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Higher Education Policies and Students' Behavior

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ABSTRACT

HIGHER EDUCATION POLICIES AND STUDENTS' BEHAVIOR

BY

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This dissertation examines the impact of need-based financing policies, performance standards, and public recognition on college students' outcomes over time. Each essay utilizes novel administrative student-level data from Jamaica and a quasi-experimental econometric design to identify the causal impact of these college-level interventions on students' behavior. The general objective of this work is to provide credible evidence that can help policymakers create more effective policies to improve student success.

Designing an effective framework for financing higher education is a major issue facing policymakers in developed and developing countries. While we have a good understanding of how college financing options affect college students' behavior in developed countries, less is known about the impact of these programs in developing countries. Chapter 1 examines the impact of need-based student loan and grant financing policies on students college and labor market outcomes in a developing country. The results indicate that the students affected by either program had a higher GPA, were more likely to remain in college beyond their third year, and graduated at a higher rate. While both programs induced treated students to reduce their labor market engagement during college, the estimates suggest that grant recipients earned more and loan recipients earned less than comparable students in the early years after expected graduation.

Chapter 2 examines how college initiatives that ascribe public recognition or written

reprimand to a set standard of academic performance impact students academic decision-making. Many colleges utilize programs such as a Dean's list and academic probation as mediums to encourage student success. These policies impose a future cost on affected students, either through the loss of acquired benefits or the threat of expulsion if they fail to perform above an established standard in future semesters. To meet these standards, treated students may be induced into exerting more effort in subsequent semesters. In addition, they have an incentive to manipulate their behavior along non-effort dimensions, such as through the type of courses and/or instructors they select. Using a regression discontinuity design, I find that the students who are treated by either the Dean's list or academic probation policy improve their academic performance in subsequent semesters. However, increased effort may not be the only mechanisms through which students change their behavior following treatment. In particular, there is evidence that the Dean's list policy induces treated students to select courses and instructors that are more likely to award higher grades and have a lower failure rate. Similarly, the results suggest that the academic probation policy causes students to switch majors and to employ what resembles a maximin strategy for expected grades when choosing courses.

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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DEDICATION

To my dad, Basil Wright, my mom, Beverley Christian Green, and my siblings Kevin Wright and Barbara Wright Weston, this accomplishment would not be possible without your unwavering support, love, and encouragement. I hope this journey is a source of inspiration for my niece and nephews.

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1 Introduction

College students are exposed to several policies and programs throughout the course of their degree. These interventions are employed by instructors, colleges, and policymakers to incentivize certain academic behaviors or to align students' actions to some established standard. Over the last decade, several education researchers have examined the impact of financing programs, public praise, and performance standards on students' college outcomes. This body of empirical evidence is useful as it provides meaningful insights for policymakers on how these programs affect students' decision-making and outcomes over time.

The existing literature provides strong support that several interventions in higher education affect students behavior and alters their outcomes over the short and long term. Many studies have shown that the college students eligible to receive merit and need-based funding are more likely to enroll in college (Dynarski (2000), Kane (2003), Cornwell et al. (2006), Kane (2007), Monks (2009)), persist longer, and are more like to graduate (Henry et al. (2004), Castleman and Long (2016), Denning et al. (2017), Melguizo et al. (2016)). The evidence on the impact of these programs on credit investment and academic performance is less consistent (Castleman and Long, 2016, Denning et al., 2017, Henry et al., 2004, Melguizo et al., 2016). More recently, a few studies have shown that receiving need or merit-based financing during college may have long term implications extending into the labor market (Denning et al. (2017), Ji (2017)).

Despite the high quality work that has been done in this area, there are still many open questions in the literature. First, more research is needed to examine the impact of college financing programs on students' work-study trade-off, career choice, and post-college earnings. Second, not much is known about the impact of student loan financing on students' academic and labor market outcomes. Third, the current studies in the college financing literature utilize panel datasets that track students over a short time horizon. As such, not much has been done on the long-term impact of these interventions or on the extent to which the effects on earnings will persist over the lifecycle. Lastly, while one-half of all college students reside in the developing world and households in this context face severe credit constraints; there is a scarcity of research on the impact of college financing policies in this context. These questions may have serious implications for policy analysis.

Other college-level interventions have also been proven effective in improving students' academic performance. This includes written reprimands and performance standards interventions such as academic probation policies. The empirical evidence suggests that these policies have an impact on affected students subsequent academic performance, likelihood of dropping out, and their earnings profile (Lindo et al. (2010), Fletcher et al. (2017), Ost et al. (2018)). Public recognition policies such as the Dean's list program has been found to have similar effects on student's academic outcomes (Seaver and Quarton (1973), Thistlethwaite and Campbell (1960)). This is a vibrant emerging literature with many unanswered questions. Key among these is understanding the mechanism through which students improve their academic performance when they are affected by the academic probation or Dean's list policies. Furthermore, no evidence exists describing how the impact of these program varies with their design and the restrictiveness of their eligibility criteria.

The main objective of this dissertation is to provide answers to several of the open questions in college financing and performance standards literature. In the first chapter, *Need-Based Financing Policies, College Decision-Making, and Labor Market Behavior*, I evaluate the impact of need-based student loan and grant financing policies on students' college and labor market outcomes in Jamaica. This chapter makes two key contributions to the college financing literature: (i) it shows new evidence on the impact of these programs on students labor market engagement during college and their earnings in the early years after college (ii) it provides new insights into the effectiveness of grant and student-loan financing in improving students' outcomes in the context of a developing country. To evaluate these programs, I construct a novel panel that follows about 50,000 students over a decade; a period which encompasses their final year in high school, their college years, and their early years in the labor market. To identify the impact of each program, I employ several quasi-experimental approaches which approximate randomization in program participation. The results show that the students who benefited from either program had a higher GPA, were more likely to remain in college beyond their third year of study, and graduated at a higher rate. However, while the grant recipients earned more following college, the earnings of loan recipients were penalized by the restrictive repayment conditions they faced. These findings suggest that the grant program may be one policy tool decision makers can utilize to improve students outcomes in Jamaica. It also suggests that more attention ought to be placed on the repayment policies facing student loan borrowers.

In the second chapter, *Perform Better, or Else: Academic Probation, Public Praise, and Students Decision-Making*, I evaluate the impact of college-level programs that ascribe public recognition or written reprimand to a set standard of academic performance on students academic decision-making. My main contributions to the literature are twofold. First, I provide extensive results on the mechanisms through which students adjust their behavior after being affected by either the Dean's list or academic probation policies. Second, this essay offers some insights into how the results may diverge when key institutional details are altered. Using the regression discontinuity design, I find that the students who are treated by either the Dean's list or academic probation policy improve their academic performance in subsequent semesters. The estimates suggest that the Dean's list policy induces treated students to select courses and instructors that are more likely to award higher grades and have a lower failure rate. Similarly, being affected by the academic probation policy causes students to switch majors and to employ what resembles a maximin strategy for expected grades when choosing courses. As such, there are several non-effort related channels through which students adjust following exposure to these programs.

2 Chapter 1: Need-Based Financing Policies, College Decision-Making and Labor Market Behavior

2.1 Introduction

Higher education plays an important role in reducing poverty and promoting upward economic mobility. With about one-half of all college students residing in the developing world (Task Force on Higher Education, 2000), policymakers in developing countries, like their counterparts in developed countries, grapple with designing an effective framework for financing higher education. Over the last two decades, many developing countries have implemented student-loan and grant financing programs to make college more affordable to low-income households(Salmi, 1999). However, many policymakers in these countries have a general view that public investments in higher education yield very small returns (Task Force on Higher Education, 2000) and that college financing programs may not be having the intended effect.[1]

There is a sizable literature exploring the effect of financing policies on college students' behavior (Arendt, 2013, Carruthers and Özek, 2016, Castleman and Long, 2016, Dearden et al., 2014, Denning et al., 2017, Dynarski, 2000, 2002, 2003, Glocker, 2011, Londono-Velez et al., 2017, Page et al., 2017, Scott-Clayton, 2011, Singell, 2004, Welch, 2014). These works have evaluated several programs including merit-based and need-based assistance, subsidized/unsubsidized loans, and institutional financing programs. They find that eligibility for need-based grant and loan programs have a positive impact on college enrollment and degree attainment(Castleman and Long, 2016, Dearden et al., 2014, Denning et al., 2017, Kane, 2003, Melguizo et al., 2016), while the evidence on academic performance and credit investment is inconclusive(Castleman and Long, 2016, Denning et al., 2017). However, there is a scarcity of empirical papers examining the impact of student-loan programs on college

[1] See Woodhall (1983, 1988) for an overview of student loan financing in developing countries. Johnstone and Marcucci (2007) and Salmi (1999) discuss the recent worldwide trends in higher education financing.

and labor market outcomes. Furthermore, for developing countries, evidence on the impact of need-based grant financing on students' college performance, work-study trade-off, and labor market outcomes is primarily descriptive. This research tries to address these important questions.

In this paper, I employ several quasi-experimental designs to estimate the impact of need-based student-loan and grant financing programs on students' college outcomes and labor market behavior during college and in the early years after college. This is done by constructing a novel and detailed individual-level panel dataset using administrative educational and labor market records across several institutions in Jamaica. Jamaica is an upper-middle income country where two in every five college students rely on publicly-financed student-loans and/or grant subsidies to consume higher education. To promote upward mobility for low- and middle-income households, these programs are used to make college more affordable for eligible students. Since the students who benefit from these programs differ from those who do not in ways that we may not observe, the main empirical specifications exploit quasi-random variation in financing to identify the causal impact of each program. As such, estimates from the instrumental variables and regression discontinuity designs are used to bolster the results from a naive within high school cohort by year fixed effects model. Together, these estimates provide key insights into the impact of each program on students behavior during and after college.

This paper has two main contributions. First, it is among the early studies focusing on the impact of college financing programs on students' work-study trade-off and post-college labor market outcomes (Denning et al., 2017, Ji, 2017, Rau et al., 2013, Ziebarth et al., 2017). My main addition to this literature is exploring how need-based student-loan and grant financing affect labor market engagement during college and in the early years after expected graduation. I also provide evidence on how the length of the grace period before loan repayment begins affects the annual earnings of new graduates in their starting job. Second, this is the first paper to examine how eligibility for a need-based grant impacts students' college outcomes and post-college labor market behavior in a developing country context.[2] There are key institutional differences between the economic environment in developed and developing economies that may cause college students to exhibit varying behavioral responses to the same exogenous change in income.[3] These two contributions are important in light of the considerable use of these financing programs in developing countries, especially as others deliberate on implementing their own versions of these policies(Salmi, 1999).

A simple theoretical argument can be made about the mechanism through which need-based grant and student-loan financing programs affect college students behavior over time. On the one hand, the need-based grant is as a lump-sum transfer that loosens the budget constraint of low-income students who are unlikely to have access to credit markets. This funding option has a pure income effect which allows an eligible student to purchase higher quality education inputs (normal goods) and potentially substitute away from working while in college. As such, this income transfer could improve students performance in college and yield positive labor market returns after completing college. Similarly, the student-loan financing option facilitates the intertemporal substitution of future earnings to offset current education expenditures. The interest rate on the loan is the price of the substitution of this income across periods. As such, the need-based student-loan augments the household's budget in the current period. This makes it easier for low-income students to offset college expenses without dramatically reducing current consumption, purchase higher quality inputs

[2] To the best of my knowledge, Denning et al. (2017) is the only other paper to assess the impact of need-based grants on post-college labor market outcome. He uses data from Texas to examine how eligibility for the maximum Pell Grant impact college and labor market outcomes

[3] First, developing countries typically have a large share of low-income households that face significant barriers to accessing financial markets and are unable to afford college expenses. As such, affected households either experience a high cost of borrowing or they are frozen out of the loan market entirely. Second, developing countries usually experience higher default rates due to institutional inefficiencies and the lack of political will to punish defaulters. Third, several developing countries implemented higher education financing reforms in the 1990s under the guidance of the World Bank(Salmi, 1999). These programs may impose conditionalities on students that may not exist in developed countries. For example, many developed countries have income-contingent repayment systems for student-loan borrowers. In contrast, student-loan repayment conditions in developing countries are usually not as generous. Finally, differences in the skill distribution and institutional framework between developed and developing countries may lead to a differential in the return to a college education(Patrinos et al., 2006, Peet et al., 2015). Given these differences, there is an economic justification for us to question the extent to which college financing policy will have a symmetric impact across these development contexts.

and be less reliant on work during college. Additionally, the students who are eligible for the loan may be induced into exerting a higher level of effort or be dissuaded from shirking while enrolled in school. This is because they want to improve the likelihood of obtaining a job upon completion and receive a higher income to repay the debt they have accumulated. Furthermore, the loan granting agency may mandate that the student maintain a certain standard of performance if funding is to be renewed (Schudde and Scott-Clayton, 2014). Lastly, among new graduates, the students who utilized a student-loan may be willing to accept a lower wage than those who did not (Gibson and Johnson, 2017, Ji, 2017, Ziebarth et al., 2017). This is because the student ought to begin repaying their debt quickly to avoid added interest, penalties or default. Strict repayment conditions increase job search costs and reduce the bargaining position of new graduates, and this may cause them to accept wages below their productive capacity. As such, these programs may alter students behavior over both the short and long term.

To examine each of these mechanisms, this study focuses on the impact of each program on academic performance, attempted credits, the likelihood of drop-out, on-time graduation and work decisions while enrolled in college. During the early years after expected graduation, I also examine how each program impacts the likelihood of being employed, initial earnings and tax contributions. As such, the current research offers general insights into the effectiveness of public financing policies and provides recommendations that education policymakers can leverage to design better targeted and more socially efficient policies.

I find evidence that the students in the loan or grant program have a better GPA throughout college, graduate at higher rates, and are more likely to persist to their third year in college relative to untreated students. The results also suggests that each program reduces treated students' labor market engagement during their college years. However, while the grant improves the labor market earnings of treated students in the early years after college, I find that the students who utilized the loan earned less during these years relative to the control group. These results are consistent across estimation approaches and research sites. These findings have two main implications. First, the need-based grant is an effective tool that can be leveraged to improve the college performance and labor market outcomes of low and middle-income students. Second, the restrictive repayment conditions attached to need-based student-loan programs may lead to negative unintended consequences when students enter the labor market. As such, policymakers ought to ensure that loan repayment conditions does not disadvantage new college graduates entering the labor market.

The remainder of this paper is organized as follows: Section II provides a description of the institutional details and data utilized in this study. Section III outlines the empirical models and identification strategy, section IV presents the main results, and section V summarizes the concluding arguments.

2.2 Data and Institutional Background

This paper utilizes administrative person-level data from five institutions in Jamaica. These are the *Students' Loan Bureau (SLB)*, the *Tax Administration of Jamaica (TAJ)*, *Ministry of Education (MOE)*, *The University of the West Indies (UWI)* and the *University of Technology (UTECH)*.

2.2.1 Student Loan Financing in Jamaica

The main institution of interest is the *Students' Loan Bureau (SLB)*. This is an independent statutory body invested with the sole responsibility of granting tuition loans to students from low- and middle-income households that are unable to self-finance college. It is publicly financed and caters to students attending one of the 45 approved tertiary educational institutions (universities, teachers' colleges, and community colleges) in the Caribbean.

The Students' Loan Bureau became operational in 1970 and was re-structured in 1996 through a US$28.5 million loan from the World Bank. The loan facility was developed as a revolving loan fund such that future loan disbursements would be financed primarily through repayment flows from past loan beneficiaries. However, the SLB annual report in 2014 shows that a large share of its source of funding was obtained from education taxes or government subvention (46%), grants earmarked for particular loan types (6%), and injections from the Ministry of Finance (3%). Consequently, 55% of loan disbursements in 2014 came from supplemental funding due to the high degree of non-repayment. This has sparked several national debates about the sustainability of the Bureau and the benefits it offers to recipients.

For students starting their first academic semester in September, the student loan application process begins in February and ends around May of each year. Each student is required to complete a comprehensive online form, which collects his personal characteristics, identification details, tertiary institution particulars, and household level information. Also, the applicant is required to provide a detailed record of the households' resources, financial assistance, savings, employment history, educational records of each household member, and the details of two guarantors who sign as underwriters for the loan.

The Bureau offers three main loan facilities. The first is a Targeted Loan facility which determines loan eligibility using a mean-test assessment to determine need. To qualify for a Targeted Loan, a student must be a Jamaican national, prove financial need and be accepted into an approved institution and/or making satisfactory academic progress in their program of study. Secondly, the SLB offers a Pay As You Study (PAYS) loan which facilitates students who fall outside of it's target loan population. This loan is repaid by salary reduction while the student is enrolled in college. Finally, the Bureau offers a Post Graduate loan which provides financing to cover the cost of tuition. This loan program offers each beneficiary the lowest interest rate for a market loan, a more affordable monthly payment and a longer repayment period. It is geared towards working students whose employment facilitates repayment through salary deduction while they are enrolled. However, the need-based targeted loan program accounts for approximately 99% of the loans disbursed (SLB, 2015). This will be the focal program being evaluated in this study.

Once a student submits the online application form, a means-test score is calculated using the self-reported information. This score is used to determine the applicant's eligibility for the Targeted Loan program. Each applicant and his guarantors must also attend an interview with the SLB to ensure that he has not misrepresented his application, to submit supporting documentation, and for guarantors to sign the contractual agreement. For approved applicants, their application is subjected to a second round of review to determine eligibility for a grant-in-aid. This can be outlined as follows:

$$I_i = F(W_h, N, L, R_i(E, H)) \tag{1}$$

where N is household size; W_h is the total household wealth; L is the cost of living in the applicant's locality of residence and R is the perceived risk of repayment. The risk of repayment is a function of the earning potential of the selected degree (E) and the payment history of past recipients that chose that degree (H)[4].

Once I_i is calculated, individuals are ordered and their scores are compared to the loan and grant eligibility thresholds. Each applicant is then presented with one of three possible results: (1) the loan is approved (2) the loan is approved and a grant is awarded or (3) the loan is denied & no grant is awarded. As such, loan applicants from needy families may also be approved for a grant if I_i is sufficiently small relative to the grant threshold. Individuals cannot directly apply for this grant and students can only exploit it if they are approved and utilize a SLB loan to pay for their tuition. The grant is non-repayable, disbursed at the start of each semester, and is intended to subsidize the cost of books and the living expenses of college students who are most in need[5]. Each individual is advised in writing of the status of his application within three weeks of his online submission. Further, disbursement is made once the applicant submits the required supporting documents to the Bureau and his guarantor signs for the loan. Returning applicants are required to submit a status report

[4] Loans and grants are issued subject to budgetary allocations. The details of the means-test score is not publicly available. Access to this information is restricted to a limited number of officials. As such, to protect the confidentiality of the SLB's operation, information will be presented on the mechanics of the scoring procedure on a need-to-know basis.

[5] Over the sample period, about 25 percent of applicants who received a student loan from the Bureau, also qualified for a grant-in-aid. The acceptance rate for the loan is approximately 98 percent.

each year, and recently, they have been required to submit their transcript to prove that they are meeting newly imposed GPA requirements. Approximately two-thirds of the value of all loans disbursed go to Jamaica's two leading institutions, namely the University of Technology and the University of the West Indies. These are the two universities of focus in this study.

