has to make payments on their loans while they are working. The results of this paper suggest that the spirit of this program is correct, that students with loans need greater incentives to enter public service careers if the existing balance is to be maintained, but that this program takes 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 we would expect. Her behavioral results suggest that having loans while in school change the way that students view the job opportunities after college. While her study considers a highly select group of law students, my results suggest that similar effects may be at work in this 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 only 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 that are 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 ways to address the effects of student loans, a very particular kind of debt and investment, on those students who may not realize the full potential of the investment they have made. 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 used 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 remedy this weakness of the paper, in future research I intend to exploit the large expansion of income based repayment by presidential decree in 2009. The announcement of this expansion occurred right as the cohort of graduates examined in this paper were leaving college, after all decisions about major were made, but potentially before their labor market outcomes were set in stone. Thus will give me variation in loan repayment (rather than in loans themselves) that I can use 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. Whether loans or grants in the form of merit aid are chosen as a funding mechanism, the occupational and income decisions of the affected graduates may be changed as a result. These unanticipated consequences highlighted in existing programs should be taken into account as new programs and reforms to old programs are being considered.

A Gender Heterogeneity

For completeness, I examine the heterogeneity of the results with respect to gender in the same way as I examined heterogeneity with respect to family income: by subdividing the indicator for qualifying for merit aid into a term for men and a term for women. The results for outcomes at graduation are shown in table 22. One interesting note is that because of the rising proportion of women in higher education, my sample is predominately female, so there is a larger support for the estimates of the treatment response for women than the treatment response for men, which may explain some of the lack of precision in the results for men. I find that women are more responsive to merit aid programs in the amount of debt they choose to not take on, and that they are about twice as likely to shift to graduating from a public school in their home state as the men in the sample, in the difference in difference estimates. Women are also somewhat more likely to maintain eligibility for the merit aid scholarships through their final undergraduate year. Women are also somewhat more responsive to the scholarship in terms of the major with which they complete their degree. They are both more likely to switch out of STEM and education majors, though equally likely to switch into a humanities major as their male counterparts.

Despite the fact that the effect on college funding seems to be stronger with women, the effect on salary one year after graduation (shown in panel A of table 23) is concentrated on men. Women are far more likely to have responded to the merit aid scholarships by four years after graduation by completing or being enrolled in a post-baccalaureate degree. The effects on salary four years after graduation remain mixed and very imprecise when dividing the sample by gender. Although the effects on salary are concentrated on men, the occupational differences four years after graduation generated by merit aid program eligibility are roughly similar. Men and women are equally less likely to have a business occupations, and roughly equally more likely to have an occupation in education. The point estimates imply that men are perhaps more responsive to switching out of business and slightly less responsive to switching into education, but the estimates are not statistically distinguishable from each other.

B Robustness

B.1 Different RD specifications

There is always a question in regression discontinuity analyses of what the appropriate bandwidth is for analyzing the data. In the analysis above I have restricted the bandwidth to a window of ten points around the cutoff score in each state with the local linear estimator varying with distance to the cutoff point. This choice was motivated by evaluating the different bandwidth options by the methods outlined in Lee and Lemieux (2010), then by examining the robustness of the results to a choice of any bandwidth between one and ten of the cutoff scores. As can be seen in figure 10 and in figure 11, as examples of this analysis, the point estimates are remarkably similar to the selection of any bandwidth from one (essentially taking the difference of means at the cutoff) to a bandwidth of 10, incorporating virtually all of the data from each state. The preferred specifications reported in the tables in the body of the paper are highlighted with a bold dot. Both of these plots use the specification that includes controls for baseline characteristics, and the R^2 for each specification is plotted above each point estimate. Both figures show a similar pattern: that widening or shrinking the bandwidth does not appreciably change the estimates calculated in the main specifications. In fact, the estimates are statistically indistinguishable from being constant at the 95% confidence interval plotted on the graphs.

I have also conducted a regression discontinuity analysis of the baseline characteristics of the individuals, and the results in table 3 are consistent with students being randomly assigned around the cutoff points. Only one characteristic (proportion white) breaks in the RD specifications, while only two break at the 95% confidence level in the difference in difference estimations. While this does pose a slight issue for the interpretation of the results, it is also indicative of the type of individual that merit aid programs may encourage to persist to graduation. To the extent that there is a lower proportion of white individuals to the right of the cutoff scores, there is a higher proportion of minorities, indicating that merit aid programs may encourage persistence and graduation of minorities. Taking the other characteristics that potentially show evidence of breaks as a package, this analysis serves as suggestive evidence that merit aid programs encourage graduate of public-school educated minorities from two-parent households in which the mother has less than a bachelor's degree education. This will provide an interesting starting point for future research on the effects of merit aid programs and other sources of college funding on measures of college persistence, performance and graduation.

B.2 Different DD specifications

The results are robust to different compositions of the comparison groups. We might suppose that states with merit aid programs that aren't being used as program states (states in light blue/grey in figure 1) do not provide a valid counterfactual since they have programs of their own that are not included in this analysis. The results are robust to excluding those states (Georgia, South Carolina, Nevada, New Mexico, Wisconsin and Oklahoma) as comparison states and restricting attention to students in only the remaining 44 states.

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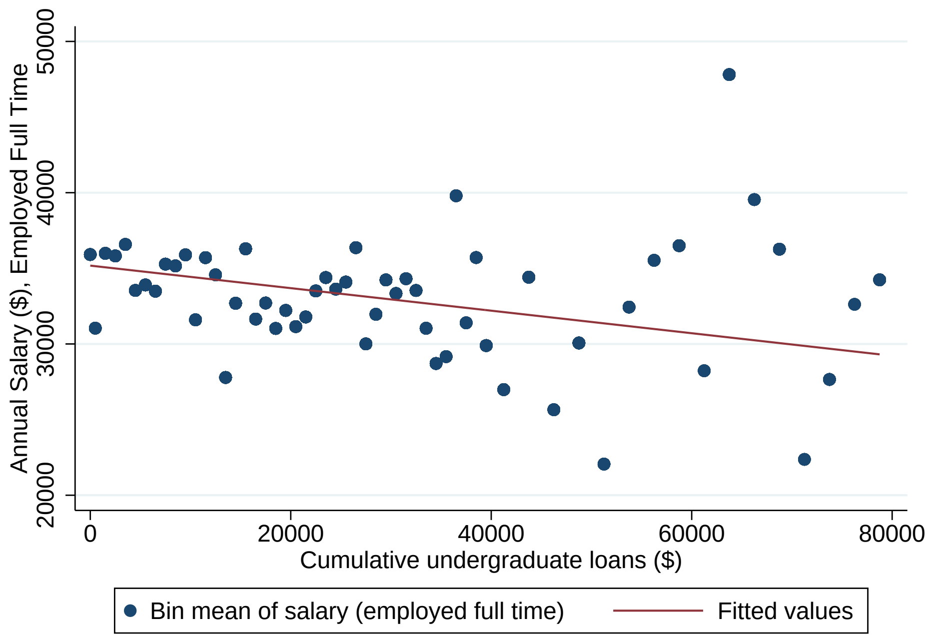
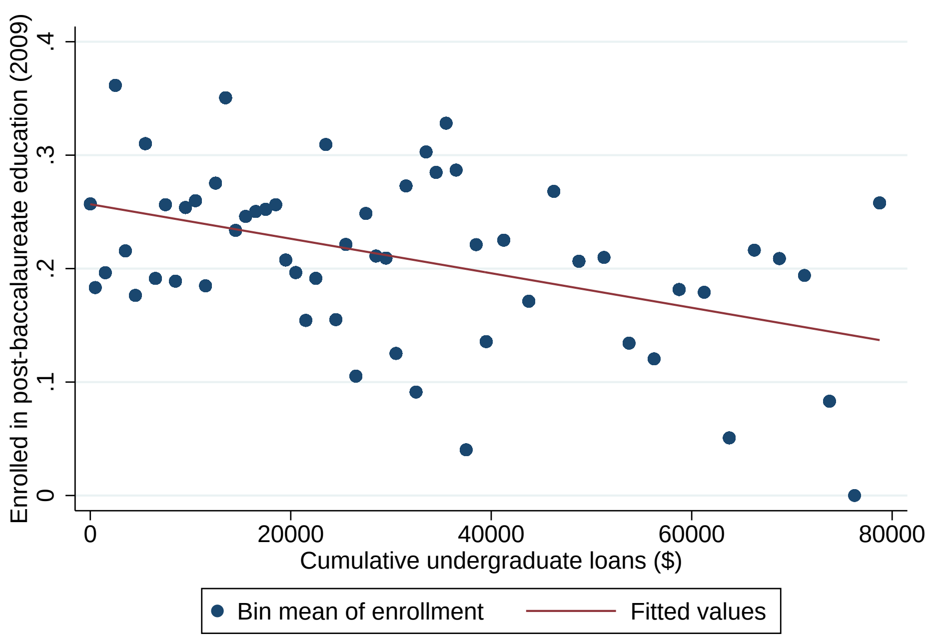
Figure 1: Loans regressed on salary of full time employed graduates



Means calculated in bins of $1000 up to $40,000, and in bins of $2500 up to $80,000

Figure 2: Loans regressed on post-baccalaureate enrollment status of graduates



Means calculated in bins of $1000 up to $40,000, and in bins of $2500 up to $80,000

Figure 3: States with merit aid programs and their ACT score cutoffs

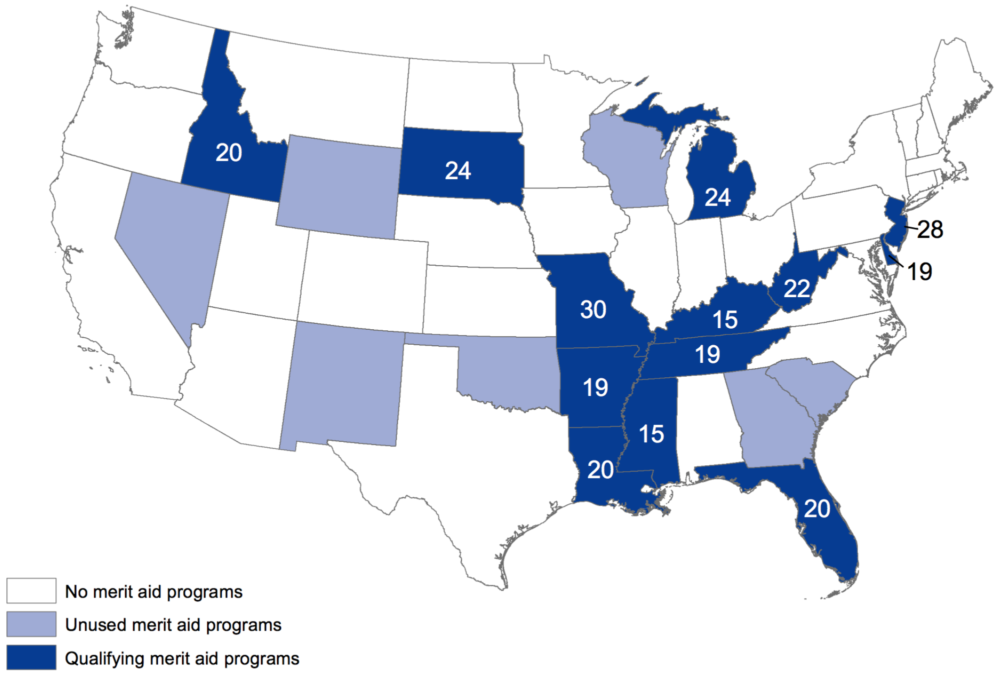


Figure 4: Program Award Size by Cutoff Score

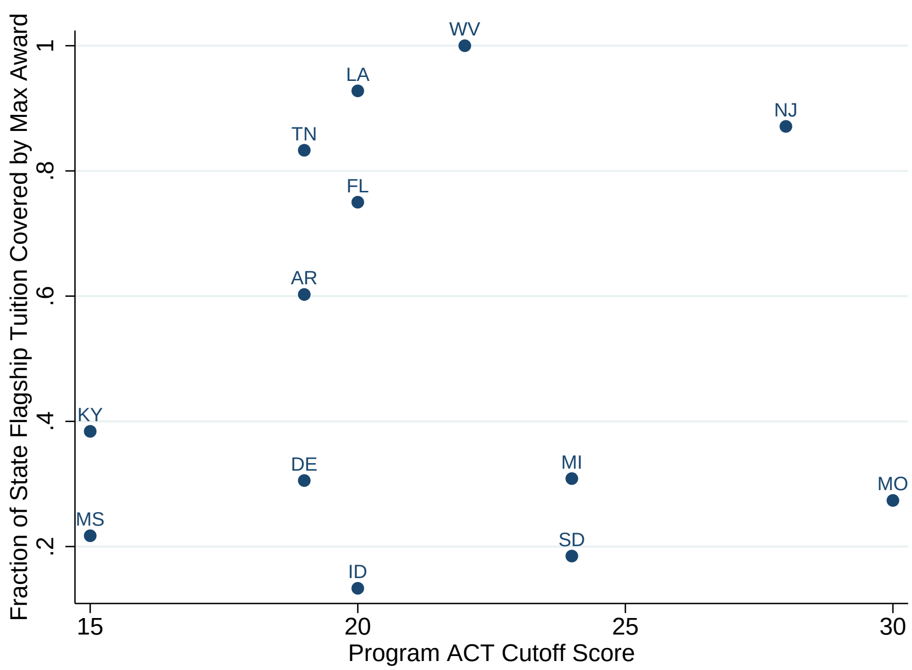


Figure 5: Program Scope by Award Size

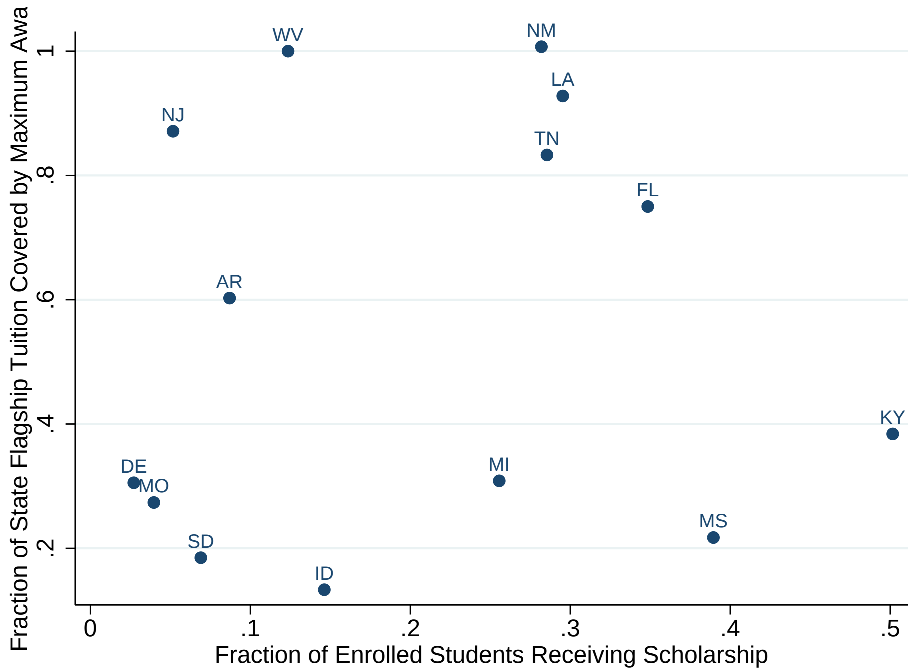


Figure 6: Centered ACT Score Histogram

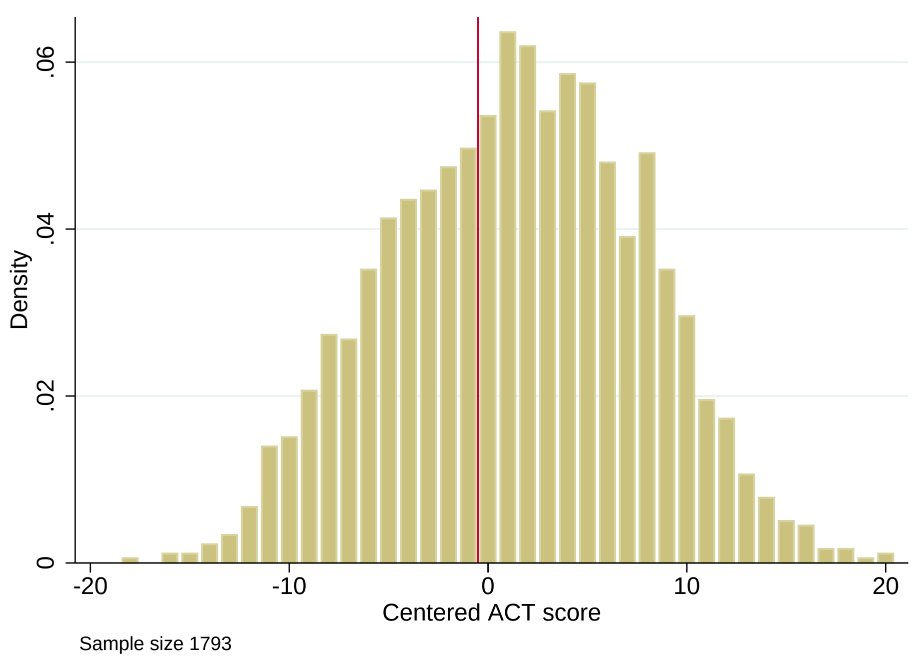


Figure 7: Amount of state merit aid by ACT Score (final year only)