For low- and middle-income families who face binding liquidity constraints, the *SLB* is the main option to finance post-secondary human capital accumulation (in the absence of being awarded an institutional or private sector scholarship). For students enrolled in a three or four-year degree program, the loan repayment period is typically 15 years at an interest rate of 9% with a six-months moratorium period post graduation[6]. Consequently, the guarantor secured loan service offered by the SLB ensures that college is affordable for low- and middle-income families at a competitive market rate.

Since the Bureau utilizes a means-test to determine applicants' eligibility for a student loan and grant, it creates discontinuities in the financial resources available to eligible students. This creates a natural experiment that can be exploited to provide meaningful insights into the impact of need-based financial assistance (loans & grants) on college students short and long run behavior.

2.2.2 Data

The core population of interest is comprised of students that are enrolled at the two most selective colleges in Jamaica (the *UWI* & *UTECH*). The records of these students are merged with their high school and labor market records.

On average, the two colleges jointly account for about two-thirds of all student loans disbursed by the SLB (SLB, 2015). The expected duration of a bachelor's degree ranges from 3 to 5 years across institutions. At both colleges, courses are offered in the fall, spring and summer semesters. New cohorts are normally admitted at the start of the academic

[6]The average lending rate for installment, mortgage, personal, and commercial credit in Jamaica between 2010 and 2016 was 18%

year in September. Each institution determines tuition based on the degree program and the number of credits in which the student is enrolled. As such there is significant heterogeneity in tuition cost and loan amount across degree programs and institutions.

Students pre-treatment academic performance is obtained from the *Ministry of Education (MOE)*. This is the public entity responsible for administering education policies that govern public schools in Jamaica. This institution also supervises the standardized regional high school exit examinations. These exams include the Caribbean Secondary Education Certificate (CSEC) and the Caribbean Advanced Proficiency Examinations (CAPE). Students sit for about 8 *CSEC* examinations in various subject areas upon completing five years of secondary school[7]. Qualifying students can then opt to complete two more years of advanced secondary studies and are typically advised to sit for 2 to 4 *CAPE* examinations each year[8]. To qualify for entry to university, students must typically complete at least 5 CSEC examinations and 4 CAPE units. The academic records obtained from this entity offers meaningful details on the high school academic performance of students. These pre-treatment characteristics can be used as a useful control for students academic ability.

Finally, to assess formal sector labor-market outcomes, employment and earnings records are obtained from the *TAJ*. This institution is the tax authority in Jamaica, which hosts employment records (annual earnings, employers and taxes) for all workers in the formal sector. To calculate a measure of labor-market intensity, records on individuals' mandatory weekly national insurance contributions are utilized. Each employer in the formal sector is required to deduct a mandatory share of their employee's salary to be paid into a national social security scheme. This scheme offers some financial protection for the taxpayer in the event of a loss of earnings through employment-related shocks. In a given year, the number

[7]There are over 30 subjects offered by the regional examination body, covering the core areas of the sciences, arts and business. All colleges strongly recommend that students complete *CSEC* Mathematics and English with a passing grade.

[8]The CSEC examinations are graded on a discrete scale ranging from 1 to 6, with 1 being the highest pass and 6 the lowest. Colleges accept grades of 1 to 3 in the CXC examinations. Similarly, the CAPE scoring ranges from 1 to 7. The poorest quality pass that colleges accept in this examination is 5. each CAPE subject is offered in units of 2.

of weeks the employer contributes on behalf of the employee can be used as a good measure of the number of weeks for which the taxpayer was employed.

To track an individual across institutions, the records held by these entities are merged using each person's date of birth, sex, first, middle, and last names. Since loan and grant eligibility is independent of these characteristics, any errors in the matching process is likely random[9]. Together, these records form a panel of students' high school performance, demographic and household details, family financial information, college records, loan repayment data, and labor market outcomes over the period 2006 to 2016. The key advantage of using this data is that it contains very detailed characteristics and an extensive time horizon. The postsecondary and labor market variables of interest include cumulative GPA, attempted credits, the likelihood of graduation, employment, and earnings. Students' labor market outcomes are examined while they are enrolled in college and within three years after expected graduation.

The sample is restricted to Jamaican college students between the ages of 17 and 44. Students with missing values for any of the outcomes of interest are also omitted[10]. The first cohort of students entered university or submitted a student loan application starting in 2006. Since degree programs have an expected duration of 3 to 4 years, the last entering cohort examined started their program in 2014. The core sample includes 48,329 new students who are followed for at least six semesters after starting college. Of this sample, 23,798 are enrolled at the UWI and 24,531 at the UTECH. The descriptive statistics for the entering cohort at each institution are presented in Tables 1 and 2. At both institutions, the treated group utilizing the loan or grant has a larger share of females and full-time students; they are younger on average and had a better mean scores on the standardized high school exit

[9]About 10% of loan applicants have unique student numbers. Among these applicants, the match rate is over 95%

[10]The only explanatory variable that had missing observations was the previous high school that students at UTECH attended. This record was incomplete from the university records and could not be completed using the MOE data. This variable was only utilized in the high school by cohort FE model. Since this approach relies on within high school by year of birth variation in financing status, this data limitation is not very concerning.

examination.

Over the sample period, about 43% of students in the entering class at UWI used a loan from the SLB to cover tuition expenses, compared to 38.5% of the entering class at the UTECH. Similarly, the SLB awarded a grant to about 11% and 9.3% of the UWI and UTECH entering class, respectively[11]. As such, a substantial share of the university population at the two largest universities have much of their college expenditures covered by the SLB and are directly affected by its policies. The average annual tuition fee is about 1300 dollars at the UWI and 1400 dollars at the UTECH. In the sample, the students utilizing the loan have an annual per capita family income of about 2700 dollars and the grant recipients had an annual per capita family income of about 1000 dollars. The annual grant amount is 500 dollars per year, which equals one-half of per capita family income. As such, the grant is a meaningful addition to each student budget.

2.3 Identification Strategy and Empirical Specification

The students that utilize the loan facilities at the SLB are different from those that finance their degrees through scholarships and/or using their own resources in ways that we may or may not observe. These differences arise from selection into the loan application process and the need-based criteria used by the Bureau to approve loans and award grants. Unobserved differences between these groups can induce bias in the estimated parameters in a way that cannot be fully anticipated.

On the one hand, many of the factors that positively affect college outcomes and persistence in college are negatively correlated with loan application and approval. On the other hand, the factors on which students positively select into student loan application may also enable them to perform well in college. For example, students with a higher family wealth and available resources are less likely to select into the loan applicant pool but they can also afford better inputs to improve performance at the college level. On the contrary,

[11] Roughly 25% of the loan beneficiaries receive a grant at each university.

the students that are responsible, motivated or have a better high school preparation may be more likely to apply for a student loan (conditional on income), and these students are better equipped to perform well at the college level. Some of these selection mechanisms are observed in the descriptive statistics tables and must be addressed through models that directly control for the potential bias caused by these selection decisions.

In this paper, I employ several models that exploit quasi-experimental variation in need-based grant and/or loan financing to examine the effect of these policies on students' academic outcomes and post-graduation labor market performances. These approaches allow us to estimate multiple treatment effects of these financing policies that may offer several insights to policymakers and education specialists. This section outlines the various empirical models utilized and details the identifying assumptions underpinning each model.

2.3.1 Secondary School Fixed Effect Model

To begin, a simple fixed effects model is utilized. This is given by:

$$Y_{it} = \lambda_0 + \lambda_1 L_{it} + \lambda_2 G_{it} + X_i \pi + \gamma_{hc} + \gamma_t + \varepsilon_{it} \tag{2}$$

Where i, h, c, t indexes students, high school, cohort and year admitted to college; γ_{hc} is the high school by cohort (year of birth) fixed effects, γ_t are admitted year fixed effects, and ε_{it} is an individual-specific random error. The vector X captures student-level time-invariant heterogeneity, which includes the student's sex, age, full-time status, college of study, year of admission, geographic region of residence, and high school performance. With this approach, we can compare the academic and labor market performance of students who were in the same high school cohort, admitted to university in the same year, but with different loan and grant status.

Under the assumption that high schools are comprised of a homogeneous student population, this approach may offer some insight into the impact of these programs. Causal identification requires that a student's loan and grant financing status be independent of other individual-specific factors affecting their academic and labor market decisions within each high school cohort. This requires that the assumption $E(\varepsilon_{it}|G_{it}, L_{it}, X, \gamma_{hc}, \gamma_t) = 0$ is satisfied. Since primary school students are assigned to a high school based on their proximity and exit test scores, a high school by cohort fixed effect model might capture much of the motivation, ability and high school preparation heterogeneity that could lead to bias. However, even within high school cohorts, participating in the program is negatively correlated to unobserved family wealth and other parental inputs that likely improve student success at the college level. As such, the high school by cohort FE estimates possibly suffer from a downward bias.

In this model set-up, there are two treated groups and a control group. The first treatment group is comprised of students that received a loan, but no grant was approved (L_{it}), the second group is comprised of students whose loan was approved and a grant was also awarded (G_{it}), and the reference group is comprised of students that did not apply to the Students' Loan Bureau. The parameter λ_1 is the impact of being assigned to the first group and ($\lambda_1+\lambda_2$) is the total impact of being assigned to the second group. Alternatively, λ_1 can be interpreted as the impact of receiving the loan-only and λ_2 can be interpreted as the incremental impact of receiving the grant. I use the second interpretation because it is the most comparable to that of the other empirical approaches.[12]

2.3.2 Regression Discontinuity

The funding criteria utilized by the SLB create discontinuities in the resources available to needy students. These discontinuities can be exploited to estimate how being eligible for a need-based grant impacts students' subsequent behavior.[13]

[12]Here, if $G_{it} = 1$, then $L_{it} = 1$. As such, everyone with a grant has a loan

[13]In employing the RD design, I focus exclusively on the grant program. Only 2 percent of applicants are denied because their means-test scores are above the eligibility cutoff for the loan program. As such, the density of students above the loan cutoff is inadequate to identify the effect of the program. In contrast, about 25 percent of loan recipients are awarded a grant-in-aid.

The students with a score below the grant-in-aid eligibility threshold are offered a grant, which they may accept conditional on their tuition being financed through the *SLB*. The students who were not awarded the grant based on their score may be recommended for the grant by their university or may appeal this decision with the Bureau[14]. Some students who are eligible for the grant may not receive it because they were awarded another form of need/merit based support from an alternative source (see figures 1 and 2).

Consequently, while the probability of treatment increases discontinuously as a student score passes the grant threshold from the right, the likelihood of treatment on each side of the thresholds are not exact. As such, the fuzzy regression discontinuity design is the most suitable to estimate the treatment effect in this context. This design estimates the impact of the grant-in-aid on the group of students who complied with their assignments. Equations (3) and (4) depicts a simple version of my fuzzy RD design:

$$D_i = \gamma_0 + \gamma_1 \mathbb{1}\left[I_i < c_G\right] + P_D(I_i - c_G,\ I_i < c_G) + X_i\omega + \varepsilon_i \tag{3}$$

$$Y_{it} = \pi_0 + \pi_1 \hat{D}_i + P_Y(I_i - c_G,\ I_i < c_G) + X_i\mu + u_{it} \tag{4}$$

where $(I_i - c_G)$ is the means-test score of student i net of the grant eligibility threshold; D_i takes the value 1 if student i utilized a grant irrespective of their assignment by the means-test score; Y_{it} is the college and labor market outcomes of interest; $P_D(I_i - c_G,\ I_i < c_G)$ and $P_Y(I_i - c_G,\ I_i < c_G)$ are polynomial terms in the means-test score and it's interaction with grant assignment status; X_i are background and demographic covariates, and ε_i and u_{it} are unobserved mean-zero random error terms. $\mathbb{1}\left[I_i < c_G\right]$ is the rule utilized by the SLB to determine the students that are eligible for the grant-in-aid. As such, if $I_i < c_G$ is satisfied, the student will be selected for a grant-in-aid and 500 dollars in grant funding is disbursed upon acceptance of the loan amount. The actual treatment status, D_i, differs from assignment for non-compliant students[15]. Consequently, the first stage regression in

[14]The administrators note that there are only a few appeal cases each year. As such, this should not violate the identifying assumptions.

[15]This second stage regression relies on the assumption that $Pr(A_i = 1|c^-) \neq Pr(A_i = 0|c^+)$ in the neighborhood around the eligibility cutoff.

equation 3 isolates the variation in grant use that is explained by means-test assignment.

The parameter of interest, π_1, captures the causal effect of the grant program. The main estimates correct for the bias in the conventional nonparametric polynomial approach as recommended by Calonico et al. (2014a). Additionally, all RD specifications refrain from the use of higher order polynomials as suggested in Gelman and Imbens (2014). In particular, a linear specification is used in the baseline estimates[16]. Triangular weights are utilized.

Finally, I utilize the mean squared error(MSE)-optimal bandwidth selection procedure proposed in Calonico et al. (2016) to select the bandwidth size for the focal estimates. However, I show that the results are robust across much of the bandwidth distribution. The bandwidth choices considered belong to the interval $|I_i - C_G| \in [2, 16]$.[17]

2.3.3 Local Randomization Assumption

The regression discontinuity estimates have a causal interpretation when the sufficient assumptions are satisfied. These key assumptions are documented in Imbens and Lemieux (2008), McCrary (2008), and Lee (2008).

The first is that crossing the eligibility cutoff from either direction, there is a discontinuous change in the likelihood of treatment. In the current context, this requires that students' with the lowest family income have a higher probability of being awarded the grant-in-aid. This criterion is satisfied by design since the SLB adheres to it's policy of using the means-test to determine applicant's eligibility for the grant program[18]. Figures 1 and 2 show that at each college, the students who are eligible for the grant have a 38-44 percent higher likelihood of utilizing a grant at the margin relative to ineligible students.

The second assumption is that in the neighborhood around the treatment cutoff, there

[16]The results are robust to the inclusion of quadratic local polynomials.

[17]To the right of the cutoff, as $I_i - C_G$ increases, families with more monetary resources are included in the analysis. Similarly, to the left of the cutoff, as $I_i - C_G$ decreases, families with less monetary resources are included. Consequently, as $|I_i - C_G|$ increases, the analysis contains students that are less homogeneous in terms of household wealth.

[18]While there are students who are non-compliant with their assignment to treatment, this does not invalidate this assumption.

are no systematic differences in the observable and unobservable characteristics of the treated and untreated groups. Table 3 shows that on average, the observed differences in student's age, sex, attendance status, city of residence, commuting status, HS performance, family size and welfare participation status is not statistically significant between the treated and untreated groups. As such, there are no systematic difference between treated and untreated students at the margin of grant assignment.

Lastly, since the SLB incorporates self-reported information in determining the means-test score of each applicant, there is a concern that students may manipulate their information to become eligible for grant assignment. In the absence of verification and with perfect knowledge of the assignment variable and cutoff, the strictly dominant strategy of each applicant is to report family characteristics that places them below the cutoff for each programs. However, there are three main reasons the applicants are unable to manipulate the threshold in this study. Firstly, the general public is unaware of the means-test formula and the cutoff utilized to determine eligibility. Secondly, the SLB requests proof of income and other supporting documentation for all successful applicants[19]. Thirdly, even if the applicant chooses to misreport his information at the expected cost, he is not able to manipulate his assignment to treatment. This is because each applicant independently decides the extent of the misreporting and is unaware of the amount that is needed to become eligible for treatment if he is in fact ineligible based on his true circumstances. This is more likely in this environment where the score is multi-dimensional. As such, if everyone misreports their true household characteristics, once there is a distribution of individuals on both sides of the unknown threshold[20], the treatment status is randomly determined in a local neighborhood around the cutoff. These justifications support the hypothesis of no manipulation of the assignment score.

[19]However, households can still be selective in what they report to the SLB. There is no auditing of the supporting documentation and no attempts are made to verify the veracity of the documentations provided. However, the SLB advertises strict penalties for lying on the application form.

[20]This is the case if everyone is not reporting a value that is small relative to the eligibility threshold. That is, there are sufficient misreported values above the threshold

In evaluating the null hypothesis of no manipulation at the cutoff, $H_0 : \lim_{x \uparrow \bar{x}} f(x) = \lim_{x \downarrow \bar{x}} f(x)$ for $x = I_i - C_G$, the nonparametric local polynomial density plots and test statistics proposed in Cattaneo et al. (2016a) are employed. Figures 3 and 4 shows no statistically significant change in the means-score density around the grant eligibility cutoff. The p-value (p=0.51) from Cattaneo et al. (2016a) suggest that the break in the density is not statistically significant at all conventional levels. As such, the statistical evidence suggest that the RD design can be credibly employed to estimate the treatment effect of the grant-financing on college and post-college outcomes.

2.3.4 Instrumental Variables Approach

Two main policy interventions create a natural experiment that can be utilized to assess the impact of each program. This section provides key details about how these interventions changed the likelihood of loan and grant eligibility for affected students.

2.3.5 Welfare and Grant Reform

In 2012, the SLB entered into an agreement with the Ministry of Labor and Social Security (MLSS) that increased the number of grants awarded to college students from households on social welfare[21]. With this agreement, the MLSS provided technical assistance to the SLB which led to the means-test formula being reformed, and consumption data being included in the analysis. It also fostered greater information sharing between the two entities, such as allowing the SLB to verify each applicant's welfare status through the MLSS welfare beneficiary database. This policy also made it easier for welfare beneficiaries to complete the application process. It waived the insurance cost of the loan and allowed these applicants to provide only one guarantor to secure the loan. As such, the new system improved the SLB's ability to target low income households and increased the likelihood that welfare beneficiaries would be awarded a need-based grant.

[21]The PATH social welfare program is means-tested conditional cash transfer policy. Households income status is verified using a screening interview.

Since only 2 percent of loan applications are declined, the reform had a negligible impact on loan approval. However, it had a significant effect at the grant award margin. These discussions were done privately and there were no press releases on the changes. Therefore, there is no reason to expect that the reform had any direct influence on student's subsequent outcomes. However, the reform created an exogenous variation in grant eligibility rules that can be exploited to examine the impact of being awarded a grant on students college and post-college outcomes.

To exploit this policy change, I utilize a 2SLS approach, with the first stage augmented with a Difference-in-Difference (DID) design. The first stage uses individuals' welfare participation status and the introduction of the program as instruments for individuals grant award status. Equations (5) and (6) model my 2SLS approach:

$$D_{grant_{it}} = \gamma_1 + \gamma_2 WELFARE_i + \gamma_3 WELFARE_i * POST_{2012} + X\pi + \tau + \varepsilon_i \tag{5}$$

$$Y_{it} = \alpha_0 + \alpha_1 \hat{D}_{grant} + \alpha_2 WELFARE_i + X\beta + \tau + \eta_{it} \tag{6}$$

where D_{grant} takes the value 1 if the student receives a grant; $WELFARE$ takes the value 1 for applicants that are welfare recipients; $POST$ takes the value 1 in all time period following the policy change in 2012; Y_{it} is the college and labor market outcomes of interest; τ are time fixed-effects and X is comprised of household-level and individual-level covariates[22]. The parameter γ_3 captures the effect of the reform on the likelihood that welfare recipients are awarded a need-based grant. For a causal interpretation, the reform must be independent of η_{it}, conditional on X.[23] Given that the reform was mainly an administrative change in the eligibility rules, it is unlikely that it would directly impact students' college and labor market outcomes, conditional on the background variables from the application form (X). The parameter of interest, α_1, shows the impact of the grant on the subsequent outcomes of

[22] Covariates include age, gender, household income, high school outcomes, parish of residence, tuition and fees and degree characteristics

[23] The instrument is $WELFARE_i * POST_{2012}$

students induced into receiving a grant by the reform in 2012. The sample is comprised of students who were approved for a loan from the SLB. The control group include the students with an approved student loan but who were not awarded the grant.