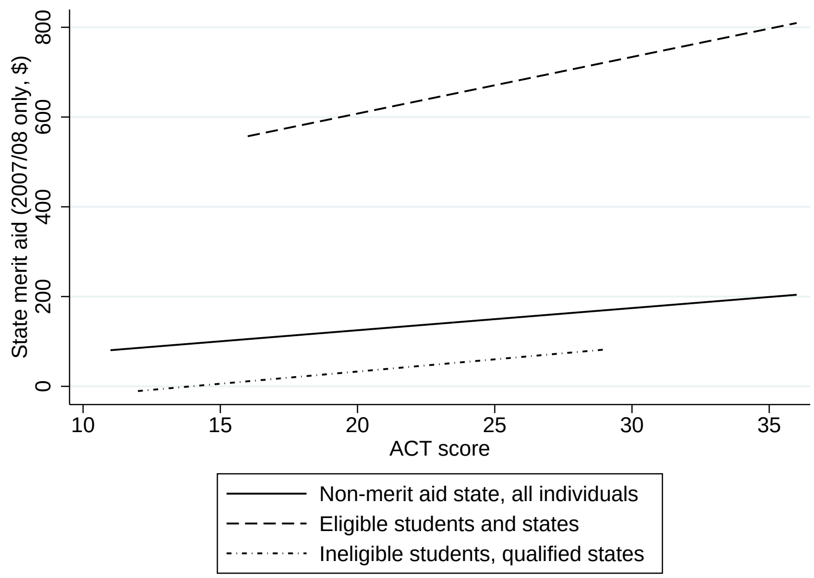


Figure 8: Total undergraduate loans by centered ACT score

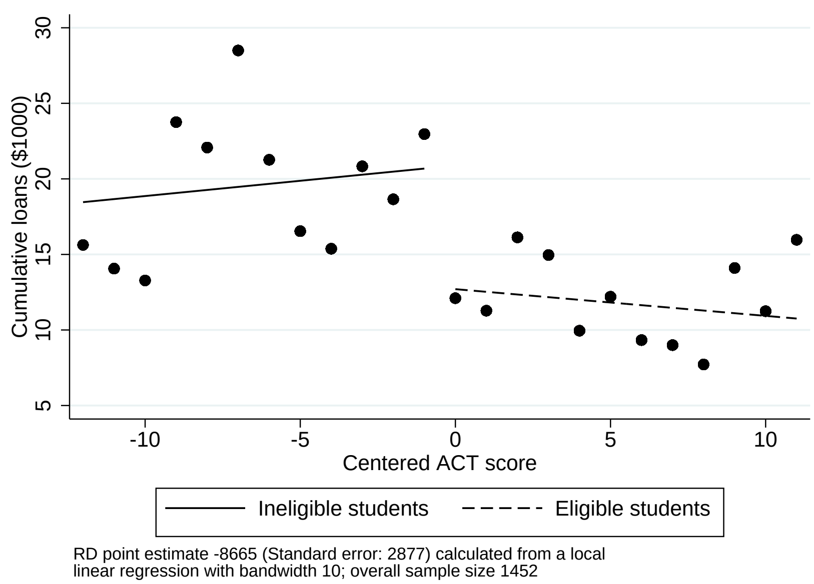


Figure 9: Salary by ACT score and treatment status

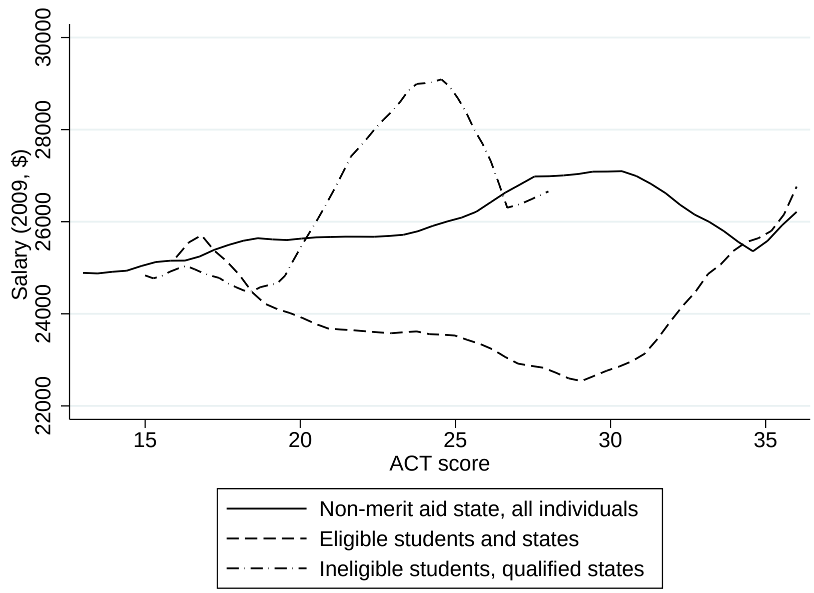


Figure 10: Robustness to bandwidth selection

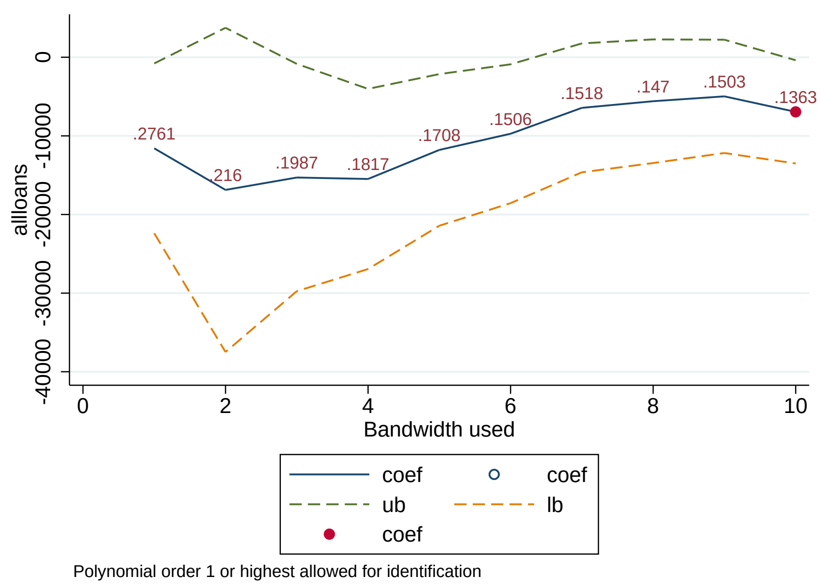


Figure 11: Robustness to bandwidth selection

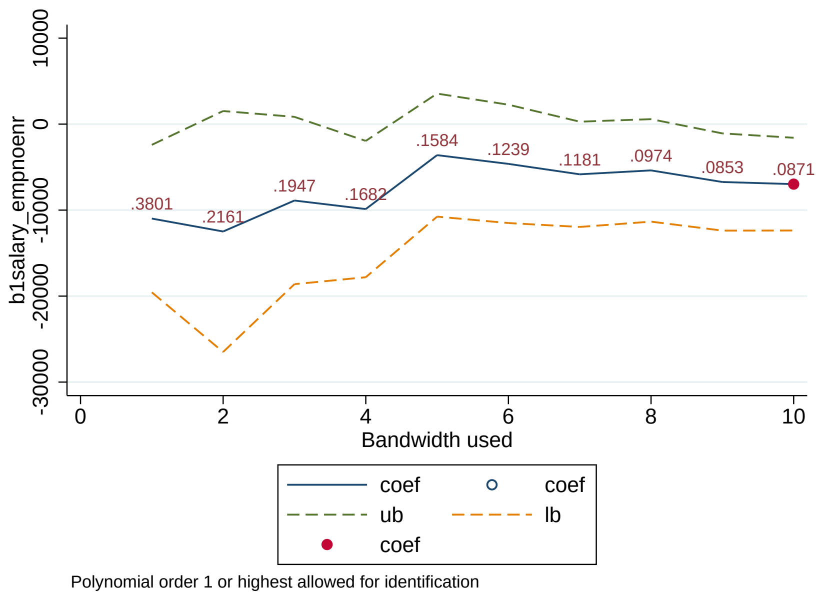


Table 1: Merit Aid Programs

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 merit aid programs		
		All students	Non-qualifying students	Qualifying students
Baseline characteristics				
Female	0.589	0.570	0.596	0.571
	(0.009)	(0.018)	(0.031)	(0.025)
White	0.816	0.825	0.822	0.822
	(0.007)	(0.014)	(0.024)	(0.020)
Family income (2006)	101279	92818	87418	92926
	(1285)	(2236)	(3984)	(2671)
Outcomes at graduation (2007/08)				
Cumulative undergraduate loans	14892	14663	20529	11696
	(319)	(714)	(1497)	(815)
Any undergraduate loans	0.619	0.620	0.727	0.559
	(0.009)	(0.018)	(0.028)	(0.025)
Graduate of public, in-state school	0.538	0.612	0.575	0.655
	(0.009)	(0.018)	(0.031)	(0.023)
Cumulative undergraduate GPA	3.288	3.258	3.172	3.273
	(0.008)	(0.018)	(0.028)	(0.025)
Outcomes one year after graduation (2009)				
Employed full time (2009)	0.694	0.670	0.706	0.678
	(0.009)	(0.018)	(0.030)	(0.024)
Full time employed salary	36278	34419	34778	33872
	(372)	(695)	(1017)	(935)
Outcomes four years after graduation (2012)				
Degree or enrolled	0.387	0.400	0.308	0.440
	(0.010)	(0.020)	(0.032)	(0.027)
Annual salary, full time employed	47471	45821	45173	45876
	(595)	(1161)	(1776)	(1592)
Observations	7290	1670	570	880

[1] Standard errors in parentheses.
[2] Means and standard errors in columns 3 and 4 calculated within ten points of the ACT cutoff scores in each state, the same bandwidth used in the regression discontinuity specifications in the rest of the analysis in this paper.
[3] All 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 as represented above. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered by ACT score - state group.

[3] RD regressions based on a bandwidth of 10

[4] All observation counts rounded to the nearest 10 for data security purposes

Table 4: Funding changes as a result of merit aid

***Panel A**: Cumulative undergraduate funding variables*	RD	RD	DD	DD
	(1)	(2)	(3)	(4)
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)
Observations	1450	1440	8970	8880