Panel A of Table 4 shows that the reform increased grant awards for students from welfare households by 12% and 17% at the UWI and UTECH, respectively. This temporal variation in grant assignment across welfare and non-welfare households is caused by the changes in grant eligibility rules. Causal identification requires that these amendments to the grant award rules do not alter the trends in the reported characteristics of applicants from welfare and non-welfare households. As such, the observed trends in need-based attributes (consumption, income & labor supply) should be unchanged before and after the reform.

I perform three robustness checks to show that the temporal variation in grant assignment across welfare status is exogenous. First, panel A of table 4 shows that the first-stage DID estimates are unaffected when a detailed list of background covariates are included in the model. Second, when the sample is restricted to the pre-intervention period and the date of the intervention is randomly assigned, the model finds no impact on grant assignment. In figure 5, the date of the policy change is assigned to various years in the pre-intervention period. The horizontal axis depicts the assigned year of the false policy change and the vertical axis shows the impact of the false policy change on the likelihood of welfare households being assigned a grant. For example, the first point on the graph shows the DID estimate under the assumption that the policy change began in 2007 and continued through to 2011. None of the point estimates were statistically significant when the policy change was re-assigned to a random year in the period prior to the true policy reform. This statistical evidence supports the claim that grant assignment trends are parallel across welfare and non-welfare household in the pre-intervention period. This result is not surprising given the evidence above and the parallel pre-trends in grant awards observed for welfare and non-welfare applicants in figure 6.

Lastly, I show that the reform did not have an impact on the reported household-level covariates that are relevant in determining grant eligibility. This includes consumption, per capita income and the reported number of employed HH members. Applicants were required to provide a detail description of their household consumption before and after the reform in 2012. Questions were asked about the applicants' housing ownership status, housing type (detached, semi-detached, apartment), housing material(block or wood), sewerage system, water source and appliance ownership (Air conditioner, Computer, Car, Gas Stove, Electric Stove, Washing Machine and Television). Using these reported household attributes, I created a consumption index. The consumption index increases with each item owned and ranges from 0 (low consumption) to 12 (high consumption), with a mean of 6.2 property ownerships[24]. Table 5 presents evidence that the policy change had no statistically significant impact on reported consumption, per capita HH income or the number of employed members in the household of welfare and non-welfare applicants. Jointly, the evidence suggests that the instrument likely satisfies both the exogeneity and relevance IV conditions.

The estimates from the second stage are discussed in the results section.

2.3.6 High School Visits and Loan-Use

Over the years, the SLB has used high school visits as a medium to expose potential applicants to information about the loan facilities offered by the institution. Generally, administrators believe that there is an information gap in students knowledge about the program across high schools and misconceptions about the terms of the services offered. High-school visits is one avenue through which the bureau attempts to close that information gap and offer greater transparency in the application process. The schools targeted for intervention are largely those from under-represented geographical locations in the application pool in previous years.

The information disseminated to treated schools may induce some students to apply

[24]Appendix B shows the consumption questions asked on the application form and provide details about the construction of the consumption index. Testing the individual components of the consumption index does not change the conclusion.

for a student loan to continue their studies. The students that are likely induced into the application process are those that are interested in seeking a college education, eligible for entry and unaware of the services offered by the SLB. These students would not have applied for a loan in the absence of the intervention. High school visits offer a good source of exogenous variation in loan financing because it is an information campaign aimed at increasing loan application and it has no direct impact on student's college performance (conditional on school selection covariates).

The high schools visited changes each year based on the priorities of the institution and available resources. By matching students to their likely exposure to the school visit information session, I exploit the variation in loan status created by the school visit information exposure. Using HS visits as an instrument, the two-stages least square model is estimated using:

$$D_{loan_i} = \delta_0 + \delta_1 VISIT_i + W\psi + \tau + u_i \tag{7}$$

$$Y_{it} = \upsilon_0 + \upsilon_1 \hat{D}_{loan} + W\omega + \tau + \xi_{it} \tag{8}$$

Where $VISIT_i$ is an indicator variable that takes the value 1 if the student was potentially exposed to an information session during their senior years of high school, D_{loan_i} takes the value 1 if the student receives a loan and W is a vector of individual level background characteristics such as gender, age, HS performance, parish of residence and admitted year fixed effects. It is not expected that the school visit campaign had an impact on students college and labor market outcome, except through its effect on their likely financing options. Causal identification requires that conditional on the geographic and background variables in W, the students were exogenously exposed to the loan information by the school visit campaign. The results in table 4 shows that conditional on W, the school visit campaign increased loan use by about 10 to 12 percent.[25]

The parameter of interest, υ_1, shows the impact of the loan program on the college and

[25]Figure 17 and 18 shows the variation in loan-use and high school visits over the sample period.

labor market outcomes of students induced into utilizing loan financing by the school visit campaign. This model provides a good estimate of the impact of student loan financing on the college and labor market outcomes of affected students.

2.4 Results

Using several quasi-experimental designs, I find strong and consistent evidence that beneficiaries of the need-based student-loan and grant financing programs experienced better outcomes during college. The participants of each program had a better grade point average (GPA), higher graduation rates and were more likely to remain in school beyond their second year relative to non-participants. Additionally, I find some evidence that each program reduced the labor market earnings of treated students during college. In the early years after college, while the grant program increased the earnings of recipients, I found that the participants of the loan program were induced into accepting starting jobs (and earnings) that are below their productive capacity.

Since the estimated treatment effects yield a similar conclusion across research sites, I will focus on the impact of each program on students attending the University of the West Indies and then briefly discuss the areas where the conclusions differ for the UTECH sample.

2.4.1 The University of the West Indies

The main results of both programs are presented in Tables 6 to 9. For the grant program, my preferred results are the estimates from the regression discontinuity and instrumental variables designs, while my preferred estimates for the loan program are those from the instrumental variables model. In this section, I will focus on the findings for The University of the West Indies sample.

Table 6A shows the effect of the grant program on students' college outcomes. Columns 1-6 displays the estimated impact on first semester GPA, first semester attempted credits; on-time graduation; the likelihood of graduating; degree GPA at graduation; and the probability of dropping out prior to the third year of study. The parameter estimates and corresponding standard errors from the RD, IV and FE models are presented in rows 1 to 3 respectively. The mean of the control group is also provided for each outcome and for each estimation procedure. The RD estimates indicate that the students who were marginally eligible for the grant invested in fewer credits and had a higher GPA in their first semester. They were also 21 percentage points more likely to graduate on time and graduated with a higher GPA of 0.27 points relative to marginally ineligible students. However, at this margin, the grant program had no impact on persistence in college or the overall likelihood of graduating from college. For students induced into the grant program by the 2012 reform, the IV result suggests that the grant reduced the probability of drop-out by 10 percentage points. The treated students at this margin were also more likely to graduate on time by 11 percentage points and had a higher GPA of 0.21 points at graduation.

Finally, the high school cohort by year FE estimates in Table 6A show that the participants of the grant program were enrolled in more credit hours in their first semester, were 3 percentage points more likely to graduate on time, and 3 percentage points less likely to drop-out prior to their third year of study. However, these estimates suggest that the grant program had no impact on the overall likelihood of completing college or students' degree GPA at graduation. As expected, the naive FE estimates are significantly lower than those obtained from the RD or IV designs and are only included in each table for completeness. This is because while the FE model may capture the unobserved heterogeneity in geographic and ability related characteristics, it likely suffers from a downward bias as discussed in the previous section. Given that these estimates are unreliable, moving forward, I mainly report on the estimated parameters from the preferred specifications.

Table 7A shows the impact of the grant program on students labor market outcomes during and after college. Columns 1 to 3 illustrate the impact on the likelihood of being employed, the number of weeks employed and students' annual earnings during their college years. Similarly, columns 4 to 7 shows the likelihood of being employed, the number of weeks worked, annual earnings and the taxes paid within three years after expected graduation. The remaining features of the table are equivalent to Table 6A. The RD estimates show that the students who were marginally eligible for the grant worked and earned the same during college as the students who were marginally ineligible. After college, however, grant recipients earned nearly $1020 more annually in their starting job and paid about $337 more in taxes each year. In contrast, the IV estimates suggest that the students induced into the grant program by the reform earned about $115 less each year during college. This reduction is primarily due to treated students adjusting their labor supply at the intensive margin. The students induced into the program were also 3 percentage points more likely to be employed in the early years after college, but they had the same annual salary as comparable untreated students.[26]

Consequently, the margin at which the grant program is evaluated is an important consideration in appraising its effectiveness. While the RD and IV estimates for most outcomes are consistent in terms of direction, the magnitude of the impact varies across the two designs. This is likely due to the fact that these estimators identify different local average treatment effects and highlight varying responses to the program at different points in the income distribution. This heterogeneous response to a single policy intervention is documented in the literature (Heckman and Vytlacil, 2005, Lamadrid-Figueroa et al., 2010).

A key question to consider is whether the students that became eligible for the grant because of the reform are more in need of financial assistance than those that were marginally eligible for the program prior to the reform. The IV and RD results suggest that this may be the case. A simple thought experiment may explain this heterogeneous response across the two empirical designs. Consider two extreme groups, one comprised of very poor students and the other comprised of very wealthy students. In each group, a grant is randomly assigned

[26]The individuals that are not observed in the formal sector are given zero annual earnings and weeks in the main estimates presented in Tables 7A, 7B, 9A, and 9B. In the online appendix, alternative estimates are presented in tables 7C and 9C which conditions on the individual being observed employed. Focusing on employed individuals does not change the conclusion. Selection issues become more important when we condition on employment status.

to half the population of students. Ceteris paribus, the grant should be more effective in preventing the poor student from dropping out of college for financial reasons relative to its impact on the rich student. Similarly, the grant should influence the work decision of the wealthier student to a smaller degree than that of the poorer student. This thought experiment is consistent with the IV model finding that the grant helped students to remain in college longer and reduced their earnings during this period, while the RD results find that these effects are not statistically different from zero. Furthermore, given that treated students are more likely to remain in college at the IV margin, it is not surprising that the grant would have a lower positive impact on their college outcomes and post-college earnings relative to students at the RD cutoff.[27]

Comparing the findings at the two margins, the role that employment during college plays in the observed improvement in students' college performance is not clear. While students at the RD margin do not reduce their work engagement during college, the grant had a greater positive impact on their likelihood of graduating on time and their graduating GPA compared to the students induced by the reform. However, this analysis is complicated by the differences in family income and the impact of the program on remaining in college at each margin.

Tables 8A and 9A display the estimated parameters summarizing the impact of the loan program. The outcomes presented in these tables corresponds to those discussed in Tables 6A and 7A above. The IV results in Table 8A suggest that the recipients of the loan program were 22 percentage points more likely to remain in college until their third year; 49 percentage points more likely to graduate from college; 19 percentage point more likely to graduate on time and had a 0.36 points higher GPA at graduation relative to similar students that did not utilize the loan. Therefore, the students that were induced into the loan program persisted

[27]The students that self-select out of college are those that would have been most likely to perform poorly had they remained. At the IV margin, more low performing grant recipients remain in college than low performing non-recipients. In contrast, at the RD margin, poor performing grant recipients drop-out at the same rate as non-recipients. Consequently, it is reasonable to expect that the IV treatment effect on college performance would be lower.

longer in college and were also more likely to complete. In addition, the estimates in Table 9A indicate that the beneficiaries of the loan program were less likely to be employed and earned about \$713 less each year during college. This reduction is about one-half the annual cost of tuition. After college, the students induced into the loan program earned roughly \$1800 less annually and paid about \$440 dollars less in taxes each year.

At first glance, the estimated impact of the loan program on students' college and post-college outcomes may appear inconsistent. The program enabled participants to improve their college outcomes, but these higher quality outcomes appear to yield a negative return in their starting jobs. One potential explanation for the post-college labor market result is that students utilizing the loan are incentivized to take-up early employment offers that pay them less than their productive capacity. This explanation is plausible since the loan agreement stipulates that students must start repaying their debt after December 31 of the year they completed their degree. I assess the feasibility of this explanation using three robustness checks below.[28]

2.4.2 Robustness of Grant Program Estimates

To evaluate the sensitivity of the RD findings, I re-estimate the model across the means-test score distribution. This is done for both research sites. The main objective of this exercise is to show that the grant estimates are robust to the bandwidth selection procedure. The sensitivity analysis also provides some insight into the likely direction of the bias as the bandwidth size increases. In the main specifications presented in Tables 6 and 7, the bandwidth size is selected using the mean squared error(MSE)-optimal bandwidth selection procedure proposed in Calonico et al. (2016). Given that $|I_i - C_G|$ belong to the interval [0,16], I examine how the estimates changes across this distribution.

The result of this analysis is presented in figures 7 to 16. Each figure plots the bandwidth

[28] Following multiple conversations with program administrators, this explanation was seen as the most plausible. They refer to the employment of loan recipients in the business process outsourcing (BPO) industry as one example. These jobs are more widely available and have a continuous hiring process but they offer salary packages that are below the average earnings of a college graduate.

size, $|I_i - C_G|$, along the horizontal axis and the resulting estimates along the vertical axis. The point estimates presented in Tables 6 and 7 are plotted using a square, while all other estimates are plotted with diamonds. The confidence interval is also presented as bands around each estimate.

The figures show that the qualitative conclusion for each outcome does not change as the size of the bandwidth increases. Additionally, using bandwidth sizes further away from the cutoff (larger bandwidths) produces estimates that are biased towards zero but with tighter confidence intervals. Jointly, these figures show that the main conclusions are robust to the bandwidth utilized in the main specification.

2.4.3 Robustness of Loan Program Estimates

It is paradoxical that loan recipients perform better in college but earn less in their starting jobs. One potential explanation for this inconsistency is that students utilizing the loan have an incentive to take-up job offers that pay them less than their productive characteristics because of the short repayment grace period they are given. To test the veracity of this explanation, I utilize three robustness checks: (i) estimate the impact of the loan program on labor market outcomes in professions with a longer moratorium (grace) period, (ii) estimate the impact of the loan program on employment and earnings for each year after expected graduation, and (iii) estimate the impact of the loan program on graduate school enrollment.

The first check directly assesses if allowing students a longer period to find a job before they are required to start repayment influence their starting salary after college. Since students from licensed occupations cannot gain employment in their field of study until they have obtained their professional license, the SLB gives them a moratorium period that is about 8 months longer than students in other fields. As such, I create a sub-sample comprised of individuals in occupations with a grace period of 14 months (nursing and pharmacology) to evaluate the plausibility of this mechanism. The result of this analysis is shown in Table 10. The findings suggest that the recipients of the loan are more likely to be employed after college but their earnings are equivalent to students that did not utilize the loan. This finding lends weak support to the proposed explanation.[29]

The second check presented in Table 11 and 12 indicates that in the year immediately following expected graduation, loan treated students are about 15 percentage points more likely to be employed and earned less relative to students that did not utilize the loan. This early disparity in employment and earnings support the theory that treated students are induced into lower paying jobs in order to fulfill their repayment obligation within six months of completion. It also shows that the disparity in employment and earnings persist for a few years after graduation.

The third check address the concern that there is a negative selection of lower quality loan recipients into the labor market in the early years after expected graduation. This would occur if high performing treated students are more likely to enroll in graduate school and low performing students enter the labor force. The result in Table 11 shows that the students who utilized the loan were less likely to enter graduate school by 21 percentage points. As such, under the assumption that smarter students are more likely to enter graduate school, the estimated effect of the loan on earnings may actually understate the detrimental impact of the loan program.[30] This result also supports the proposed explanation.

Jointly, the three robustness checks support the proposed explanation that the students utilizing the loan may be induced into accepting job offers that pay them less than their productive characteristics because of the short moratorium period allotted for students to

[29]Students majors are determined before they apply to the SLB. As such, the loan should not have any direct effect on the students who select into licensed occupations. However, the extent to which these results are externally valid is an open question.

[30]One may be similarly concerned that the better-trained loan recipients are more likely to migrate. For this to be a major concern, overseas migration would have to be higher for the talented loan recipients than the talented non-loan population. While this selection concern is legitimate and cannot be directly ruled out, given that the loan population have fewer resources available to them, I suspect that this is unlikely. Furthermore, a simple descriptive analysis of the labor market suggests that selective migration cannot plausibly explain the results. About 35 percent of students in the sample complete college on time. Among these students, 12 percent are observed in graduate school and 33 percent find a job in the formal sector within one to three years. Consequently, 45 percent of the students that graduate on time can be directly tracked in the early years after college. Given that the estimated size of the informal sector is 35 to 44 percent in Jamaica (Peters, 2017) and the youth unemployment rate is roughly 30 percent, it is likely that the migration rate is minute during one to three years after expected graduation.

find a job and start meeting their repayment obligation with the SLB.

2.4.4 The University of Technology

Corresponding estimates are presented for students attending the University of Technology are presented in Tables 6B to 9B. Each outcome of interest can be compared directly to those for the UWI just discussed.

The estimates for the UTECH sample show that the grant program improved treated students graduating GPA, on-time graduation, and the likelihood of remaining in college until their third year. The IV estimates suggest that the grant program reduced employment during college, but it had no impact on students labor market outcomes in the early years after college. In addition, the students who were marginally eligible for the grant program worked and earned the same during college, but obtained a higher labor market earnings of \$1092 after graduating from this university. These findings mirror those obtained from the UWI sample.

At the UTECH campus, the participants of the loan program also performed better and remained in college longer than non-participants. The loan recipients also worked and earned less during college, and earned about \$1630 less each year after college, relative to non-recipients. Consequently, the students at UTECH also apparently accepted lower paying jobs in an effort to meet their repayment obligation to the Bureau.

The UTECH estimates are typically within the 95 percent confidence interval of those obtained from the UWI sample. I utilize the independent samples test to formally assess if the corresponding point estimates from the two samples are statistically different (H_0 : $\hat{\beta}_{UTECH} = \hat{\beta}_{UWI}$). The standard error of the difference in the coefficients is calculated using $SE(\hat{\beta}_{UTECH} - \hat{\beta}_{UWI}) = \sqrt{SE(\hat{\beta}_{UTECH})^2 + SE(\hat{\beta}_{UWI})^2}$ (Clogg et al., 1995, Paternoster et al., 1998). In all cases, the test fail to reject the null hypothesis that the point estimates in the UTECH sample are statistically different from those found at UWI, at all conventional levels of significance. As such, the estimated parameters across the two research sites are statistically equivalent[31]

2.5 Conclusion and Policy Discussion

In this paper, I leveraged several quasi-experimental designs to estimate the impact of need-based student-loan and grant financing programs on students' college outcomes and labor market behavior during and after college. This examination is conducted by constructing a novel and detailed individual-level panel dataset using administrative educational and labor market records across several institutions in Jamaica. While there have been previous attempts to investigate the questions posed in this paper, I make two important contributions to the literature. First, this research is among the early studies examining the impact of college financing programs on students work-study trade-off and post-college labor market outcomes. Second, this is the first paper to examine how eligibility for a need-based grant impacts students' college outcomes and post-college labor market behavior in a developing country context.

I found that a grant targeting the neediest college students had a large positive and statistically significant impact on the likelihood of graduating on time, graduating GPA and persistence in college. There is also some evidence that the recipients of the grant program engaged in the labor market at a lower intensity during college but were more employable after completing college.

Similarly, the results indicate that the students who received the loan had a better GPA throughout college, a higher on-time graduation rate, and a lower drop-out rate relative to other students. The beneficiaries of this loan program also worked and earned less while enrolled in college. These students were also more likely to be employed after college and earned less annually in their starting job. The evidence suggests that this result may be explained by the small time window students are given to search for a job before they are required to start servicing their debt. As such, affected students are willing to accept lower

[31] These calculations are available upon request.

salary offers to meet this obligation.

Estimating the economic return of each program is challenging. One approach is to assess if each program has achieved their primary policy objective of improving college outcomes and the prospects of economic mobility for needy students. In this respect, each program has had significant successes. Looking across the IV and RD margins in both schools, each additional year of grant funding ($500) improves the likelihood of on time graduation by 5 to 9 percentage points and graduating GPA between 0.09 and 0.12 points. Similarly, each $1000 in loan funding improves the likelihood of graduating on time by roughly 1 to 4 percentage points and reduced the likelihood of drop-out before the third year by 2 to 4 percentage points. In each sample, 1 in every 8 college students benefited from the grant program and 2 in every 5 received a loan, so these improvements are large and practically meaningful for aggregate college-level outcomes.[32]

A second approach is to weigh the budgetary cost of each program against their public returns (taxes, educational attainment, crime, health, welfare). This is not very feasible in the short-term, given that most of the returns to education are realized over a long-term horizon. Over the period 2008-2016, the total budgetary cost of the grant and loan programs were US$8.92 million and US$221 million respectively. The corresponding per-recipient value is roughly US$1150 and US$4700. It is promising that in the early years after expected graduation, some grant recipients paid an average of about US$350 dollars more in taxes each year. In present discounted value, these marginal students would have repaid the grant through higher tax contributions in about 4 years. In a decade, if these differences persist, the average treated students would contribute about US$2250 more in taxes, enough to repay their subsidy and fund the grant amount for an incoming student.

A similar argument cannot be made about the loan program from evidence in the short-

[32] The average grant recipient received US$1150 or 2.3 years of grant funding and the average loan recipient borrowed about US$4700. The coefficients presented in the main tables represents the total impact of these average per-person funding. As such, to approximate the value of one year of grant funding ($500), I employ the simple formula: $\frac{500}{1150} \times \beta_{grant}$, where β_{grant} is the estimated impact of the grant on college outcomes. Similarly, the value of $1000 in loan funding is given by $\frac{1000}{4700} \times \beta_{loan}$.

term. For this program, the puzzle facing policymakers is how to ensure consistent and timely repayment while encouraging students to meet their full potential in the job market. While finding a good job match may be time-consuming, a longer job search may yield better wages and higher tax contributions. In addition, students also need an incentive to find a job and begin loan repayment. In theory, adjusting the moratorium period and formally incorporating income-based repayment may be a good place to start (Akers and Chingos, 2014, Dynarski and Kreisman, 2013). The evidence for licensed occupations supports this conclusion.

Evaluating the social return of both programs will require more analysis over a longer time horizon. This will provide more concrete evidence on the successes and failures of each program. There are important implications of this research for designing loan and grant programs in Jamaica. First, since the grant program generates net revenue for the government, it may be useful to raise the program's eligibility threshold to improve students' on time graduation and academic performance. Second, the short moratorium period appears to be forcing loan recipients to take-up lower paying job rather than searching for a job that matches their productive characteristics. While there is no evidence that this will necessarily improve the labor market outcomes of these students, the Student Loan Bureau should potentially experiment along this margin.

3 Chapter 2: Perform Better, or Else: Academic Probation, Public Praise and Students Decision-Making

3.1 Introduction

A college student receives various administrative feedback from their academic institution that may be pertinent to their subsequent decisions and outcomes. In some cases, this feedback informs students that they are performing below the acceptable academic standard expected by the institution and that failure to meet this standard in subsequent assessments will result in some action being taken against them. At other times, the institution may choose to celebrate, levy praise or publicly recognize the academic achievement of high-performing students. In both of these cases, the student receives a salient signal of the quality of their academic performance. This feedback changes the student's default option by introducing a punishment they may face or a benefit they could lose in future periods (Baumeister et al., 1990). Many colleges employ these administrative policies to encourage student success and to align each student's actions with the expectations of the institution.

Despite the high frequency with which students receive feedback about the quality of their performance, empirical evidence of their impact on students' subsequent academic decisions and outcomes is relatively sparse in the literature. This is because it is difficult to assess empirically the impact of praise and reprimands in a non-experimental setting, owing to the differences between those who are exposed to the information and those who are not. Furthermore, quality data on the performance feedback that postsecondary students receive and their academic records are not widely available. In this paper, I utilize administrative data from a large public university in Jamaica to estimate the causal impact of receiving a warning letter or being recognized on the Dean's list on students' subsequent academic decisions and outcomes.

The Dean's list initiative is one medium that colleges utilize to recognize the academic performance of high-achieving students. The list is comprised of the names of students whose grade point average (GPA) exceeds some high performance threshold. Eligible students may also receive a combination of public recognition, praise or awards for achieving this accomplishment. This list is dynamically updated at a fixed time interval and new students who have passed this performance threshold are added, while others that have fallen below are dropped from the list. In contrast, the academic probation policy targets poor performing students whose academic performance falls below some minimum performance expected by the institution. The students who satisfy this criteria receive a warning letter that explains that they have failed to meet the academic standard expected by the college. The letter also informs the student that if this performance is repeated in the following semester, they will be asked to withdraw from their studies. Both of these policies provide students with a signal about the quality of their academic performance and imposes a cost on academic performance below the established standard.

There are four main channels through which these policies can potentially impact a student's subsequent behavior. First, since being treated imposes a cost on subsequent performance below the acceptable standard, affected students may be induced into increasing the level of effort they exert. A student that is affected by these policies enters a new state of nature where he is at risk of facing a penalty or losing an acquired (endowment) benefit. This may incentivize him to more closely align his actions with the institution's preferences (Baumeister et al., 1990). Lindo et al. (2010) found evidence of this mechanism when they examined how an academic probation policy impacts students academic outcomes. Second, these policies could inform about a student's place in the GPA distribution and could cause students to update their beliefs about their academic ability (Weiner et al., 1972). Several studies show that students update their beliefs about their academic ability while in college and make decisions about persisting in a logical manner. This shows that students behave like producers in evaluating their current performance and decide how much effort to invest in subsequent periods (Elsner and Isphording, 2017, Lin, 2013, Stinebrickner and Stinebrickner, 2011). Third, receiving positive or negative information about one's academic performance may alter a student's self-image, confidence, and motivation. This argument has been made in the psychology literature, where it has been posited that goal-setting, personal expectation and mental effort investment may be influenced by the perception of success or failure (Bénabou and Tirole, 2002, Feather, 1966, Seaver and Quarton, 1973, Venables and Fairclough, 2009). Similarly, Baumeister et al. (1990) finds evidence that praise may improve an individuals subsequent performance on tasks that depend primarily on effort. These initiatives could have an impact on a student's course selection strategy and persistence in their chosen major of study. Since these policies require that students exceed a set standard of academic performance, it may incentivize them to change their behavior along non-effort margins to achieve this objective[33]. I also explore some of these behavioral changes in this paper.

This paper shares some similarity to the works of Lindo et al. (2010), Seaver and Quarton (1973) and Thistlethwaite and Campbell (1960). Lindo et al. (2010) is a recent paper that focused on the impact of performance standards on students subsequent academic outcomes[34]. I contribute to this new literature in two respects. First, I provide extensive results on the mechanisms through which students adjust their behavior after receiving a warning letter. Secondly, this paper offers some insights into how the results may diverge when key institutional details - such as the generosity of the performance standard- are altered. In the psychology literature, Seaver and Quarton (1973) and Thistlethwaite and Campbell (1960) examined the impact of public recognition on students' subsequent attitude and academic performance. Seaver and Quarton (1973) argued that Dean's list recognition serves as a potentially powerful social reinforcer of the behaviors leading to academic achievement, and it should improve students' self-esteem and expectation for future performance relative to

[33]In psychology, Baumeister et al. (1990) argues that performance-based praise may convey an implicit demand for continued good performance. The warning letter also states this expectation explicitly. In such cases, the student may be pressured into exceeding a certain level of performance. Consequently, if affected students are pressure in this manner, they have an incentive to adjust their behavior in less costly ways to achieve this objective. This may include selecting easier courses, more generous instructors or switching major. These mechanisms may prove less costly than increasing one's effort.

[34]There has been a few papers subsequently published. An example of such a study is Fletcher et al. (2017) and Ost et al. (2018).

others. Similarly, Thistlethwaite and Campbell (1960) found that there is a motivational effect of public recognition which improves students' subsequent academic aspirations and outcomes. To augment the findings in these studies, I examine the mechanism through which students adjust their behavior after being included on the the Dean's list and assess the impact of the program on student success. Further, I exploit across-school variations in the design of the Dean's list initiative to explore the importance of the frequency of treatment and degree of public recognition in students response to the program.[35] Additionally, this study benefits from several significant improvements in the RD technology that was unavailable when previous papers in the literature were written. This is the first paper to examine the impact of the Dean's list policy in the economics literature.[36]

To examine the causal effect of each program, the regression discontinuity design is employed. This is done by exploiting the discontinuities in treatment eligibility that is created by institutional policies. In particular, the identifying assumption requires that students who are immediately to the right of the performance standard threshold for academic probation and were not eligible for receiving the warning letter have similar observed and unobserved characteristics to those who are just to the left of the threshold and were treated by receiving the written reprimand. Similarly, we assume that students who are narrowly eligible for a spot on the Dean's list provide a good counterfactual to students whose grade point average (GPA) fell just below the Dean's list academic standard. While this assumption is usually violated by students' manipulation of their eligibility for treatment, this is unlikely to be the case in this paper due to the policies governing students at the institution being examined.

The results finds that both policies have a positive effect on the academic performance

[35] faculty is used equivalently to school. Each represents a collection of academic departments. For example, the social sciences faculty contains several departments including government, economics, business, psychology and sociology.

[36] To the best of my knowledge, Seaver and Quarton (1973) is the only paper that has empirically assessed the impact of the Dean's list policy. likewise, the literature on public praise is very sparse. This is not due to a lack of interest in these programs, since most students are affected by this policy and most colleges utilize them. Further, these topics are extensively examined using different methods in the psychology literature. As such, the scarcity of empirical evidence on the effect of this program is likely due to significant restrictions on the availability of college student-level academic and programs data.

of treated students in subsequent semesters. For the dean's list policy, there is evidence suggesting that the intensity of the effect varies with the frequency of the intervention and the degree of public recognition students receive. The results also suggest that the academic probation policy causes some treated students to exit the university and continuing students to improve their match quality by transferring across faculties or switching their major of study. In responding to either policies, I found that treated students are incentivized to engage in strategic course taking behavior, choosing courses that are more generous along dimensions we would expect. However, there is no evidence that either policy had a negative impact on students long-term outcomes, such as their prospects for graduation. These results provides mixed support for existing findings in the literature and offers several insights on the mechanisms students utilize to meet these standards once they have been affected by them.

The remainder of this paper is organized as follows: Section 2 outlines the data and institutional framework specific to the policies evaluated in this study. Section 3 describes the empirical model and major assumption of the regression discontinuity design, section 4 presents the main results, and section 5 provides the main the concluding arguments.

3.2 Data and Institutional Framework

The data employed in this study are taken from the administrative records of a large publicly-funded university in Jamaica. Together, these records form a student-level panel that is comprised of observations spanning the period 2008 to 2016. For each semester a student is enrolled, the panel contains data on their demographic characteristics, course details, major and degree information, academic performance, financial records, academic standing and living arrangements. This is ideal because it comes directly from the confidential records held by the university, and as such, it contains minimal measurement error. Furthermore, the level of detail contained in these records allow us to examine the impact of various academic programs on a wide array of academic decisions and outcomes of postsecondary students.

Though the institution mainly caters to domestic students, approximately 12% of all admitted students are international students coming from 98 countries worldwide. In any given semester, the enrolled student population is about 16,000, of which 21% are enrolled as part-time students. The university offers a wide array of programs choices, with students enrolled in about 200 majors over the sample period. Each student's final grades is governed by a university-wide grade scale that is converted into a standardized grade point average (GPA), ranging from a low of 0 to a high of 4.3.

After registration, each student is given a handbook which details the grading criteria, university policies, and provides information about course requirements for their chosen program of study. The majority of the undergraduate degrees offered by the university requires that students complete six semesters (3 years) with a recommended course load of fifteen (15) credits per semester. Each academic year, courses are offered in a three-semester system, corresponding to the fall, spring, and summer framework. All students entering the university must satisfactorily complete a set of core first year courses to meet the pre-requisite requirements for their second year courses. Upon completing their first year, students have the option of changing their major, pursuing a double major, or adding a minor to their current program of study.

To satisfy the requirements of their major, a student must complete a certain number of courses within their area of interest. Each school within the university has its own regulations regarding the number of courses students must complete to satisfy their major requirements, the number of core courses that are mandatory for all students in a given major and the flexibility students have in choosing free elective courses or courses outside of their departments[37].

Each school independently determines how they will recognize and reward high performing students. As a result, there is variation at the school-level in how awards programs are implemented and the criteria for qualifying for such awards. Given the heterogeneity in stu-

[37] the set of core courses varies across schools.

dent award programs across schools, the Dean's list analysis is divided into two sub-samples. To form a sub-sample, schools are grouped based on the similarity of their awards program in terms of the criteria for selection, the recognition students receive and the frequency with which students are awarded. The first sub-sample contain students in the restricted neighborhood around the eligibility cutoff in the Faculty of Social Sciences. This sample is comprised of 7,098 students that are observed until their expected graduation date (about five semesters following treatment). The sample tracks students that are treated in their first semester, starting with the cohort entering in 2009 and ending with the cohort entering 2014. Since students are followed for five semesters, the last cohort are followed through to the second semester in 2016. The second sub-sample includes students in the restricted neighborhood around the eligibility cutoff in the Faculty of Medical Sciences and the Faculty of Pure and Applied Sciences. This sub-sample contains 4,354 students, observed over four semesters following their first treatment[38]. The sample begins with students treated in their first year of 2008 and ends with the cohort of students entering in the 2014 academic year.

In contrast, the minimum acceptable performance standards for low achieving students and the GPA scheme for the class of degree students obtain at graduation are university-wide policies. These policies are established and administered by the university's examinations board. As such, this sample contains students in the maximum neighborhood around the treatment cutoff, irrespective of their school. This sample contains 15,428 students. These students are treated at the end of their first semester and are followed for five semesters thereafter.

The sample is further restricted to newly admitted full-time students enrolled in an undergraduate degree program. These students must be between 17 and 25 years of age and have no major gaps in their academic records. As such, if a student has a previous record with the university, decides to dropout but re-enters in a new major at a later date, only their initial records are included in the analysis. These restrictions ensures that the sample

[38] Both faculties in the second sub-sample generates a new Dean's list on a yearly basis. This is outlined in more details in section 2.2 below.

includes students that are most likely to comply with the mandatory course requirements governing first year students and are unlikely to have manipulated their qualifying GPA. Table A1 shows the descriptive statistics for the entering cohort of students over the sample period 2008 to 2014. Among these students, 32% are males, the mean age at entry is about 19.26 years and 69% commute to the university from off-campus housing. On average, they are were enrolled in 15.52 credits, paid $1733.30USD in tuition and 12% financed this tuition through student loan agreements.

The outcomes of interest include subsequent GPA and cumulative GPA, the likelihood of using student loan, and the likelihood of transferring or changing registration status. Additionally, the analysis examines the likelihood that students who are treated by a given program in their first year, qualify for the same treatment in future semesters. To explore the mechanism through which student's adjust their performance following treatment by either policy, I explore whether treated students select into taking less credits or easier courses. However, due to data availability limitations, students course choice selection can only be examined for the social sciences[39]. As such, students response to each policy is examined within the context of this limitation.

In measuring the level of difficulty that students select into, two main questions are asked. First, across all courses available to the student, do we observe the treated students enrolling in courses that are more generous in awarding higher grades in the past year? Secondly, if there are several instructors offering a given course at the same time, to what extent are treated students more likely to choose the stream with the professor that was more generous in awarding good grades for that course in past periods.

To explore the first question, for a given courses, we need to calculate the fraction of grades awarded as A , B, C, D and the pass rate at time $t-1$. We then assign these rates to students taking the course at time t. Since a student takes multiple courses in each semester, we average these rates within each student to get an index of the expected difficulty of the

[39]The data sharing agreement restricted course information to students in this school

courses they take. For example, consider two courses X and Y. If 30% of students pass course X and 80% pass course Y in 2008, then a student enrolled in both X and Y in 2009 would be assigned an expected pass rate of 55%[40]. A tantamount argument can be made for the share of grades that are awarded as A, B, C and D for each course. To interpret these measures as proxies for the level of difficult, the assumption is that courses with a higher pass rate or that awarded more A's or B's are easier on average[41]. As such, if students observe the past grade distribution with enough signal, they can change their performance through this mechanism.

To assess the second question, we exploit the fact that a large cross-section of courses have multiple streams offered simultaneously and independently by various instructors. As such, within these multiple stream courses, each professor is assigned a rank that is based on the level of diffiulty of their stream(s) using the measures outlined above. A student is matched to their professor and a dummy variable is created that takes the value 1 if the student chooses the instructor with the lowest level of difficulty and 0 otherwise. For example, consider two instructors W and Z who both teaches different streams of the same course in 2008. If instructor W has the highest pass rate for this course, then any student selecting the stream taught by W would be assigned 1 and those selecting Z would be assigned 0. Similarly, this measure is averaged across courses for which students are enrolled[42].

As such, if the answer to either of these questions are in the affirmative, that would be an indication that treated students are engaging in strategic course enrolment behavior by choosing courses or professors that had a more generous grading policy for past cohorts.

[40]Using performance information 3 years before student i took the course $(t-3)$ to calculate the difficulty level of each course does not change the main results. Additionally, weighting each course by the number of credits does not change the results because the majority of courses are for three credits.

[41]This is a good approximation if there is little turnover in the instructors and little changes in the ability distribution of incoming students. Previous studies have focused on a measure of averaged GPA to proxy for course difficulty. This measure is significantly less detailed since averages offer little information on the shape of the GPA distribution.