ACT Controls	Linear	Linear	FE	FE
State FE	X	X	X	X
Exogenous controls		X		X

***Panel B**: Final undergraduate year funding variables (2007/08)*	RD	DD	RD	DD
	(1)	(2)	(3)	(4)
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 as represented above. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered by ACT score - state group

[3] 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

[4] All observation counts rounded to the nearest 10 for data security purposes

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

	RD (1)	RD (2)	DD (3)	DD (4)
Graduate of public, in-state school	0.127** (0.062)	0.129** (0.064)	0.100** (0.040)	0.084** (0.040)
In-state graduate	0.056 (0.055)	0.060 (0.058)	0.043 (0.034)	0.026 (0.035)
Public school graduate	0.094 (0.058)	0.091 (0.058)	0.081** (0.039)	0.067* (0.038)
Months to graduation	0.688 (1.441)	0.275 (1.427)	-0.241 (0.810)	-0.340 (0.780)
Undergraduate GPA	-0.041 (0.055)	-0.032 (0.052)	-0.026 (0.035)	-0.027 (0.035)
Honors graduate	-0.028 (0.041)	-0.026 (0.044)	-0.008 (0.030)	-0.014 (0.030)
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 as represented above. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered by ACT score - state group.

[3] 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

[4] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, indicators for majors in: STEM, liberal arts, applied majors, business, education

[5] All observations rounded to the nearest 10 for data security purposes

Table 6: Differences in employment patterns as a result of merit aid (2009)

	RD (1)	RD (2)	RD (3)	DD (4)	DD (5)	DD (6)
Employed full time	-0.026	-0.013	0.007	-0.004	-0.006	0.005
	(0.058)	(0.058)	(0.061)	(0.043)	(0.042)	(0.042)
Employed part time	-0.054	-0.066	-0.074	-0.029	-0.028	-0.032
	(0.055)	(0.055)	(0.056)	(0.038)	(0.038)	(0.038)
Unemployed	0.096**	0.094**	0.083*	0.043	0.042	0.039
	(0.045)	(0.045)	(0.045)	(0.028)	(0.027)	(0.027)
Out of the labor force	-0.015	-0.016	-0.017	-0.010	-0.008	-0.011
	(0.042)	(0.042)	(0.039)	(0.026)	(0.026)	(0.026)
Enrolled only	-0.006	-0.009	-0.007	-0.009	-0.008	-0.011
	(0.033)	(0.033)	(0.032)	(0.022)	(0.022)	(0.022)
Resident of home state	-0.018	-0.003	-0.053	0.067	0.057	0.044
	(0.071)	(0.072)	(0.070)	(0.046)	(0.046)	(0.042)
Observations	1330	1320	1320	8330	8250	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 as represented above. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered by ACT score - state group.

[3] 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

[4] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, indicators for majors in: STEM, liberal arts, applied majors, business, education

[5] All observations rounded to the nearest 10 for data security purposes

Table 7: Changes in salary as result of merit aid (2009)

	RD (1)	RD (2)	RD (3)	DD (4)	DD (5)	DD (6)
Annualized salary	-5279** (2251)	-5253** (2242)	-4302** (2183)	-2666 (1889)	-2868 (1833)	-2285 (1775)
Employed	-4963** (2262)	-5142** (2295)	-4469** (2200)	-2558 (1855)	-2791 (1790)	-2389 (1691)
Employed, not enrolled	-6638*** (2242)	-7070*** (2357)	-6207*** (2338)	-4106* (2144)	-4512** (2061)	-4262** (1918)
Full time employed	-4789** (1933)	-5492*** (1908)	-4339** (1920)	-3339* (1732)	-3727** (1639)	-3979*** (1522)
Full time employed, not enrolled	-4901** (2088)	-5471*** (2064)	-4454** (2138)	-3175* (1877)	-3669** (1756)	-4084** (1626)
Logged annualized salary	-0.146* (0.076)	-0.119 (0.079)	-0.123 (0.090)	-0.115 (0.071)	-0.122* (0.070)	-0.108 (0.073)
Logged full time annualized salary	-0.153** (0.066)	-0.162** (0.067)	-0.134* (0.073)	-0.127** (0.061)	-0.134** (0.060)	-0.136** (0.059)
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 as represented above. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered by ACT score - state group.

[3] 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

[4] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, indicators for majors in: STEM, liberal arts, applied majors, business, education

[5] All observations rounded to the nearest 10 for data security purposes

Table 8: Differences in occupational choice as a result of merit aid (2009)

	RD (1)	RD (2)	RD (3)	DD (4)	DD (5)	DD (6)
Business	0.061	0.038	0.035	0.004	-0.005	-0.006
	(0.059)	(0.056)	(0.050)	(0.042)	(0.041)	(0.037)
STEM	-0.032	-0.048	-0.033	-0.035	-0.039	-0.053*
	(0.038)	(0.041)	(0.040)	(0.033)	(0.033)	(0.031)
Education	-0.053	-0.044	-0.031	-0.020	-0.019	0.017
	(0.062)	(0.064)	(0.053)	(0.038)	(0.039)	(0.038)
Entertainment	-0.012	-0.012	-0.017	0.039*	0.044**	0.046**
	(0.025)	(0.028)	(0.030)	(0.022)	(0.022)	(0.021)
Health	-0.021	-0.032	-0.021	-0.043*	-0.042*	-0.043**
	(0.030)	(0.032)	(0.032)	(0.022)	(0.022)	(0.021)
Sales	-0.016	-0.009	-0.014	-0.002	0.002	-0.004
	(0.041)	(0.044)	(0.043)	(0.033)	(0.033)	(0.032)
Office	0.062	0.097**	0.083**	0.028	0.035	0.026
	(0.047)	(0.042)	(0.041)	(0.029)	(0.027)	(0.028)
Production	0.011	0.009	-0.003	0.028	0.025	0.018
	(0.071)	(0.070)	(0.069)	(0.041)	(0.040)	(0.039)
Observations	1070	1060	10620	6750	6690	6690
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 as represented above. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered by ACT score - state group.

[3] 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

[4] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, indicators for majors in: STEM, liberal arts, applied majors, business, education

[5] All observations rounded to the nearest 10 for data security purposes

Table 9: Changes in employment outcomes due to merit aid (2012)

	RD (1)	RD (2)	RD (3)	DD (4)	DD (5)	DD (6)
Degree or enrolled	0.126* (0.071)	0.111 (0.069)	0.108 (0.071)	0.044 (0.045)	0.041 (0.045)	0.036 (0.044)
Annual salary, employed	1142 (4830)	452 (4559)	1585 (4444)	-2054 (3302)	-1999 (3197)	-1179 (3124)
Annual salary, full time employed	-156 (4913)	-1236 (4481)	-32 (4449)	-2077 (3375)	-1987 (3231)	-1228 (3223)
Logged annual salary, employed	-0.019 (0.111)	-0.025 (0.109)	0.007 (0.105)	-0.067 (0.076)	-0.071 (0.075)	-0.053 (0.073)
Logged annual salary, full time employed	-0.033 (0.098)	-0.049 (0.091)	-0.019 (0.090)	-0.046 (0.068)	-0.050 (0.065)	-0.042 (0.065)
Total jobs since graduation	0.224 (0.230)	0.215 (0.229)	0.164 (0.234)	0.212 (0.139)	0.210 (0.140)	0.189 (0.141)
Observations	850-1210	840-1190	840-1190	5020-7550	4980-7480	4980-7480
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 as represented above. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered by ACT score - state group.

[3] 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

[4] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, indicators for majors in: STEM, liberal arts, applied majors, business, education

[5] All observations rounded to the nearest 10 for data security purposes

Table 10: Changes in occupational choice due to merit aid (2012)

	RD (1)	RD (2)	RD (3)	DD (4)	DD (5)	DD (6)
Business	-0.184**	-0.178**	-0.186***	-0.111**	-0.109**	-0.117***
	(0.080)	(0.079)	(0.069)	(0.046)	(0.045)	(0.042)
STEM	0.022	0.016	0.040	-0.003	-0.005	-0.003
	(0.042)	(0.044)	(0.042)	(0.029)	(0.029)	(0.031)
Education	0.123**	0.125**	0.103**	0.065*	0.066*	0.083**
	(0.058)	(0.056)	(0.043)	(0.039)	(0.040)	(0.035)
Entertainment	-0.012	-0.016	-0.010	0.018	0.021	0.020
	(0.033)	(0.035)	(0.036)	(0.025)	(0.025)	(0.024)
Health	-0.029	-0.034	-0.005	-0.032	-0.028	-0.025
	(0.041)	(0.041)	(0.040)	(0.025)	(0.025)	(0.026)
Sales	-0.031	-0.035	-0.031	0.021	0.018	0.018
	(0.022)	(0.027)	(0.029)	(0.022)	(0.023)	(0.023)
Office	0.122*	0.122**	0.092	0.029	0.022	0.014
	(0.064)	(0.061)	(0.062)	(0.033)	(0.032)	(0.032)
Production	-0.010	-0.001	-0.002	0.014	0.016	0.011
	(0.061)	(0.059)	(0.054)	(0.035)	(0.034)	(0.034)
Observations	1100	1090	1090	6810	6750	6750
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 as represented above. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered by ACT score - state group.