[42]Students enrolled in courses with only one instructor would automatically be assigned a value of 1 for those courses. From the data, 64% of courses only had a single instructor. Among courses with multiple instructors, 21% had 2 instructors and 10% had 3-4 instructors. Equivalent conclusions can be reached when we examine the likelihood that students choose the most difficult instructor or a below instructor within course

3.2.1 Dean's List: Social Sciences

The Dean's list initiative was introduced in the social sciences faculty in 2009. The impetus for this policy was to create a medium through which this school could recognize the outstanding academic performance of deserving undergraduate students. This list is published on the university's notice boards and website, though eligible students do not receive any other form of recognition, awards, or prizes. To be eligible for inclusion on the list each semester, students are generally required to (i) meet or exceed a semester GPA of 3.60, (ii) register for a minimum of nine credit hours, and (iii) pass all courses that are not taken on a pass/fail basis. As such, the the assignment variable this study exploits is the GPA each student receives in their first semester at the university. Within a bandwidth of 0.6, about 35% of full-time students recieved treatment by this policy at the end of their first semester over the period of interest.

The list is usually compiled and published within four weeks following the official deadline for the posting of final grades for the applicable semester. Each student is notified of his inclusion on the list by an electronic letter from the dean of the school. As such, students who have made the list are likely to be aware that they have achieved this accomplishment.

3.2.2 Dean's List: Medical, Pure and Applied Sciences

The Dean's list initiative has been longstanding in the Medical Sciences and the Pure and Applied Sciences schools (MPAS). This program is utilized to celebrate and acknowledge the academic achievement of high-performing students.

To be eligible for the Dean's list in the Medical Sciences college, students' must (i) carry a full course load and have no incomplete or failing grades (ii) have no disciplinary actions taken or pending against them, and (ii) maintain a weighted GPA above 3.6 (3.7 for specific degrees) for the previous academic year. The students who have achieved this distinction are awarded a framed certificate at an annual ceremony that is held at the end of the previous academic year. Each student is notified about his inclusion on the list about 3 weeks prior to the award ceremony by letter and email.

Similarly, to be eligible for the Dean's list in the Science and Technology college, students must (i) register for a minimum of three courses that directly relate to their program of study (ii) have exam grades that are B+ and above (iii) meet or exceed a GPA above 3.6 in each semester of a given academic year, and (iv) pass all courses in both semesters or face disqualification. As such, the policy suggests that students should be treated in a given academic year if:

$$T_i = \begin{cases} 1, & \min\left(GPA_{it}, GPA_{i,t-1}\right) \geq 3.6 \\ 0, & \text{otherwise.} \end{cases} \tag{9}$$

The students meeting these criteria are invited to an annual award ceremony where they are presented with a trophy for achieving this distinction. Across both schools, approximately 19% of students attain this honor at the end of their first academic year, within a bandwidth of 0.6 around the qualifying GPA cutoff.

The award ceremonies for both of these colleges are not open to the general student body or public. Generally, each student is allowed to invite two guests to the award ceremony to help them celebrate this accomplishment. However, unlike in the Social Sciences College, the list is not usually made public or advertised on either the university's website or school's notice boards. As such, while the awardees are likely to be aware that they have attained this recognition, the general student body and teaching staff may be unaware of the students who have achieved this level of excellence.

For the Medical Sciences, the assignment variable utilized in this study is each student weighted semester GPA at the end of their first academic year. Similarly, given the assignment to treatment equation depicted above, the running variable this study utilizes for the Science and Technology College is the minimum semester GPA a student recieves between semester 1 and 2 of their first year.

3.2.3 Academic Probation Policy

The university employs an academic probation policy to ensure that students performance is above the level necessary for the successful completion of their degree. This policy applies uniformly to all students enrolled in undergraduate program at the institution.

Each semester, all enrolled students are required to perform above a minimum academic standard to remain in good academic standing with the university. This standard was set to a GPA of 0.75 over the period 2008 to 2011 and then subsequently increased to a GPA of 1.00 during the years 2012 to 2013[43]. Those who failed to achieve this standard are placed on academic probation, received an official warning letter from the institution and their academic standing is changed from *GOOD* to *WARNING* on their online student transcript. About 13% of students are affected by this policy in their first semester.

This letter informs the student that his academic performance is deemed to be unsatisfactory and that failure to meet this standard in the subsequent semester would lead to him being asked to withdraw from the university. The student is also advised to take it seriously and seek counselling from his academic advisor or by accessing the resources of the academic support unit. If a student is required to withdraw as a result of the conditions outlined in this policy, he is allowed to return to his studies one year after withdrawal. Alternatively, he may apply for a withdrawal waiver from the Dean of his school. Consequently, the extent to which the university's threat of expulsion is viewed as credible can be questioned. In a bandwidth of 0.6 around each of the cutoffs, about 33% of students are treated by this policy at the end of their first semester and 11.5% are required to withdraw at the end of their first year. Among those who were required to withdraw, only 46% actually exited the university. As such, a large share of students are allowed to continue among those that were expected to withdraw.

Nontheless, there is an incentive to avoid being placed on academic probation. The

[43]The analysis is done for the full sample period (2008-13), with a dummy variable included to control for the shift in the warning threshold from 0.75 to 1.0. The results are robust to the inclusion of this control.

information about the criteria for this policy is outlined in the regulations handbook provided to students at registration. However, due to the size of the handbook (over 200 pages), most students may be unaware of the policy until they are bounded by it. Additionally, the university has strict course taking requirements for first year students. The extent to which students can manipulate their treatment status is explored in more details in the following sections. A copy of the warning letter treated students recieve is presented in the appendix.

3.3 Empirical Design

The regression discontinuity design is the main empirical method utilized in this study. This approach enables us to derive causal estimates that are locally valid and unbiased. Further, it complements the benefits derived from using administrative data and reduces the likelihood that the estimates outlined in this paper are affected by the typical causes of endogeneity such as measurement error or selection bias.

As summarized in Imbens and Lemieux (2008), McCrary (2008), and Lee (2008), there are three key assumptions that must be satisfied for causal identification using the RD approach. The first is that approaching the cutoff from either direction, there is a discontinuous change in the likelihood of being assigned to treatment once the threshold is crossed. For the warning letter program, the eligibility criteria for any given semester is solely dependent on that semester's GPA and is uniformly applied across all schools. However, a student's GPA does not perfectly predict their assignment to treatment. This may be due to some students applying for grade reviews, cases of pending grades or intervening program specific regulations. While administrators strictly enforce this policy, the relatively few cases where a student's GPA does not predict their treatment eligibility merits the use of a Fuzzy RD design. Similarly, to become qualified for Dean's list recognition, the student must satisfy a GPA requirement and several other criteria outlined above. Conditional on meeting the GPA requirement, the other criteria are enough to disqualify some students from making the list. As such, a student's GPA does not perfectly predict recognition on the Dean's list.

To uncover the local average treatment effect of the policy, the Fuzzy RD design is also employed in evaluating this program.

The second RD assumption requires that in a small neighborhood around the treatment eligibility GPA cutoff, those who are untreated provide a good counterfactual for those who narrowly satisfied the eligibility criteria. As such, any differences in observable student attributes within the neighborhood of the discontinuity are not systematic and are solely due to differences in the assignment variable (Bettinger et al. (2016); Leeds and DesJardins (2015)). To evaluate these assumptions, for each program, the next section provides evidence on the probability of treatment across the GPA distribution and the degree to which students characteristics are balanced in the optimal neighborhood around each program's eligibility cutoff.

The third important criteria for local randomization is that students are not able to manipulate their GPA in order to be eligible for assignment or lack thereof. This is an assumption that is frequently of concern in RD studies that uses GPA as the assignment variable. For example, Carruthers and Özek (2016) find evidence of bunching around the GPA cutoff determining eligibility for scholarship renewal. When students are unaware of the GPA threshold or do not have sufficient control over changes in the assignment variable, Bettinger et al. (2016), Leeds and DesJardins (2015), and Cohodes and Goodman (2014) show that this criterion is seldomly violated. Similarly, we intuitively expect to find no evidence of manipulation of the running variable in this paper due to four main reasons: (a) at this university, students in their first year are required to complete a set of core mandatory prerequisite courses which they must pass in order to qualify for entry into second year courses; (b) the university requires that each full-time student take a minimum amount of credits each year or withdraw from the university; (c) all mid-term and final examinations are graded using at least a single-blind process. That is, the student is always required to use his university identification number in place of his name, and the grading of undergraduate papers is usually done by graduate students; and (d) the university policy stipulates that all exam scripts ought to be independently reviewed by both a primary and secondary grader before final grades can be certified by the university registrar. In support of this theory, nonparametric local polynomial density plots and test statistics recommended in Cattaneo et al. (2016a) are discussed at the beginning of the next section. The procedure conducts a hypothesis test to determine if the density of the running variable is continuous around the cutoff[44]. That is, we are interested in evaluating the null hypotheses that $H_0 : \lim_{x\uparrow\bar{x}} f(x) = \lim_{x\downarrow\bar{x}} f(x)$, signaling no manipulation at the cutoff[45].

Given that treatment by the program is only partly determined by the established GPA cutoff, an instrumental variables approach is necessary to estimate the treatment effect. The first stage is given by:

$$D_i = \gamma_0 + \sum_{j=1}^{k}[\gamma_j PriGPA_i^j \times T_i] + \gamma_3 T_i + \sum_{j=1}^{k}[\rho_j PriGPA_i^j] + \gamma_r + \varepsilon_i$$

For each policy of interest, T_i equals 1 if student i is eligible for treatment as defined by $1(PriGPA_i > 0)$; D_i equals 1 if the student was actually treated, $PriGPA$ denotes each student's first semester GPA net of the established eligibility threshold, and γ_r is a grading regulations fixed effect[46]. From the first stage regression, we can assess the extent to which assignment to treatment, using the running variable as control, predicts a student's observed treatment status. The second stage is given by:

[44] This procedure is very similar to McCrary (2008). However, McCrary (2008) requires pre-binning of the data and introduces additional tuning parameters.

[45] However, since each student takes a finite number of courses and receives a set number of possible grades, then an assignment variable based solely on GPA is discrete. In the context of this study, there is some merit to this argument, given that within a GPA bandwidth of 0.6 around the cutoff, there are only about 100 unique values that the qualifying GPA takes on for each policy. Frandsen (2017) argues that when the running variable is discrete, then the existing RD manipulation tests are inconsistent. For such case, he proposes a test that is consistent and locally unbiased relying only on support points at and immediately adjacent to the RD threshold. Given this criticism, while this study presents the manipulation test proposed in Cattaneo et al. (2016a), the procedure outlined in Frandsen (2014) are conducted for robustness. The evidence in support of the null hypothesis of no manipulation is strong when the manipulation test proposed in both Cattaneo et al. (2016a) and Frandsen (2017) are utilized.

[46] For either policy, a student with a positive PriGPA has met the GPA requirement for treatment. γ_r captures any reforms in the grading system during the sample period. This ensures that $\alpha_3 > 0$ can be interpreted as the policy having a positive treatment effect. To ensure this ease of interpretation, in evaluating the warning letter policy, $PriGPA = -(GPA - c)$, since a student is treated by this policy when they receive a GPA below the eligibility cutoff c.

$$Y_{is} = \alpha_0 + \sum_{j=1}^{k}[\alpha_j(Pri\hat{GPA}_i^j \times D_i)] + \alpha_3\hat{D}_i + \sum_{j=1}^{k}[\pi_j PriGPA_i^j] + \nu_s + \nu_r + u_{is}$$

where Y_{is} is the academic decision or outcome variable for student i in semester s and k is the degree of the local polynomial. The parameter of interest is α_3. All the model specifications presented in this paper includes semester and grading regulations fixed effects.[47] This parameter provides an estimate of the causal impact of each policy on students' subsequent behavior.

As recommended by Gelman and Imbens (2014), I refrain from the use of higher order polynomials in the implementation of the RD design in this study. In particular, all baseline estimation and bandwidth selections in this paper use a linear polynomial specification ($k = 1$). However, the results are robust to the choice of k. Following Fan and Gijbels (1996) calculation that the triangular kernel is optimal for estimating local linear regressions at the boundary, all estimation procedures in this paper employs a triangular kernel, such that the highest weights are given to observations closest to the cutoff and the weight given to each observation decreases linearly as $PriGPA$ tends to the bandwidth size h. The consensus in the RD literature is that in practice, the choice of kernel is usually inconsequential (Lee and Lemieux, 2010)[48]. This study also employs the approach recommended by Lee and Card (2008), clustering the standard errors by each value of the normalized first semester GPA. This clustering is necessary because we have repeated observations with a single $PriGPA$ and because changes in the running variable occurs at a discrete rate of 0.01. This clustering ensures that the random errors are correlated both within and across individuals, conditioned on the qualifying GPA.

Calonico et al. (2014a) argue that the conventional nonparametric local polynomial es-

[47] The grading policy governing enrolled students were reformed in 2012 and again in 2014. Including fixed effect for these changes may improve the standard error of the estimates. However, they had no practical impact on the estimated parameters

[48] The choice of the kernel has no impact on the estimated treatment effects in this study.

timates of the treatment effect have confidence intervals that may be biased, and as such, it may substantially over-reject the null hypothesis of the non-existence of a treatment effect. This is because in estimating the conventional treatment effect, an approximation of the regression function is required on both sides of the threshold using weighted polynomial regressions. These weights are generated by means of a kernel function which is based on the distance of each observation's running variable relative to the eligibility cutoff and the optimal bandwidth selected. They posit that the usual optimal bandwidth selectors which balance the squared-bias and variance of the RD estimator yield bandwidth choices that are too large to meet invoked distributional approximations. They propose an alternative approach which bias-correct the estimated treatment effect. I use this approach in this paper, though the approach does not change the qualitative conclusions.

In selecting the optimal bandwidth around the threshold, I utilize the balance of covariates test and bandwidth selection procedure proposed by Cattaneo et al. (2016b) and Calonico et al. (2014b). The first uses finite-sample methods to select the optimal neighborhood around the eligibility threshold where the randomization assumption is most plausibly satisfied. The bandwidth selected using this approach ensures that students' age, gender, credits attempted, commuting and loan status are balanced on both sides of the threshold. The second is a data driven bandwidth method which provides an MSE optimal bandwidth choice.

3.4 Empirical Results

This section presents formal test statistics and graphical evidence supporting the hypothesis that the qualifying GPA of students are not manipulated within the optimal bandwidth around the policy cutoffs examined. Further, evidence is shown that the observed covariates between the treated and untreated groups are balanced. Sections 4.2 and 4.3 then discusses the estimates from the main specification for the Dean's list and warning letter policies respectively.

3.4.1 Satisfying the RD Assumptions

Potential subjects' manipulation of their eligibility status is the main concern of applied researchers when using the regression discontinuity design. In the context of this study, this becomes an issue if students who are aware of the treatment thresholds are incentivized to change their behavior to influence their treatment eligibility status. This is especially true for the marginal students who expect their performance to be close enough to the treatment threshold. As such, these students may be motivated to exert a higher level of effort or enroll in what they perceive to be easier classes in order to avoid getting a warning letter or to become qualified for Dean's list recognition. Additionally, such students may lower the number of credits they pursue in a given semester so that they may devote more time and effort per enrolled course.

If students manipulate their GPA and non-randomly sort themselves to treatment, this would lead to a violation of the local randomization assumption. As such, we would observe a discontinuous change in the density of the running variable at the eligibility cutoff, and the distribution of observable characteristics on both sides of the cutoff within the optimal bandwidth would be significantly different. Using the density test outlined in Cattaneo et al. (2016a), we fail to reject the null hypothesis of no manipulation for both policies and each sub-sample. This result is further supported by the density plots presented in figures 21 to 23, showing that students' qualifying GPA appears to be continuous across each program's cutoff. This implies that at this university, students around the cutoff do not change their behavior to be treated by these policies in the first semester.

To further support this conclusion, we can examine evidence on the extent to which the observed characteristics between the treated and untreated groups are balanced. Table A2 show that across policies and sub-samples, the treated and untreated groups had no significant statistical differences in their gender, age, loan status, attempted credits and commuting. Together, these results provide strong evidence that students are unable to manipulate their qualifying GPA. As such, it is reasonable to expect that there should be no systematic differences in the unobserved attributes of students, and that receiving a warning letter or being recognized on the Dean's list in the first semester can be considered random in the neighborhood around the eligibility cutoff.

Figures 24 to 26 also provide evidence that that the probability of treatment discontinuously increases once a student's GPA exceeds the eligibility threshold. This supports the notion that the programs administrators largely adhered to the GPA eligibility criteria as outlined in the institution's policy documents. However, a student's treatment status is not a perfect function of their GPA. The last column in Table A2 shows the first stage results of the fuzzy regression discontinuity for each policy. For both policies, the treatment status implied by a student's qualifying GPA significantly predicts their observed treatment status.

3.4.2 Dean's List Results

The main results for the Dean's list policy are presented in Table A3. The upper and lower panels contain the estimates for the Social Sciences and the Medical, Pure and Applied Sciences respectively. Columns 1-4 shows the impact of being eligible for the Dean's list in the first semester on a student's subsequent cumulative GPA, likelihood of Dean's list eligibility, credits attempted, and loan use. The mean and standard deviation of each outcome variable are also provided for students belonging to the control group.

The estimates presented in columns 1 indicate that the students who make the Dean's list in their first semester experience a better academic trajectory than the students' who were narrowly ineligible. On average, these students were found to have a greater likelihood of making the dean's list in future semesters and obtain a higher degree GPA relative to the untreated students. The treated students maintain a cumulative GPA that is roughly 0.36 points higher than similarly students that were not treated. This represent a substantial improvement for the treated students, accounting for approximately 11% of the average GPA in the control group[49]. In contrast, the results for the MPAS sample suggests that the

[49]Since the control groups outcome is the counterfactual in the absence of the treatment, the treatment effect is reported relative to the mean outcome in the control group

Dean's list had no impact on students' subsequent performance. While the treated students invested in more credits, the program had no impact on their degree GPA and no effect on their likelihood of making the Dean's list in future semesters.

The differences in the implementation of the Dean's list initiative across colleges may be one reason for the observed differences in the impact of the program on student behavior[50]. The method utilized to recognize student's accomplishment and the awards students receive varies across both samples. In particular, the social sciences faculty publicly displays a physical copy of the Dean's list on the university website and college notice boards. However, students who meet the requirements and receive this distinction receive no additional benefits, awards or prizes. In contrast, the MPA Sciences do not publicly display a physical copy of the Dean's list and students who achieve this recognition are invited to a ceremony where they are presented with certificates or trophies for this achievement. Another key distinction between the two sets of policies is that while the social sciences creates a new list at the end of each semester, the MPA Sciences recognizes students at the end of each academic year.

Therefore, one mechanism which may account for the differences in the results is the degree of public recognition and/or the frequency of the intervention. In both cases, students receive some positive benefits from having their accomplishment recognized by their institution. However, while recognition in one college is done in an award ceremony that is closed to the general public, the recognition students receive in the other college mainly offers public recognition. As such, students on the Dean's list in the Social Sciences will have a greater incentive to maintain this achievement if students are sensitive to the public humiliation created when they publicly fail to make the list consistently. Together, both estimates provide convincing evidence that the Dean's list program induces students to do no worse in future semesters, though the magnitude of the impact may vary depending on how the program is designed.