[3] 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

[4] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, indicators for majors in: STEM, liberal arts, applied majors, business, education

[5] All observations rounded to the nearest 10 for data security purposes

Table 11: Changes in salary due to student loans (2009)

	RD	RD	RD	DD	DD	DD
	(1)	(2)	(3)	(4)	(5)	(6)
Annualized salary	640	631	562	411	416	370
	(441)	(411)	(417)	(315)	(293)	(312)
Employed	679	642	600	415	429	414
	(494)	(425)	(427)	(352)	(331)	(358)
Employed, not enrolled	768	747*	705*	614	636*	663
	(495)	(432)	(424)	(404)	(378)	(407)
Full time employed	1244	1070	944	515	540*	638*
	(1230)	(798)	(769)	(343)	(318)	(363)
Full time employed, not enrolled	1230	1020	945	492	546	660
	(1292)	(808)	(827)	(378)	(363)	(410)
Logged annual salary	0.025	0.018	0.022	0.016	0.016	0.016
	(0.025)	(0.018)	(0.022)	(0.013)	(0.012)	(0.013)
Logged full time annual salary	0.041	0.031	0.028	0.021	0.021*	0.023*
	(0.042)	(0.024)	(0.024)	(0.014)	(0.012)	(0.014)
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 as represented above. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered by ACT score - state group.

[3] 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

[4] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, indicators for majors in: STEM, liberal arts, applied majors, business, education

[5] All observations rounded to the nearest 10 for data security purposes

Table 12: Changes in occupational choice due to student loans (2009)

	RD (1)	RD (2)	RD (3)	DD (4)	DD (5)	DD (6)
Business	-0.011	-0.006	-0.006	-0.001	0.001	0.001
	(0.012)	(0.009)	(0.010)	(0.005)	(0.005)	(0.006)
STEM	0.006	0.007	0.006	0.005	0.005	0.008
	(0.008)	(0.007)	(0.007)	(0.005)	(0.004)	(0.005)
Education	0.009	0.007	0.006	0.003	0.002	-0.002
	(0.014)	(0.011)	(0.011)	(0.006)	(0.006)	(0.006)
Entertainment	0.002	0.002	0.003	-0.005	-0.006	-0.007
	(0.005)	(0.004)	(0.006)	(0.004)	(0.004)	(0.004)
Health	0.004	0.005	0.004	0.006*	0.006*	0.006
	(0.007)	(0.006)	(0.007)	(0.004)	(0.003)	(0.004)
Sales	0.003	0.001	0.002	0.000	-0.000	0.001
	(0.009)	(0.008)	(0.009)	(0.005)	(0.005)	(0.005)
Office	-0.011	-0.015	-0.015	-0.004	-0.005	-0.004
	(0.011)	(0.011)	(0.011)	(0.004)	(0.004)	(0.005)
Production	-0.002	-0.001	0.000	-0.004	-0.003	-0.003
	(0.014)	(0.012)	(0.013)	(0.006)	(0.006)	(0.006)
Observations	1070	1060	1060	6750	6690	6690
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 as represented above. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered by ACT score - state group.

[3] 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

[4] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, indicators for majors in: STEM, liberal arts, applied majors, business, education

[5] All observations rounded to the nearest 10 for data security purposes

Table 13: Changes in employment outcomes due to student loans (2012)

	RD (1)	RD (2)	RD (3)	DD (4)	DD (5)	DD (6)
Degree or enrolled	-0.018 (0.015)	-0.016 (0.014)	-0.016 (0.014)	-0.007 (0.009)	-0.006 (0.008)	-0.006 (0.009)
Annual salary, employed	-206 (892)	-77 (786)	-260 (741)	318 (489)	302 (453)	184 (452)
Annual salary, full time employed	26 (809)	194 (711)	4 (631)	338 (539)	313 (489)	190 (471)
Logged annual salary, employed	0.004 (0.024)	0.005 (0.021)	-0.001 (0.020)	0.010 (0.012)	0.011 (0.011)	0.008 (0.012)
Logged annual salary, full time employed	0.007 (0.021)	0.009 (0.018)	0.003 (0.016)	0.008 (0.012)	0.008 (0.011)	0.007 (0.011)
Total jobs since graduation	-0.033 (0.038)	-0.031 (0.037)	-0.025 (0.036)	-0.036 (0.027)	-0.033 (0.024)	-0.033 (0.026)
Observations	850-1210	840-1190	840-1190	5020-7550	4980-7480	4980-7480
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 as represented above. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered by ACT score - state group.

[3] 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

[4] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, indicators for majors in: STEM, liberal arts, applied majors, business, education

[5] All observations rounded to the nearest 10 for data security purposes

Table 14: Changes in occupational choice due to student loans (2012)

	RD (1)	RD (2)	RD (3)	DD (4)	DD (5)	DD (6)
Business	0.034	0.032	0.034	0.019*	0.017*	0.019*
	(0.029)	(0.027)	(0.026)	(0.011)	(0.010)	(0.010)
STEM	-0.004	-0.003	-0.007	0.001	0.001	0.000
	(0.008)	(0.008)	(0.010)	(0.006)	(0.005)	(0.006)
Education	-0.022	-0.023	-0.019	-0.011	-0.010	-0.014*
	(0.019)	(0.018)	(0.015)	(0.008)	(0.007)	(0.008)
Entertainment	0.002	0.003	0.002	-0.003	-0.003	-0.003
	(0.007)	(0.007)	(0.007)	(0.005)	(0.005)	(0.005)
Health	0.005	0.006	0.001	0.005	0.004	0.004
	(0.009)	(0.009)	(0.008)	(0.005)	(0.005)	(0.005)
Sales	0.006	0.006	0.006	-0.004	-0.003	-0.003
	(0.007)	(0.008)	(0.007)	(0.004)	(0.004)	(0.004)
Office	-0.022	-0.022	-0.017	-0.005	-0.003	-0.002
	(0.018)	(0.017)	(0.014)	(0.007)	(0.006)	(0.006)
Production	0.002	0.000	0.000	-0.002	-0.002	-0.002
	(0.010)	(0.010)	(0.010)	(0.006)	(0.005)	(0.006)
Observations	1100	1090	1090	6810	6750	6750
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 as represented above. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered by ACT score - state group.

[3] 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

[4] Endogenous controls: public in state school indicator, months to graduation, undergraduate GPA, honors graduate, indicators for majors in: STEM, liberal arts, applied majors, business, education

[5] All observations rounded to the nearest 10 for data security purposes

Table 15: Ability heterogeneity results, outcomes at graduation

***Panel A**: Funding and graduation outcomes*

	Cumulative undergraduate loans		Graduate of public in-state school		Months to graduation	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
Qualified	-7522**	-4714**	0.085	0.012	-1.405	-2.714***
	(3286)	(2137)	(0.065)	(0.052)	(1.477)	(0.950)
Qualified at low cutoff	418	-84	0.117	0.135	-0.252	0.892
	(3373)	(2938)	(0.118)	(0.088)	(1.695)	(1.424)
Qualified at high cutoff	-3369	-2445	-0.023	0.059	5.690***	5.180***
	(4693)	(3502)	(0.089)	(0.081)	(1.907)	(1.446)
Observations	1440	8880	1440	8880	1440	8880
R-squared	0.157	0.101	0.203	0.182	0.146	0.110

***Panel B**: Major choice outcomes*

	STEM		Humanities		Education	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
Qualified	-0.010	0.057	0.315***	0.083	-0.122***	-0.083**
	(0.047)	(0.041)	(0.070)	(0.069)	(0.044)	(0.038)
Qualified at low cutoff	-0.047	-0.076	-0.280***	-0.057	0.287***	0.096*
	(0.060)	(0.060)	(0.092)	(0.086)	(0.068)	(0.054)
Qualified at high cutoff	-0.191***	-0.080	0.015	-0.098	0.045	0.034
	(0.072)	(0.064)	(0.088)	(0.093)	(0.046)	(0.045)
Observations	1440	8880	1440	8880	1440	8880
R-squared	0.148	0.102	0.123	0.061	0.123	0.059

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

[2] Each column of the table represents a single regression with an indicator for qualifying for a merit aid program, and interactions for qualifying in a state with a low score (15-20) or a high score (28-30).

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

[4] All regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects. The RD regressions are restricted to the subsample of students living in states with programs with sharp cutoffs, whereas the DD regressions encompass the full set of students in the sample

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

Table 16: Ability heterogeneity results, outcomes one year post graduation

Panel A: *Employment and time use outcomes*

	Home state resident		Employed part time		Enrolled only	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
Qualified	-0.152**	-0.073	-0.156**	-0.086*	0.039	0.049*
	(0.073)	(0.049)	(0.079)	(0.052)	(0.033)	(0.027)
Qualified at low cutoff	0.061	0.162*	0.127	0.032	-0.042	-0.077
	(0.115)	(0.089)	(0.098)	(0.086)	(0.053)	(0.053)
Qualified at high cutoff	0.192*	0.191**	0.219***	0.160**	-0.152**	-0.104***
	(0.102)	(0.076)	(0.080)	(0.074)	(0.067)	(0.036)
Observations	1320	8250	1320	8250	1320	8250
R-squared	0.326	0.337	0.120	0.038	0.138	0.072

Panel B: *Salary outcomes*

	Full time employed salary		Full time employed, not enrolled salary		Logged full time annual salary	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
Qualified	-1067	-637	-828	-1399	-0.006	0.082
	(2969)	(2401)	(3313)	(2564)	(0.100)	(0.081)
Qualified at low cutoff	-87	-2153	-467	-1506	-0.119	-0.224*
	(3509)	(3015)	(3938)	(3318)	(0.140)	(0.115)
Qualified at high cutoff	-6715	-4695	-3897	-3239	-0.215	-0.293**
	(4218)	(3653)	(4901)	(3814)	(0.201)	(0.143)
Observations	790	4860	680	4240	860	5390
R-squared	0.277	0.199	0.309	0.226	0.187	0.139

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

[2] Each column of the table represents a single regression with an indicator for qualifying for a merit aid program, and interactions for qualifying in a state with a low score (15-20) or a high score (28-30).

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

[4] All regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects. The RD regressions are restricted to the subsample of students living in states with programs with sharp cutoffs, whereas the DD regressions encompass the full set of students in the sample

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

Table 17: Ability heterogeneity results, outcomes four years post graduation

Panel A: *Employment outcomes*

	Has degree or enrolled		Annual salary, full time employed		Logged annual salary, full time employed	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
Qualified	0.219**	0.117*	7214*	2611	0.157	0.073
	(0.100)	(0.064)	(3866)	(3504)	(0.108)	(0.076)
Qualified at low cutoff	-0.183	-0.201*	-8113	-5457	-0.174	-0.130
	(0.152)	(0.115)	(9551)	(7331)	(0.163)	(0.131)
Qualified at high cutoff	0.038	-0.027	-13328	-9436	-0.250	-0.238*
	(0.143)	(0.082)	(8479)	(6636)	(0.181)	(0.138)
Observations	1190	7480	870	5200	840	4980
R-squared	0.131	0.129	0.271	0.185	0.214	0.148

Panel B: *Selected occupation choices*

	Business		STEM		Education	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
Qualified	-0.202**	-0.042	0.031	-0.024	0.135*	0.089*
	(0.095)	(0.069)	(0.058)	(0.035)	(0.069)	(0.050)
Qualified at low cutoff	-0.028	-0.143	0.025	0.097**	0.015	0.019
	(0.123)	(0.104)	(0.068)	(0.045)	(0.095)	(0.067)
Qualified at high cutoff	-0.027	-0.045	0.013	-0.009	-0.064	-0.066
	(0.107)	(0.082)	(0.090)	(0.065)	(0.105)	(0.081)
Observations	1090	6750	10920	6750	1090	6750
R-squared	0.191	0.129	0.226	0.222	0.321	0.223

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

[2] Each column of the table represents a single regression with an indicator for qualifying for a merit aid program, and interactions for qualifying in a state with a low score (15-20) or a high score (28-30).

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

[4] All regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects. The RD regressions are restricted to the subsample of students living in states with programs with sharp cutoffs, whereas the DD regressions encompass the full set of students in the sample

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

Table 18: Family income heterogeneity results, funding outcomes at graduation

	Cumulative undergraduate loans		Any state merit grant (2007/08)		Cumulative federal Pell grant amount		Work income while in school (2007/08)	
Family income	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
quartile	RD	DD	RD	DD	RD	DD	RD	DD
< 25%	-6594	-3997	0.157**	0.222***	2805***	2484***	-1462	-2242**
	(4185)	(2970)	(0.074)	(0.055)	(910)	(849)	(1122)	(924)
25% - 50%	-9718***	-7483***	0.153***	0.190***	-2268***	-2049***	337	-481
	(3093)	(2169)	(0.055)	(0.051)	(671)	(499)	(1242)	(940)
50% - 75%	-9052***	-6931***	0.165***	0.212***	-2629***	-2210***	-520	-1086
	(2654)	(1837)	(0.055)	(0.044)	(544)	(394)	(1056)	(721)
≤ 75%	-7774***	-5856***	0.133***	0.212***	-1096**	-561	22	-462
	(2944)	(1739)	(0.051)	(0.040)	(507)	(401)	(1099)	(756)
Observations	1440	8880	1440	8880	1440	8880	1440	8880
R-squared	0.158	0.101	0.311	0.341	0.490	0.331	0.096	0.056

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

[2] 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] Each column of the table represents a single regression with separate treatment indicators for students in states with the various cutoff scores noted on the far left. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered at the state by score level.

[4] All regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects. The RD regressions are restricted to the subsample of students living in states with programs with sharp cutoffs, whereas the DD regressions encompass the full set of students in the sample.

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

Table 19: Family income heterogeneity results, graduation outcomes

	Graduate of public in-state school		Honors graduate		STEM major		Humanities major	
Family income	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
quartile	RD	DD	RD	DD	RD	DD	RD	DD
$<$ 25%	0.002	-0.035	0.100*	0.018	-0.153***	-0.013	0.187***	-0.016
	(0.088)	(0.070)	(0.060)	(0.048)	(0.058)	(0.048)	(0.071)	(0.057)
25% - 50%	0.133*	0.075	0.092*	0.037	-0.129***	-0.050	0.338***	0.163***
	(0.077)	(0.061)	(0.054)	(0.048)	(0.048)	(0.040)	(0.075)	(0.061)
50% - 75%	0.170**	0.122**	0.093*	0.040	-0.050	0.016	0.192***	0.045
	(0.066)	(0.048)	(0.051)	(0.042)	(0.043)	(0.042)	(0.068)	(0.051)
$\leq$ 75%	0.128*	0.089*	0.053	0.008	0.005	0.042	0.217***	0.048
	(0.068)	(0.049)	(0.051)	(0.041)	(0.058)	(0.040)	(0.070)	(0.050)
Observations	1440	8880	1440	8880	1440	8880	1440	8880
R-squared	0.208	0.183	0.288	0.172	0.152	0.102	0.124	0.063

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

[2] 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] Each column of the table represents a single regression with separate treatment indicators for students in states with the various cutoff scores noted on the far left. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered at the state by score level.

[4] All regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects. The RD regressions are restricted to the subsample of students living in states with programs with sharp cutoffs, whereas the DD regressions encompass the full set of students in the sample.