There are several mechanisms through which treated students may attain a positive

[50]There may be other unobservable differences between high achieving students in both faculties that could plausibly explain the differences in the results

benefit in subsequent semesters. On the one hand, this policy may induce students into exerting a higher effort level to maintain the recognition they have attained. For example, Seaver and Quarton (1973) notes that the Dean's list may be a powerful social reinforcer of the behaviors leading to academic achievement and it should improve students' self-esteem and their expectation for future performance. If this is the mechanism at work, we would expect treated students to invest in no fewer credits and choose courses that are equally difficult relative to those who were marginally ineligible for treatment. This would align with the administrators' vision of the program, since the goal of this program is to recognize those that have distinguished themselves as high-performing model students. On the other hand, being publicly recognized for academic performance could induce students into making myopic perverse choices in an attempt to continue making the list. This is because if they fail to make the list again, the student would lose the positive social reinforcement they have attained or potentially even face public humiliated. If we observe treated students reducing their credit load or opting for easier courses relative to the control group, this can be taken as evidence in support of this mechanism. Such actions could lead to long-term adverse consequences on the quality of education the student receives and would offset some of the gains of the improved performance that is observed.

The estimates in column 3 show that the students impacted by the policy invest in no less credits than unaffected students. At the margin, treated students in the Social Sciences invest in the same number of credits as their untreated counterparts in the control group. As such, the improved performance of treated students in this sample cannot be explained through their credit taking behavior. However, this result is expected because the strict Dean's list eligibility criteria do not create much opportunity for treated students to reduce their credit load and remain eligible for the list in a given semester. In contrast, the treated students in the MPA Sciences are induced into accumulate more credits at the margin relative to the control group. As such, there is no evidence that the students exposed to the Dean's list invest in less credits as a strategy to improve their subsequent performance.

I also examine if students' course selection strategy is one mechanism through which they adjust their subsequent performance.[51] The result for this analysis is presented in the upper panel of Table A4. The outcomes in columns 1-5 measures the share of A's, B's, C's, D's and passes that were awarded in the past for the courses students in the treated and control group have selected into. Similarly, columns 6-7 measure the likelihood that students select the professor with the highest share of A's and B's or pass rate, when there are multiple professors teaching the same course in a given semester. These outcomes capture various difficulty criteria that student's may consider when selecting courses.

From these measures, it can be seen that students who make the Dean's list are induced into selecting courses that are easier relative to those selected by the control group. The treated group is observed enrolling in courses that are 4 percentage point less likely to award D's, 5 percentage points more likely to award B's and 2 percentage points more likely to award A's. Similarly, treated students enrolled in classes that are 5 percentage points more likely to award them a passing grade. For courses with multiple instructors, the results show that the students who are treated by the Dean's list are 15 percentage points more likely to select the instructor that awarded the highest fraction of A's and B's in the past and 21 percentage points more likely to select instructors that have the highest pass rate, among the available within-course choice set of professors.

Jointly, these results indicate that treated students are able to ascertain credible signals about the level of difficulty of courses and instructors based on the distribution of past grades. They utilize these signals to avoid selecting courses with a high failure rate and choose courses that are more likely to award grades that are B and above. This strategic course taking behaviors may partially account for their improved performance in future semesters. However, given that at the margin, the treated group are enrolled in courses that awards

[51] Given that there are no performance improvements observed in the MPAS, the analysis regarding course selection focuses primarily on the Social Sciences. In the data sharing agreement, course information was only requested for only the students enrolled in this faculty. While course-level data for the MPAS sample can be requested, the net gain of doing this is very small in light of the conclusions about the impact of the program in this sample and the high administrative cost of collating the data.

only slightly more A's and B's, we must exercise caution when concluding about the role of course selection on the observed differences in subsequent academic performance across groups. The analysis thus far does not rule out increased effort as a potential mechanism through which treated students adjust, and other non-effort mechanisms could play a role in explaining the improvement in students performance.

Since it is not clear if students are trading off the content they learn by choosing easier courses or instructors, it is difficult to assess the long term implications of these actions. However, the results from Table A3 and A5 suggest that there are tangible benefits that treated students receive in the medium term not restricted to academic performance. For example, there is some evidence that the treated students in the social sciences reduced their reliance on loans by about 29 percentage points following treatment. Further, affected students were more likely to graduate on time or they graduated at no worse durations relative to those who were not treated.

3.4.3 Academic Probation Results

The academic probation policy is another mechanism through which university administrators provide feedback to students about the quality of their academic performance relative to expected university standards. The students who fail to meet or exceed this level of academic performance receive a letter that formally places them on academic warning. If this poor performance persists in the subsequent semester, the regulation requires that the student be asked to withdraw from the institution. As such, after students are warned in their first semester, they have to make key decisions about the courses they select, whether to exit and if they should persist in the major they have selected when they started the university. The results below shows the effect of the academic probation policy on students subsequent academic performance and explores the mechanism through which students adjust their behavior following treatment.

Table A6 shows the main estimates for the academic probation policy. Columns 1-6 shows the impact of receiving a warning letter on students' subsequent cumulative GPA, likelihood of falling below the warning letter threshold, attempted credits, switching majors, changing faculties, and exiting college. Among the students that continue their studies beyond the first year, the estimates in column 1 show a modest improvement in their subsequent academic performance. The overall improvement in degree GPA of about 0.16 accounts for roughly 9% of the control group's mean degree GPA. However, the estimated parameter is insignificant at conventional levels. This may be partly explained by students' heterogeneous response to the policy, where the average effect masks substantial heterogeneity across groups, as argued in Lindo et al. (2010)[52].

The estimates presented in columns 4-6 indicate that at the margin, this policy improves the match quality between the university and the enrolled student population. The students who receive a warning letter in their first semester are on average 11 percentage points more likely to exit the university relative to the untreated students. This decision to exit is mainly driven by the withdrawal requirement of the policy, rather than being the choice of affected students[53].

Additionally, treated students are 10 percentage points more likely to switch to a new major and 8 percentage points more likely to transfer to a new faculty relative to students in the control group. This suggests that changes in the program of study is one mechanism through which students adjust after being placed on academic warning if they remain in school.[54] Together, these findings suggests this policy induces poor performing students

[52]Heterogeneity in the treatment effect across faculty was confirmed using this data. The largest positive impact was found for students in the social sciences, with the treatment effect ranging from negative to positive values across faculties. These estimates can be provided upon request.

[53]Exit in column 6 takes the value 1 if the student is absent at the start of any future semester. The same result is obtained when exit at the start of semester 3 is utilized. I found no significant effect of the program, when voluntary exit is the outcome of interest.

[54]The university administrators view transfers and major change as the main response of students that perform poorly in their first year. They argue that poor performing students from the pure and applied sciences faculty switch to degree programs in the social sciences faculties at the end of their first year. Citing anecdotal evidence, they suggest that these transfers can be explained by the general perception held by pure and applied sciences students that social sciences courses are easier on average. Within the optimal bandwidth, 85% of all transfers consist of flows from the pure and applied sciences to the social sciences faculty.

to either exit the university or change their program of study at the margin. As such, students that are not a good fit for the university have their matches terminated and others are allowed to make new matches with majors that may better fit their academic ability. However, with 8 percentage points of students switching majors and a 10 percentage points estimated differential in the probability of a major switch, the impact of these decisions on students subsequent performance may not be large, but certainly not negligible.

While previous studies did not find any evidence that being placed on probation causes continuing students to enroll in easier courses, the estimates in the lower panel of Table A4 contradicts this result. Using average course grades as the measure of course difficulty, Lindo et al. (2010) found that treated students do not select into easier courses. Similarly, the results in this study suggests that treated students do not choose courses that has a higher pass rate[55]. However, when the level of difficulty is measured as the share of the grade distribution allocated as A's, B's, C's and D's, a completely different picture arises.

The lower panel of Table A4 shows that treated students appear to follow a strategy of maximizing their minimum grade (maximin strategy) when choosing courses in future semesters. The students that received a warning letter at the 0.75 or 1.00 threshold are observed selecting into courses that have higher density in the lower tail and lower density in the upper tail, awarding more C's and D's but less A's. On average, these students enroll in courses that awards about 7% more C's and D's (the lowest passing grades) relative to the courses taken by the control group. However, there is no evidence that when treated students are given the option of choosing among several instructors teaching the same course, they were relatively more likely to select the professors that were generous in passing students or awarding higher grades.[56] It is not clear if this maximin selection strategy helps the treated students to perform better in the short term, given that there is no difference in the pass rate and they are less likely to get grades in the upper tail of the distribution. However, there is no evidence that their behavioral changes have any long term impacts on their graduation

[55]This study was also able to confirm their result using average course grade as the measure of difficulty

[56]The same results are obtained when instructors are ranked using the percent of C's and D's they award

rates. Treated students had an equal likelihood of graduating and were also equally likely to graduate in three years or four years.

The results established for the academic probation policy in this paper offers mixed support for those established in the literature, while offering more robust clarification for the mechanisms at work. For example, Lindo et. al. (2010) argues that this policy induces some students to voluntarily exit the university and remaining students to exert a higher level of effort in future semesters. Additionally, they do not find any evidence that students adjust their behavior along other dimensions unrelated to their effort choice. In contrast, the results presented above shows that the probation policy influences the match quality between students and the university through exits, transfers and major switches. It also induces the treated students that continue to change their course taking behavior along several dimensions. However, not much can be said about the impact of the policy on student's subsequent effort decisions. It is not clear the extent to which key institutional differences across studies may account for these contradictions[57]. There is much that future studies need to examine for us to garner a fulsome understanding of the impact of these policies on students behavior.

Unlike previous studies, the analysis in this paper is not affected by endogenous exits. This selection would cause the less motivated treated students to choose to exit the university. The remaining sample would not be balanced in the later semesters and could bias the estimated parameters. However, given that in this study, exits are involuntary and driven by the estimates in this study suggest no impact on voluntary exit, the results presented here are less likely to be affected by this selection bias.

[57]The most notable institutional difference is that at the institution being examined, approximately one-half of the students that are required to withdraw receive a waiver of this requirement. Additionally, the probation policy cutoff examined in this paper is 1.00 and 2.00, compared to 1.60 in Lindo et al. (2010)

3.4.4 What is the Impact of Making the Probation Policy More Restrictive?

In the 2014-2015 academic year, the university administrators implemented several changes to the institution's grading and degree granting regulations. These changes increased the cutoffs for each letter grade and also changed the warning letter threshold from 1.0 to 2.0. This policy change is a natural experiment that be exploited to test the sensitivity of the main results to the generosity of the warning letter policy. That is, it allows us to examine how the impact of the policy varies as the performance threshold becomes more restrictive and binding on students that are higher up in the grade distribution.[58]

The estimated impact of the 2.0 probation policy is presented in the lower panel of Table A6. The probation policy leads to a small improvement in degree GPA of about 0.13. The results indicate that treated students were also more likely to switch their majors and school of study. However, these parameters are insignificant at the conventional levels. Finally, the impact on exit is smaller in magnitude at this margin and is no longer significant.[59] The treated students at this margin also utilized a maximin strategy by selecting courses with a higher pass rate and professors with a history of awarding passing grades.

As such, the conclusion remains the same as reported for the baseline estimates. It implies that the small improvement in students subsequent performance may not be mainly driven by an increase in the level of effort they exert. The key insight of this result is that when the probation environment becomes more restrictive and the ability of the marginally treated increases, the effect of the policy remains consistent and the channels through which students adjust their behavior is largely unaffected.

3.4.5 Robustness Checks

While great care was taken in selecting the bandwidth size, I next examine the robustness of the results to the bandwidth choice. As such, the estimates from the main RD specifications

[58]With the revised grading regulation, approximately 30% of students are affected by the policy at the end of their first semester. This compares to 13% being affected under the prior threshold

[59]There is still no evidence of an effect on voluntary exits.

are evaluated using a simple robustness analysis. The sensitivity of the estimated parameters are assessed by examining the impact of changing the qualifying GPA bandwidth within the interval [0.2, 0.6], for each stepwise unit of 0.1.[60]

Table A7 shows the falsification results for the Dean's list policy and both the Social Sciences and MPA Sciences samples. Across all of the bandwidth choices, the estimated effect of the policy is consistent in direction and magnitude for bandwidth choices closer to the cutoff. As the bandwidth increases, there are no major improvements in the efficiency of the estimated parameters, but the estimated impact on students' subsequent performance decays monotonically as the bandwidth interval tends to 0.6. This suggests a downward bias in the estimated parameters as the local randomization assumption fails. It also indicates that the standard error estimate was reliable for the baseline results. From this exercise, it becomes more convincing that the Dean's list policy has a positive impact on students' subsequent performance in the social sciences, but a negligible impact in the medical, pure and applied sciences faculties.

Tables 8 and 9 present the results for the academic probation policy. For all potential bandwidth sizes within the interval of interest, the estimated parameters are consistent in both direction and magnitude. As the bandwidth size tends to the upper limit of the interval of interest, all the estimates remain relatively stable. As such, the main estimates and conclusions from Table A3-A6 are robust to the bandwidth choice.

3.5 Conclusion

This study examines the extent to which college students are incentivized to change their behavior when they receive administrative feedback that either reprimand or reward them for their past academic performance. To explore the effects of praising, rewarding or reprimanding students for their past performance, this paper examines the impact of the academic probation and Dean's list policies on college students' academic decision-making. Further,

[60] We arrive at similar conclusions when the falsification test of shifting the cutoff is utilized.

the study examines the mechanisms through which students treated by these policies subsequently alter their behavior. The main conclusions outlined in this study may be applicable in most contexts where an individual's past performance is used as a basis for reprimand or recognition, in an attempt to influence their future performance. For example, in the service industry, workers performance are frequently appraised and those performing exceptionally well are often publicly recognized. Similarly, several HR departments utilize a warning letter system to discourage employees from continuously engaging in unproductive behaviors (Phillips et al., 2017).[61]

Consistent with the few studies in the literature, I find that the Dean's list policy improves the academic performance of treated students in subsequent semesters. However, the intensity of the effect seem to vary by the design and implementation of the program. In particular, the frequency of the intervention and/or the degree of public recognition seem to be important policy conditions that influence how students respond to being treated. The result also found mixed support for the mechanism through which treated students adjust their subsequent behavior. The evidence suggests that treated students are incentivized to engage in strategic course taking behavior. However, the degree of this adjustment does not seem large enough to fully explain the estimated effect of the policy on academic performance. As such, treated students likely improve their subsequent performance through multiple mechanisms.

The results for the academic probation policy offers mixed support for findings in the literature. This study finds that treated students had a modest improvement in their subsequent academic performance. However, this improvement masks significant heterogeneity in how students responded to the policy. Consistent with the literature, this study finds that the academic probation policy causes low-performing students to exit the university at a relatively high rate. However, unlike previous studies, the decision to exit is due to the conditions of the policy. The evidence suggests that treated students are incentivized

[61] Such behavior includes showing up to work late or failing to meet some set targets among other things

to improve their match quality by transferring across faculties or switching their major of study.

There is also evidence that the policy induces students to adjust their credit taking and course selection behavior. Students are observed following what resembles a maximin strategy when selecting courses, by choosing courses that have higher density in the lower tail of the past grade distribution and less density in the upper tail. As such, this study provides evidence that refutes the claim that improvements in student's subsequent GPA can be explained primarily by increased effort. It also suggests that the data utilized in this study does not fit the simple theoretical performance standards model presented in previous papers (Lindo et al., 2010).

These results indicate that a university administrator's action of reprimanding or rewarding students for their past academic performance may induce important behavioral changes. While there is no evidence that either policy has resulted in any long-run negative outcomes, the results suggests that the positive gains to student academic performance may not be solely driven by increased effort. The positive impact of being warned in the first semester become effective during the second year, with students' response holding constant as they progress. This provides an opportunity for innovation, where the potential benefits of this policy can be greater exploited through university's sustained engagement with low-performing students. This is already being employed by some universities that uses a data-analytics driven approach to target low-performing students and offering them greater assistance that improve their likelihood of success[62]. This approach is now feasible due to significant improvements in technology and it may yet show greater promise in promoting student success than a static warning letter system.

These results offer many insights that policymakers ought to consider when designing interventions that rewards or reprimands students.

[62] These universities have emphasized many benefits of using this approach, though the reported successes have been mainly anecdotal(Arnold and Pistilli, 2012).

4 Top-Line Summary and Future Projects

This dissertation provides answers to several of the open questions in the college financing and performance standards literature. This is done by employing several quasi-experimental econometric approaches and using novel administrative panel data from Jamaica.

In the first chapter, *Need-Based Financing Policies, College Decision-Making, and Labor Market Behavior*, I examined the impact of need-based student loan and grant financing policies on students' college and labor market outcomes in Jamaica. This chapter addressed three important questions (i) can student loan and grant financing programs improve students' college outcomes in a developing country? (ii) Are these programs reducing the incentive to work during college? and (iii) Does participating in these program impact post-college LM outcomes?

In the second chapter, *Perform Better, or Else: Academic Probation, Public Praise, and Students Decision-Making*, I evaluate the impact of college-level programs that ascribe public recognition or written reprimand to a set standard of academic performance on students academic decision-making. This chapter examined three key open questions in the literature (i) what is the impact of academic probation and Dean's list programs on student's behavior? (ii) what mechanisms does student use to improve their performance when they are exposed to these policies, and (iii) what is the impact of changing the academic probation eligibility threshold?

There are five main conclusions from both chapters of the dissertation. First, the results showed that both the grant and student-loan programs help needy college students to perform better during college in Jamaica. Second, the grant program improved the annual earnings of beneficiaries by as much as $1020 US in the early years after college. The evidence suggests that among marginal recipients of the program, the grant had a positive net inflow for the government. Third, the evidence suggests that the repayment conditions are important in determining the labor market performance of students that rely on student loans. In particular, a short grace period prior to the mandatory start of repayment may cause students to accept lower earnings. Fourth, performance standards, such as academic probation and Dean's list policies, can raise the academic performance of low and high achieving students in the college context. Fifth, the mechanism through which these policies improve students' performance may not be through inducing greater effort as administrators. These students change their behavior along several non-effort dimensions including course selection and major switches.

I have several upcoming projects that are broadly related to the questions addressed in the dissertation. I expect to continue my research in at least two directions. First, I plan to assess the behavioral and psychological triggers of student-loan delinquency. Student-loan debt has been steadily increasing in most countries. Loan granting agencies struggle to develop an effective strategy to encourage repayment without imposing a huge cost on borrowers. In some cases, non-payment of student loans means that other deserving students are not able to access loan facilities to finance their college education. While a large share of students has been affected by this problem, there is a scarcity of empirical studies examining the factors that cause students not to repay their loans and the strategies that may be used to encourage repayment. In a future project, my co-authors and I will examine the way nudges can be used to improve student-loan repayments for students in varying stages of delinquency. By assessing five framing treatments, we attempt to estimate which message framing creates the most effective incentive to induce repayment. Furthermore, I have collected several years of student-loan repayment data for my dissertation that has yet to be examined. This data is novel and may offer significant insights into policy questions about the causes of student loan delinquency once it is linked with students' high school, college, labor market, and loan information.