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

Table 20: Family income heterogeneity results, outcomes one year post graduation

	Resident of home state		Unemployed		Full time employed salary	
Family income quartile	(1) RD	(2) DD	(3) RD	(4) DD	(5) RD	(6) DD
< 25%	-0.151*	0.028	0.003	-0.013	-4425*	-4696**
	(0.080)	(0.064)	(0.059)	(0.038)	(2541)	(2380)
25% - 50%	-0.108	0.032	0.091	0.055	-4103	-4546**
	(0.078)	(0.056)	(0.057)	(0.049)	(2671)	(2176)
50% - 75%	-0.092	0.028	0.099	0.062*	-1245	-2962
	(0.082)	(0.054)	(0.061)	(0.033)	(2267)	(2110)
≤ 75%	-0.035	0.072	0.062	0.023	-320	-4532**
	(0.075)	(0.047)	(0.054)	(0.030)	(2903)	(1983)
Observations	1320	8250	1320	8250	790	4860
R-squared	0.325	0.336	0.092	0.038	0.279	0.200

Panel B*: Selected occupational outcomes*

	Business		STEM		Office	
Family income quartile	(1) RD	(2) DD	(3) RD	(4) DD	(5) RD	(6) DD
< 25%	0.078	-0.000	-0.068	-0.052	0.128*	0.112**
	(0.075)	(0.062)	(0.043)	(0.038)	(0.074)	(0.055)
25% - 50%	0.094	0.020	-0.067	-0.060*	0.015	-0.013
	(0.064)	(0.046)	(0.043)	(0.034)	(0.056)	(0.037)
50% - 75%	0.045	-0.039	-0.062	-0.051	0.091	0.047
	(0.057)	(0.042)	(0.046)	(0.042)	(0.057)	(0.035)
≤ 75%	0.117*	0.014	-0.079	-0.053	0.013	-0.005
	(0.069)	(0.049)	(0.056)	(0.041)	(0.059)	(0.034)
Observations	1060	6690	1060	6690	1060	6690
R-squared	0.242	0.164	0.354	0.304	0.121	0.061

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

[2] 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] Each column of the table represents a single regression with separate treatment indicators for students in states with the various cutoff scores noted on the far left. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered at the state by score level.

[4] All regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects. The RD regressions are restricted to the subsample of students living in states with programs with sharp cutoffs, whereas the DD regressions encompass the full set of students in the sample.

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

Table 21: Family income heterogeneity results, outcomes four years post graduation

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

	Has degree or enrolled		Annual salary, full time employed		Total jobs since graduation	
Family income quartile	(1) RD	(2) DD	(3) RD	(4) DD	(5) RD	(6) DD
< 25%	0.171*	0.035	-870	-3089	0.367	0.430**
	(0.097)	(0.081)	(5816)	(4742)	(0.305)	(0.200)
25% - 50%	0.143	0.055	530	-870	0.231	0.243
	(0.093)	(0.065)	(4837)	(3862)	(0.317)	(0.230)
50% - 75%	0.136	0.015	2912	-1583	-0.009	0.042
	(0.092)	(0.058)	(5342)	(3720)	(0.290)	(0.165)
≤ 75%	0.175*	0.049	6095	-417	0.210	0.240
	(0.103)	(0.059)	(5904)	(3993)	(0.312)	(0.170)
Observations	1190	7480	870	5200	1190	7480
R-squared	0.129	0.128	0.268	0.184	0.114	0.069

***Panel B**: Selected occupational outcomes*

	Business		Education		Production	
Family income quartile	(1) RD	(2) DD	(3) RD	(4) DD	(5) RD	(6) DD
< 25%	-0.229**	-0.109*	0.083	0.018	0.132*	0.114*
	(0.088)	(0.064)	(0.058)	(0.050)	(0.076)	(0.060)
25% - 50%	-0.217***	-0.143**	0.157**	0.126**	-0.017	-0.028
	(0.079)	(0.057)	(0.068)	(0.055)	(0.059)	(0.040)
50% - 75%	-0.264***	-0.164***	0.145***	0.101**	0.022	0.009
	(0.074)	(0.046)	(0.051)	(0.045)	(0.068)	(0.046)
≤ 75%	-0.137*	-0.043	0.124*	0.057	0.017	-0.003
	(0.080)	(0.054)	(0.066)	(0.046)	(0.058)	(0.038)
Observations	1090	6750	1090	6750	1090	6750
R-squared	0.199	0.130	0.322	0.224	0.145	0.062

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

[2] 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] Each column of the table represents a single regression with separate treatment indicators for students in states with the various cutoff scores noted on the far left. Standard errors are in parentheses and p-values in brackets. All standard errors are clustered at the state by score level.

[4] All regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects. The RD regressions are restricted to the subsample of students living in states with programs with sharp cutoffs, whereas the DD regressions encompass the full set of students in the sample.

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

Table 22: Gender heterogeneity results, in school outcomes

***Panel A**: Funding and graduation outcomes*						
	Cumulative undergraduate loans		Graduate of public in-state school		Any state merit grant (final year)	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
Male	-4669	-5119***	0.110	0.045	0.113**	0.173***
	(3784)	(1851)	(0.072)	(0.047)	(0.051)	(0.04)
Female	-10909***	-7459***	0.116*	0.120***	0.180***	0.242***
	(2633)	(1745)	(0.066)	(0.045)	(0.052)	(0.037)
Observations	1440	8880	1440	8880	1440	8880
R-squared	0.162	0.101	0.140	0.107	0.312	0.343

***Panel B**: Selected majors at graduation*						
	STEM		Humanities		Education	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
Male	-0.057	0.022	0.270***	0.066	0.023	-0.049**
	(0.054)	(0.039)	(0.070)	(0.048)	(0.042)	(0.024)
Female	-0.095**	-0.003	0.209***	0.056	-0.052	-0.075***
	(0.040)	(0.032)	(0.066)	(0.046)	(0.055)	(0.027)
Observations	1440	8880	1440	8880	1440	8880
R-squared	0.144	0.102	0.116	0.061	0.108	0.059

[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 states with the various cutoff scores noted on the far left. Standard errors are in parentheses and p-values in brackets. All standard errors are heteroskedasticity robust.

[3] All regressions include baseline characteristic controls, ACT score fixed effects and state fixed effects. The RD regressions are restricted to the subsample of students living in states with programs with sharp cutoffs, whereas the DD regressions encompass the full set of students in the sample

Table 23: Gender heterogeneity results, employment outcomes

***Panel A**: Employment and salary outcomes*

	Full time employed salary (2009)		Degree or enrolled (2012)		Full time employed salary (2012)	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
Male	-4623*	-4867**	0.055	0.005	-433	1034
	(2709)	(1984)	(0.090)	(0.052)	(6063)	(3975)
Female	-857	-3131*	0.208**	0.063	3993	-3231
	(2210)	(1651)	(0.083)	(0.049)	(4757)	(3342)
Observations	790	4860	1190	7480	870	5200
R-squared	0.277	0.200	0.133	0.128	0.266	0.185

***Panel B**: Selected occupational categories (2012)*

	Business		STEM		Education	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	DD	RD	DD	RD	DD
Male	-0.247***	-0.129***	0.032	-0.008	0.104*	0.085**
	(0.084)	(0.048)	(0.066)	(0.039)	(0.058)	(0.040)
Female	-0.199***	-0.105**	0.051	0.001	0.147***	0.081*
	(0.075)	(0.045)	(0.047)	(0.035)	(0.054)	(0.046)
Observations	1090	6750	1090	6750	1090	6750
R-squared	0.192	0.129	0.226	0.221	0.321	0.223

[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 states with the various cutoff scores noted on the far left. Standard errors are in parentheses and p-values in brackets. All standard errors are heteroskedasticity robust.

[3] All regressions include baseline characteristic controls, endogenous controls, ACT score fixed effects and state fixed effects. The RD regressions are restricted to the subsample of students living in states with programs with sharp cutoffs, whereas the DD regressions encompass the full set of students in the sample