Second, I plan to further examine the impact of education policies and public programs on individual behavior in both developed and developing countries. The main mandate in this second line of research is to broadly examine the way large-scale policy interventions affect individuals' education, health, and labor-market outcomes. For example, in an upcoming piece of research, "Teacher Quality and High School Performance: Evidence from a Math Specialist Intervention" (with Patrice Anderson), my co-author and I are utilizing the universe of high school records in Jamaica to examine the efficacy of an intervention program directed at improving the quality of mathematics teachers.

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5 Chapter 1 Appendix

5.1 Appendix A1: Tables

Table 1: Entering Students Descriptive Statistics, University of the West Indies

Fin Stat	Male	Age	FT	Tuition	Loan	Entry Score	CXC
DNA	0.33	20.66	0.76	1216.78	0.00	36.29	7.72
	0.47	4.63	0.43	1289.42	0.00	15.29	1.57
Denied	0.27	19.54	0.85	1522.26	0.00	39.78	7.91
	0.45	3.00	0.36	1617.78	0.00	13.25	1.51
Loan	0.28	19.55	0.89	1431.37	1357.08	41.07	8.02
	0.45	2.79	0.31	1380.54	691.07	13.53	1.48
Loan & Grant	0.26	19.54	0.95	1316.19	1303.22	41.16	7.96
	0.44	2.29	0.21	1037.26	588.05	12.95	1.42
Total	0.30	20.16	0.83	1301.21	573.86	38.40	7.85
	0.46	3.92	0.38	1304.72	794.55	14.66	1.53

Note: The mean and standard deviations are presented within each financial status. The first group consist of students that did not apply (DNA) for a student loan and pay their tuition from personal funds or through scholarships. The remaining groups includes the unsuccessful applicants, the loan-only approved applicants and students approved for both loan and grants. The number of observations in each of these groups are 13211, 420, 7555 and 2612 respectively.

Table 2: Entering Students Descriptive Statistics, The University of Technology

Fin Stat	**Male**	**Age**	**FT**	**Tuition**	**Loan**	**HS GPA**	**CXC**
DNA	0.48	21.43	0.67	1450.82	0.00	1.68	5.13
	0.50	5.33	0.47	1406.30	0.00	0.99	3.40
Denied	0.37	20.80	0.71	1243.08	0.00	1.64	4.90
	0.48	4.04	0.46	1288.64	0.00	0.97	3.11
Loan	0.40	20.38	0.78	1378.99	1567.20	1.81	5.62
	0.49	3.42	0.41	1371.76	385.18	1.04	3.71
Loan & Grant	0.35	20.36	0.84	1410.84	1510.10	1.81	5.59
	0.48	3.04	0.37	1398.20	385.63	1.03	3.64
Total	0.44	21.02	0.72	1423.80	599.33	1.73	5.32
	0.50	4.68	0.45	1394.71	793.32	1.01	3.52

Note: The mean and standard deviations are presented within each financial status. The first group consist of students that did not apply (DNA) for a student loan and pay their tuition from personal funds or through scholarships. The remaining groups includes the unsuccessful applicants, the loan-only approved applicants and students approved for both loan and grants. The number of observations in each of these groups are 14842, 225, 7189 and 2275 respectively.

Table 3: Local Randomization Within Bandwidth

	Age	Male	FT	City	Commute	HS GPA	Fam Size	Welfare	Treatment
UWI	-0.35	0.11	0.003	0.05	-0.09	0.20	-0.08	-0.02	0.47***
	(0.95)	(0.16)	(0.11)	(0.09)	(0.12)	(0.20)	(0.43)	(0.05)	(0.06)
Obs	1,735	1,899	1,737	1,823	1,686	1,140	1,768	1,301	1,588
UTECH	0.960	-0.0287	0.0603	-0.0296		0.449	-0.634	0.0478	0.40***
	(0.817)	(0.268)	(0.199)	(0.161)		(0.720)	(0.505)	(0.0666)	(0.05)
Obs	1,603	1,036	825	920		975	1,358	1,019	1,327

Note: Standard errors are clustered within year of entry and presented below each estimate. The HS performance measure differs across university. As such, it is normalized for comparability. For each model, the bandwidth size is selected using the MSE-optimal bandwidth procedure proposed in Calonico et al. (2016).

Table 4: IV First Stage Results

	UWI	UTECH	Controls
Panel A: Grant			
WELFARE*POST	0.12***	0.17***	Yes
	(0.033)	(0.034)	
WELFARE*POST	0.12***	0.16***	No
	(0.033)	(0.036)	
Obs	10,003	8,916	
Panel B: Loan			
HS VISIT	0.12***	0.10***	Yes
	(0.01)	(0.019)	
HS VISIT	0.18***	0.16***	No
	(0.01)	(0.017)	
Obs	23,432	16,720	

Grant Instrument: Welfare Reform; Loan Instrument: HS Visit

Note: Control covariates include: gender, age, full time status, parish of residence dummies, HS performance, per capita family income(grant), faculty of study dummies and admitted year dummies.

Table 5: Trends in Household-Level Variables

	Consumption	Log HH Income	# of Employed HH Members
Treatment Effect	0.04	0.01	0.001
	(0.05)	(0.20)	(0.02)
Mean	6.18	7.19	1.37

Table 6A: The impact of the grant program on college outcomes

	GPA	Credits	Ontime Grad	Any Grad	Grad GPA	Exit (3^{rd} Year)
			UWI			
RD	0.48**	-1.15***	0.21***	0.03	0.27**	-0.02
	(0.22)	(0.37)	(0.08)	(0.15)	(0.13)	(0.08)
Mean	2.16	15.16	0.49	0.71	2.82	0.32
Obs.	1,333	1,557	1,681	1,197	655	1,651
IV	0.19**	0.26	0.11**	0.08*	0.21***	-0.10**
	(0.09)	(0.21)	(0.04)	(0.05)	(0.07)	(0.04)
Mean	2.13	15.04	0.38	0.66	2.71	0.19
Obs	10,003	10,003	10,003	7176	6041	10,003
FE	0.03	0.19***	0.03**	0.02	0.002	-0.03***
	(0.03)	(0.06)	(0.012)	(0.012)	(0.018)	(0.01)
Mean	2.04	14.48	0.346	0.60	2.72	0.26
Obs	23,432	23,432	23,432	17,241	13,288	23,432

IV Instrument: Welfare and Grant Reform

Note: Standard errors are clustered within year of entry and presented below each estimate. Control variables include: gender, age, family income & consumption, HS performance, faculty/major of study, parish of residence & admitted year dummies. GPA and Credits are presented for the first semester. The exit outcome captures students persistence as of the beginning of the year.

Table 7A: The impact of the grant program on labor market outcomes

	Emp[1]	Weeks[2]	Earnings[3]	Emp[4]	Weeks[5]	Earnings[6]	Taxes[7]
			UWI				
RD	0.01	-0.41	150.86	-0.10	-2.34	1018.2***	336.50***
	(0.06)	(1.31)	(179.40)	(0.11)	(3.33)	(346.67)	(107.77)
Mean	0.08	1.39	184.2	.26	8.16	1558.66	377.81
Obs	4,161	3,093	3,531	3,516	4,212	3,891	3,756
IV	0.003	-1.35**	-114.5**	0.03**	-0.59	-11.05	32.32
	(0.02)	(0.52)	(45.88)	(0.013)	(0.52)	(226.40)	(61.10)
Mean	.13	2.45	308.17	.24	7.50	1559.18	378.31
Obs	30,009	30,009	30,009	25,383	25,383	25,383	25,383
FE	0.002	-0.31**	-41.85	0.02*	0.37	-45.83	-2.87
	(0.01)	(0.15)	(25.67)	(0.01)	(0.32)	(51.05)	(13.11)
Mean	.13	3.23	571.32	.25	7.67	1759.55	450.40
Obs	70,296	70,296	70,296	60,294	60,294	60,294	60,294

IV Instrument: Welfare and Grant Reform

Note: Standard errors are clustered within year of entry and presented below each estimate. Control variables include: gender, age, family income & consumption, HS performance, faculty/major of study, parish of residence & admitted year dummies. Columns [1]-[3] are outcomes observed during college and [4]-[7] are observed after college.

Table 8A: The impact of the loan program on college outcomes

	GPA	Credits	Ontime Grad	Any Grad	Grad GPA	Exit (3^{rd} Year)
			UWI			
IV	0.42***	0.15	0.19***	0.49**	0.36*	-0.22***
	(0.15)	(0.67)	(0.07)	(0.22)	(0.20)	(0.08)
FE	0.08***	0.20***	0.01	0.04***	-0.01	-0.07***
	(0.02)	(0.05)	(0.01)	(0.01)	(0.01)	(0.01)
Mean	2.04	14.48	0.35	0.60	2.72	0.26
Obs	23,432	23,432	23,432	17,241	13,288	23,432

IV Instrument: High School Visits

Note: Standard errors are clustered within year of entry and presented below each estimate. Control variables include: gender, starting age, family income, HS performance, faculty of study, parish of residence & admitted year dummies. GPA and Credits are presented for the first semester. The exit outcome captures students persistence as of the beginning of the year.

Table 9A: The impact of the loan program on labor market outcomes

	Emp[1]	Weeks[2]	Earnings[3]	Emp[4]	Weeks[5]	Earnings[6]	Taxes[7]
			UWI				
IV	-0.02	-2.36	-712.5***	0.073	2.00	-1791.31**	-441.34**
	(0.047)	(2.13)	(243.6)	(0.06)	(2.69)	(663.13)	(200.84)
FE	0.01**	-0.36***	-162.80***	0.03***	0.47**	-111.90***	-50.91***
	(0.004)	(0.13)	(26.52)	(0.01)	(0.21)	(39.59)	(10.51)
Mean	.13	3.23	571.32	.25	7.67	1759.55	450.40
Obs	70,296	70,296	70,296	60,294	60,294	60,294	60,294

IV Instrument: High School Visits

Note: Standard errors are clustered within year of entry and presented below each estimate. Control variables include: gender, starting age, HS performance, faculty of study, parish of residence & admitted year dummies. Columns [1]-[3] are outcomes observed during college and [4]-[7] are observed after college.

Table 6B: The impact of the grant program on college outcomes

	GPA	Credits	Ontime Grad	Any Grad	Grad GPA	Exit (3^{rd} Year)
			UTECH			
RD	0.22	-0.13	0.17	0.08	-0.07	-0.09***
	(0.21)	(1.09)	(0.25)	(0.19)	(0.18)	(0.034)
Mean	2.35	14.93	0.52	0.68	2.77	0.094
Obs	1,184	881	871	810	745	1,058
IV	0.34***	0.03	0.05	0.11*	0.34***	-0.10**
	(0.08)	(0.33)	(0.04)	(0.06)	(0.10)	(0.04)
Mean	2.25	14.25	0.33	0.59	2.72	0.143
Obs	8,916	8,916	8,916	6,632	4,124	8,916
FE	0.07***	0.24**	0.03**	0.04**	0.05**	-0.02*
	(0.03)	(0.10)	(0.01)	(0.02)	(0.02)	(0.01)
Mean	2.13	13.63	0.27	0.52	2.69	0.23
Obs	16,635	16,635	16,635	11,919	6,718	16,635

IV Instrument: Welfare and Grant Reform

Note: Standard errors are clustered within year of entry and presented below each estimate. Control variables include: gender, age, family income & consumption, HS performance, faculty/major of study, parish of residence & admitted year dummies. GPA and Credits are presented for the first semester. The exit outcome captures students persistence as of the beginning of the year.

Table 7B: The impact of the grant program on labor market outcomes

	Emp[1]	Weeks[2]	Earnings[3]	Emp[4]	Weeks[5]	Earnings[6]	Taxes[7]
			UTECH				
RD	0.05	0.88	365	-0.06	6.85	1092***	356.02**
	(0.09)	(2.95)	(392)	(0.18)	(6.89)	(430.3)	(178)
Mean	0.08	1.84	227	0.36	13.10	2838.05	732.59
Obs	2,940	3,528	3,123	1,742	1,509	2,125	2,250
IV	-0.01	-0.63	-174.60	-0.09	-3.93	-73.96	-191.30
	(0.04)	(1.22)	(189.70)	(0.09)	(2.48)	(589.20)	(285.60)
Mean	0.12	3.02	428.93	0.33	11.52	2507.60	632.60
Obs	26,748	26,748	26,748	11,295	11,295	11,295	11,295
FE	-0.02**	-0.86***	-136.90***	0.01	0.34	25.97	27.52
	(0.01)	(0.25)	(45.58)	(0.02)	(0.76)	(132.70)	(42.76)
Mean	0.12	3.42	634.78	0.31	11.12	2725.98	699.15
Obs	49,905	49,905	49,905	18,109	18,109	18,109	18,109

IV Instrument: Welfare and Grant Reform

Note: Standard errors are clustered within year of entry and presented below each estimate. Control variables include: gender, age, family income & consumption, HS performance, faculty/major of study, parish of residence & admitted year dummies. Columns [1]-[3] are outcomes observed during college and [4]-[7] are observed after college.

Table 8B: The impact of the loan program on college outcomes

	GPA	Credits	Ontime Grad	Any Grad	Grad GPA	Exit (3^{rd} Year)
			UTECH			
IV	0.42***	1.12	0.02	0.16	-0.008	-0.27***
	(0.12)	(1.19)	(0.09)	(0.16)	(0.13)	(0.081)
FE	0.13***	0.46***	0.04***	0.06***	0.02	-0.11***
	(0.02)	(0.07)	(0.01)	(0.01)	(0.01)	(0.01)
Mean	2.13	13.63	0.27	0.52	2.69	0.23
Obs	16,635	16,635	16,635	11919	6718	16,635

IV Instrument: High School Visits

Note: Standard errors are clustered within year of entry and presented below each estimate. Control variables include: gender, starting age, family income, HS performance, faculty of study, parish of residence & admitted year dummies. GPA and Credits are presented for the first semester. The exit outcome captures students persistence as of the beginning of the year.

Table 9B: The impact of the loan program on labor market outcomes

	Emp[1]	Weeks[2]	Earnings[3]	Emp[4]	Weeks[5]	Earnings[6]	Taxes[7]
			UTECH				
IV	-0.18***	-5.70***	-779.3**	-0.014	1.87	-1630.43**	-532.90**
	(0.028)	(1.00)	(306.6)	(0.092)	(4.11)	(795.68)	(234.69)
FE	0.01	-0.51***	-248.10***	0.04***	1.17**	-213.50**	-52.29*
	(0.005)	(0.19)	(44.44)	(0.01)	(0.48)	(98.11)	(29.86)
Mean	0.12	3.42	634.78	0.31	11.12	2725.98	699.15
Obs	49,905	49,905	49,905	18,109	18,109	18,109	18,109

IV Instrument: High School Visits

Note: Standard errors are clustered within year of entry and presented below each estimate. Control variables include: gender, starting age, HS performance, faculty of study, parish of residence & admitted year dummies. Columns [1]-[3] are outcomes observed during college and [4]-[7] are observed after college.

Table 10: Sensitivity of labor market estimates to longer moratorium period

	Emp	Earnings
FE	0.045*	32.14
	(0.025)	(88.16)
IV	0.31*	-140.1
	(0.180)	(478.6)
Obs	2999	2999

Note: Specification check for professions that require a license.

Table 11: Disaggregated post-college impact of the loan

	Grad School	Second Yr Earnings	Third Yr Earnings	Fourth Year Earnings
			UWI	
IV	-0.21***	-1402.44*	-1124.75	-3052.78**
	(0.03)	(834.20)	(876.52)	(1472.27)
FE	-0.012***	-147.60***	-59.30	64.20
	(0.004)	(53.07)	(102.06)	(518)
Mean	0.07	1928	2683	3426
Obs	23,432	20,056	17,241	14,212
			UTECH	
IV	0.01	-770.40	-2261.60**	-3224.91**
	(0.04)	(882.50)	(1118.12)	(1381.80)
FE	-0.01***	-144.04	-294.60*	97.10
	(0.002)	(122.72)	(165.50)	(510.18)
Mean	0.02	3223.90	4077.81	4729.80
Obs	16,635	6,437	5,273	4,112

Note: Standard errors are clustered within year of entry and presented below each estimate. Control variables include: gender, starting age, family income, HS performance, faculty of study, parish of residence & admitted year dummies.

Table 12: Disaggregated impact of the loan program on employment

	Second Yr Employment	Third Yr Employment	Fourth Year Employment
		UWI	
IV	0.15	0.09	0.17*
	(0.10)	(0.16)	(0.10)
FE	0.031***	0.028***	0.036***
	(0.01)	(0.01)	(0.01)
Mean	0.24	0.28	0.32
Obs	20,056	17,241	14,212
		UTECH	
IV	0.16	-0.05	-.03
	(0.13)	(0.15)	(0.19)
FE	0.037**	0.038*	0.04*
	(0.018)	(0.02)	(0.025)
Mean	0.32	0.35	0.36
Obs	6,437	5,273	4,112

Note: Standard errors are clustered within year of entry and presented below each estimate. Control variables include: gender, starting age, family income, HS performance, faculty of study, parish of residence & admitted year dummies.

5.2 Appendix A2: Online Tables

Table 7C: The impact of the grant program on LM outcomes

	Emp[1]	Weeks[2]	Wages[3]	Emp[4]	Weeks[5]	Wages[6]	Taxes[7]
			UWI				
RD	0.007	-4.83	188.88	-0.12	3.13	3045.5**	865.2**
	(0.06)	(8.30)	(1046.4)	(0.11)	(6.59)	(1345.0)	(357.2)
Mean	0.083	16.5	2149.81	.26	31.78	6043.70	1466.88
Obs	4,551	379	379	4,551	1,165	1,165	1,165
IV	0.018	-9.08***	-732.0**	0.038***	-7.86**	-344.0	64.48
	(0.022)	(1.84)	(288.8)	(0.012)	(3.07)	(1254.3)	(369.2)
Mean	.13	19.01	2386.56	.22	31.09	6467.84	1569.30
Obs	30,009	3,875	3,875	25,383	6,119	6,119	6,119
FE	0.0019	-1.86**	-206.3	0.014	-0.017	-342.7	-55.18
	(0.006)	(0.75)	(130.9)	(0.0084)	(0.66)	(222.3)	(51.97)
Obs	70,296	9,177	9,177	60,294	13,798	13,798	13,798
			UTECH				
RD	0.065	1.14	284.7	-0.05	6.96	3431.5**	1377.3**
	(0.11)	(3.20)	(393.1)	(0.21)	(8.73)	(1740.4)	(637.9)
Mean	0.082	23.58	3046.75	0.36	36.38	7860.29	2011.66
Obs	3,921	322	322	2,668	960	960	960
IV	-0.009	1.25	-530.3	-0.070	-2.95	210.5	-232.5
	(0.034)	(4.17)	(588.3)	(0.086)	(2.94)	(1243.1)	(193.3)
Mean	0.13	25.43	3660.91	0.32	35.02	6234.58	1495.82
Obs	26,748	3,206	3,206	11,295	3,719	3,719	3,719
FE	-0.014*	-2.69	-669.0	0.009	0.48	372.8	224.7
	(0.0074)	(1.82)	(442.0)	(0.019)	(1.14)	(497.5)	(145.7)
Obs	49,905	5,864	5,864	18,109	5,617	5,617	5,617

IV Instrument: Welfare and Grant Reform

Note: Standard errors are clustered within year of entry and presented below each estimate. Control variables include: gender, starting age, family income, HS performance, faculty of study, parish of residence & admitted year dummies.

Table 9C: The impact of the loan program on LM outcomes

	Emp[1]	Weeks[2]	Wages[3]	Emp[4]	Weeks[5]	Wages[6]	Taxes[7]
			UWI				
IV	-0.02	-2.2	-2566.8**	0.073	-0.98	-3230.4*	-770.5
	(0.047)	(6.92)	(1248.4)	(0.06)	(5.62)	(1728.6)	(540.7)
FE	0.006	-2.56***	-873.8***	0.025***	-1.01**	-275.9	-150.0***
	(0.004)	(0.60)	(131.4)	(0.005)	(0.48)	(170.8)	(44.23)
Obs	70,296	9,177	9,177	60,294	13,798	13,798	13,798
			UTECH				
IV	-0.17***	-6.92	-1585.7	0.024	4.12	-2864.4**	-620.3
	(0.025)	(5.32)	(1333.0)	(0.09)	(4.13)	(1232.4)	(401.3)
FE	0.0063	-3.43***	-1546.8***	0.036***	-0.38	-696.0**	-163.9*
	(0.005)	(1.04)	(297.6)	(0.01)	(0.75)	(322.0)	(95.98)
Obs	49,905	5,864	5,864	18,109	5,617	5,617	5,617

IV Instrument: High School Visits

Note: Standard errors are clustered within year of entry and presented below each estimate. Control variables include: gender, starting age, HS performance, faculty of study, parish of residence & admitted year dummies.

5.3 Appendix A3: Figures

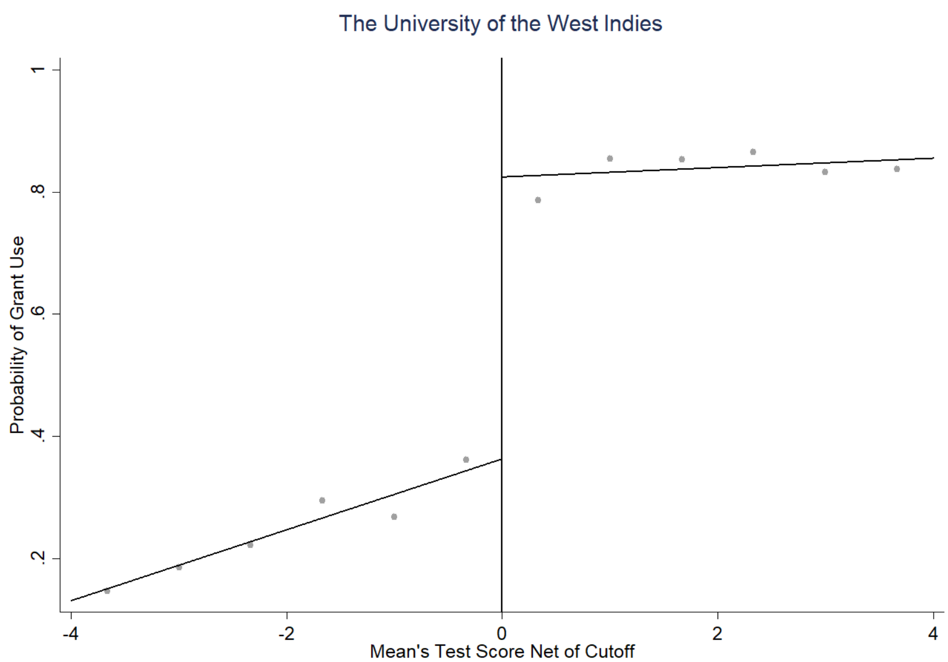


Figure 1: Prob of grant use w/in bandwidth (UWI)

(Each dot represents the fraction of students treated within evenly spaced GPA bins)

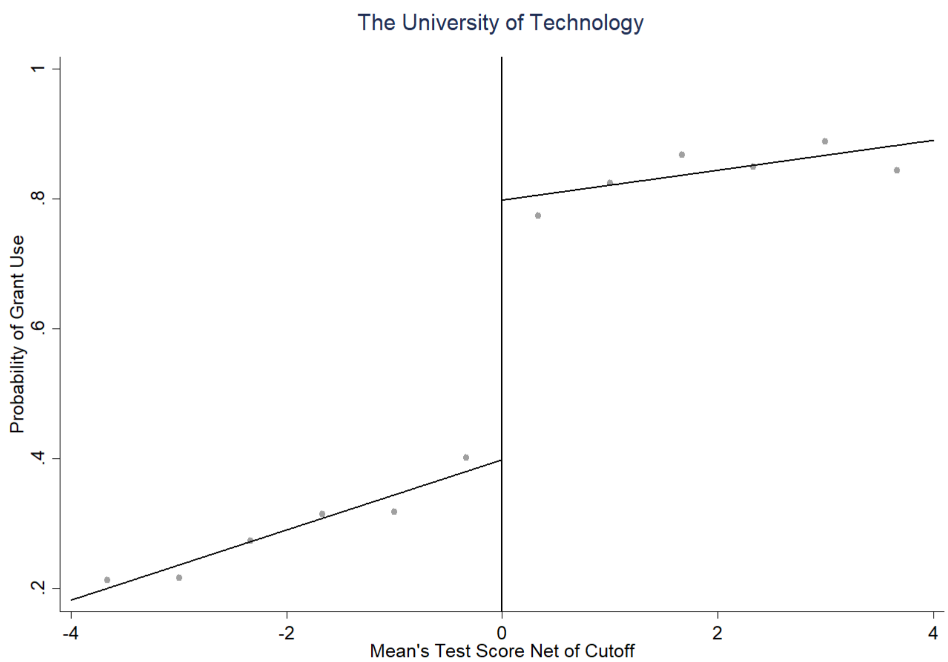


Figure 2: Prob of grant use w/in bandwidth (UTECH)

(Each dot represents the fraction of students treated within evenly spaced GPA bins)

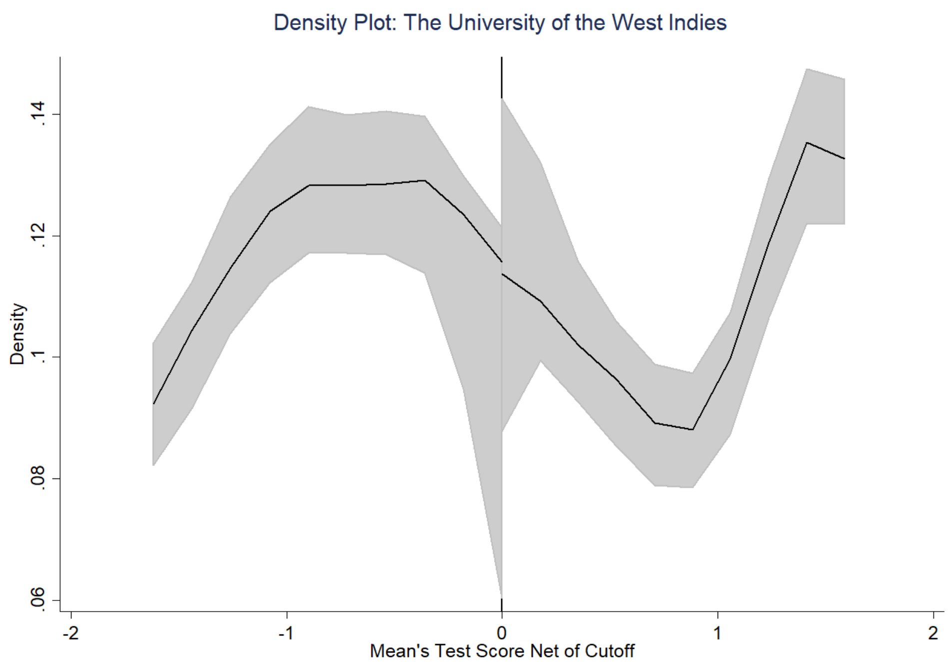


Figure 3: Assignment Score Density Plot (UWI)

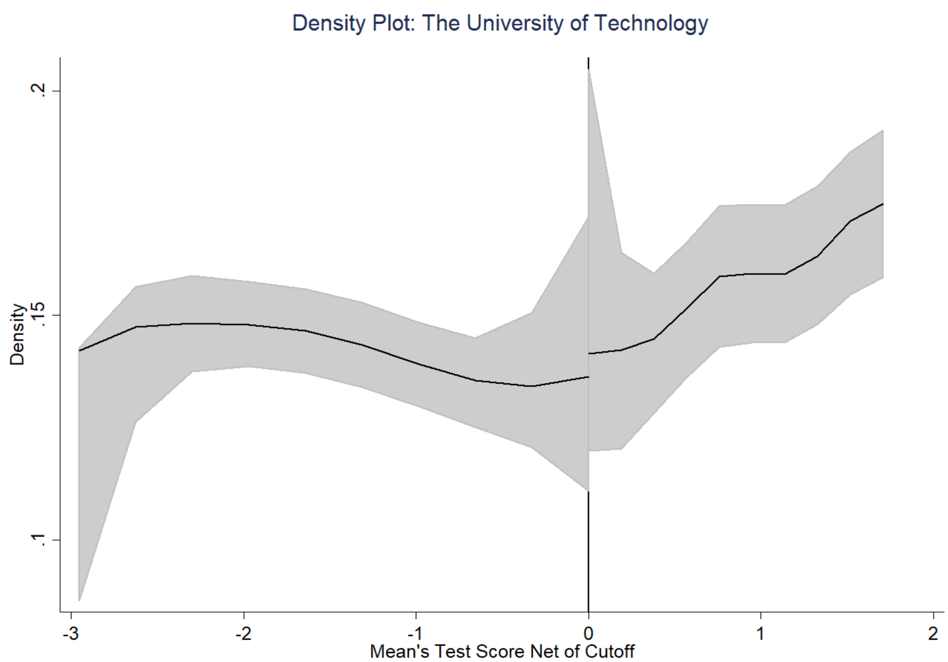


Figure 4: Assignment Score Density Plot (UTECH)

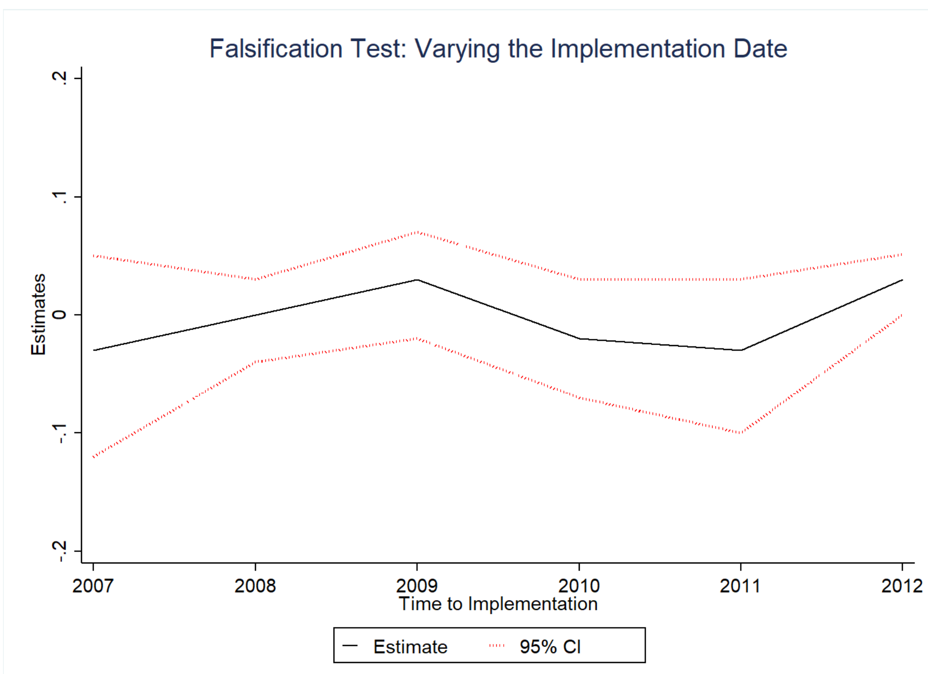


Figure 5: Changing the Implementation Date

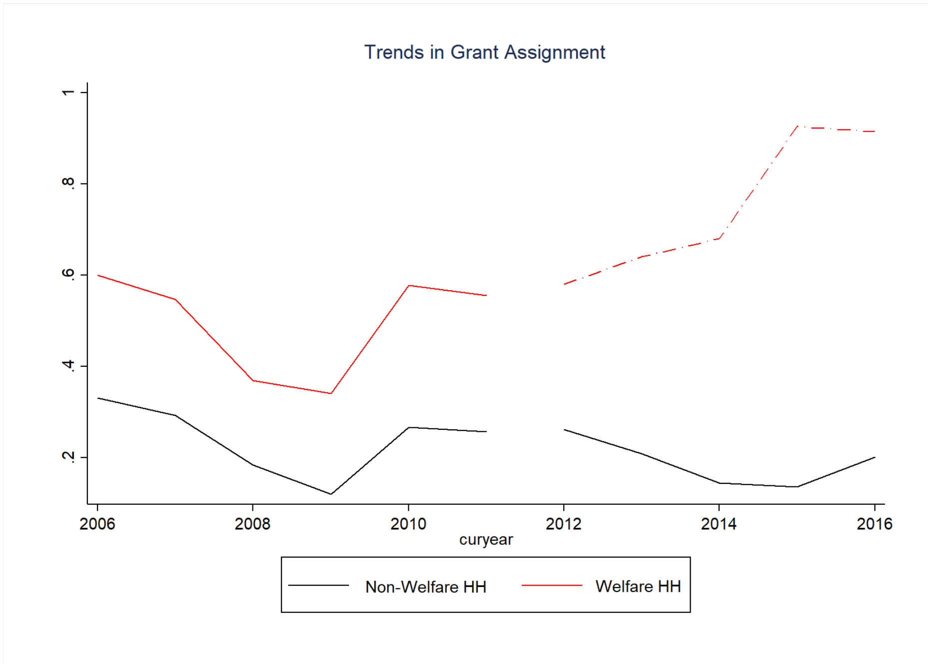


Figure 6: Testing Parallel Pre-Trend Assumption

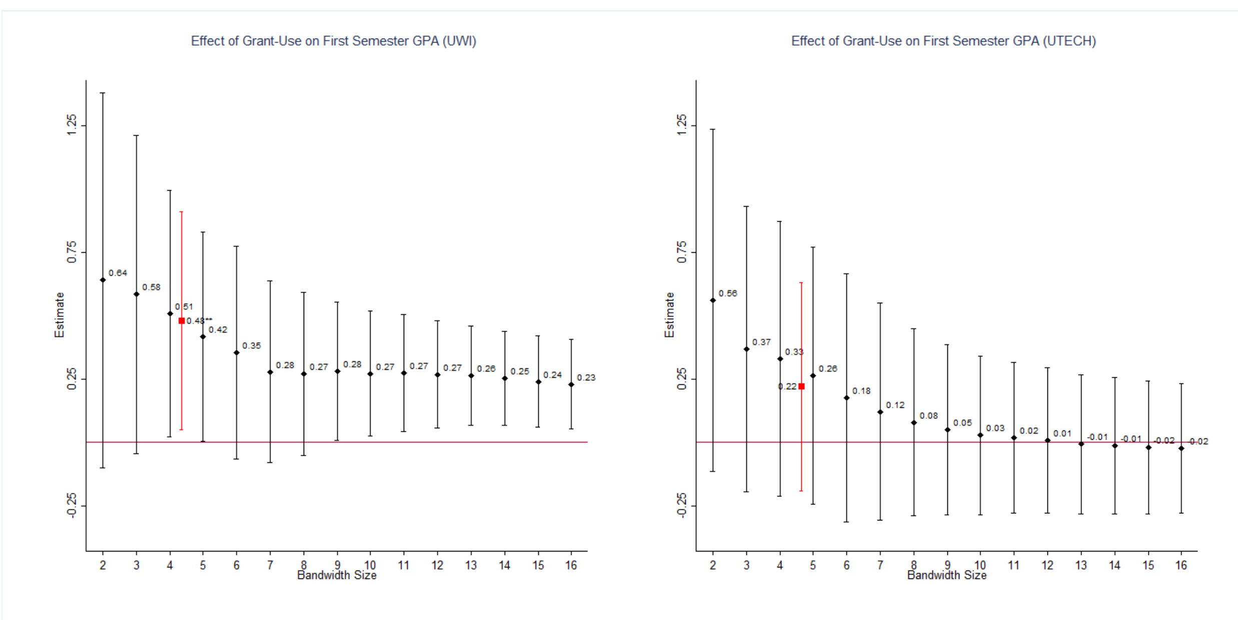


Figure 7: RD Sensitivity Analysis, GPA

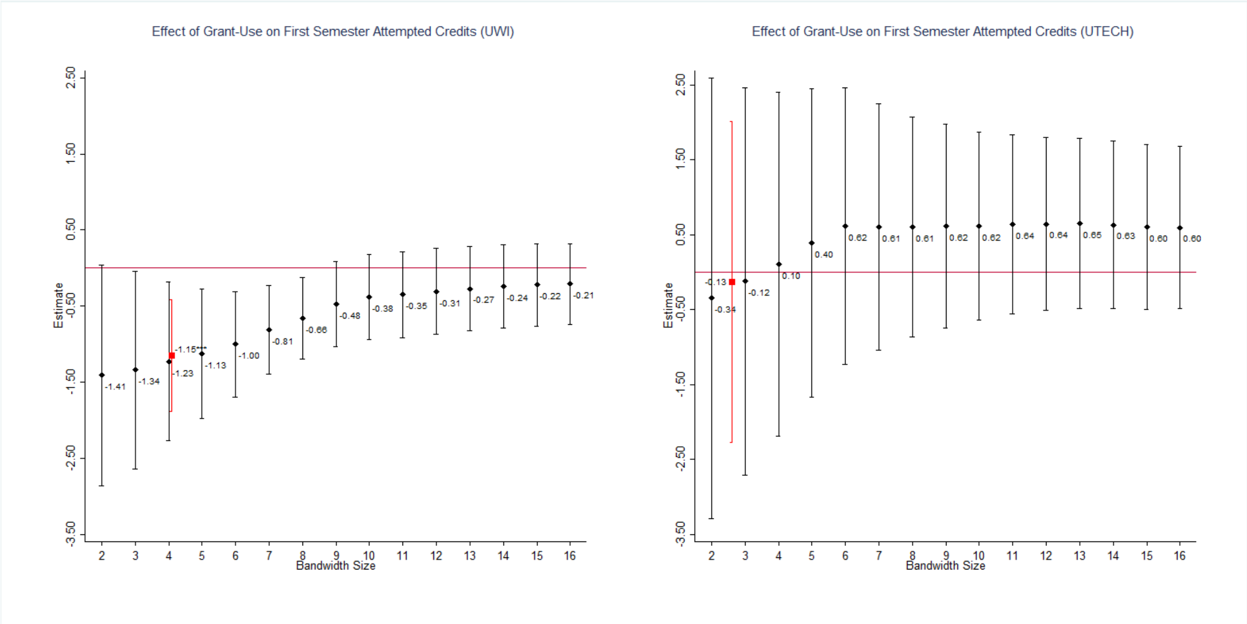


Figure 8: RD Sensitivity Analysis, Attempted Credits

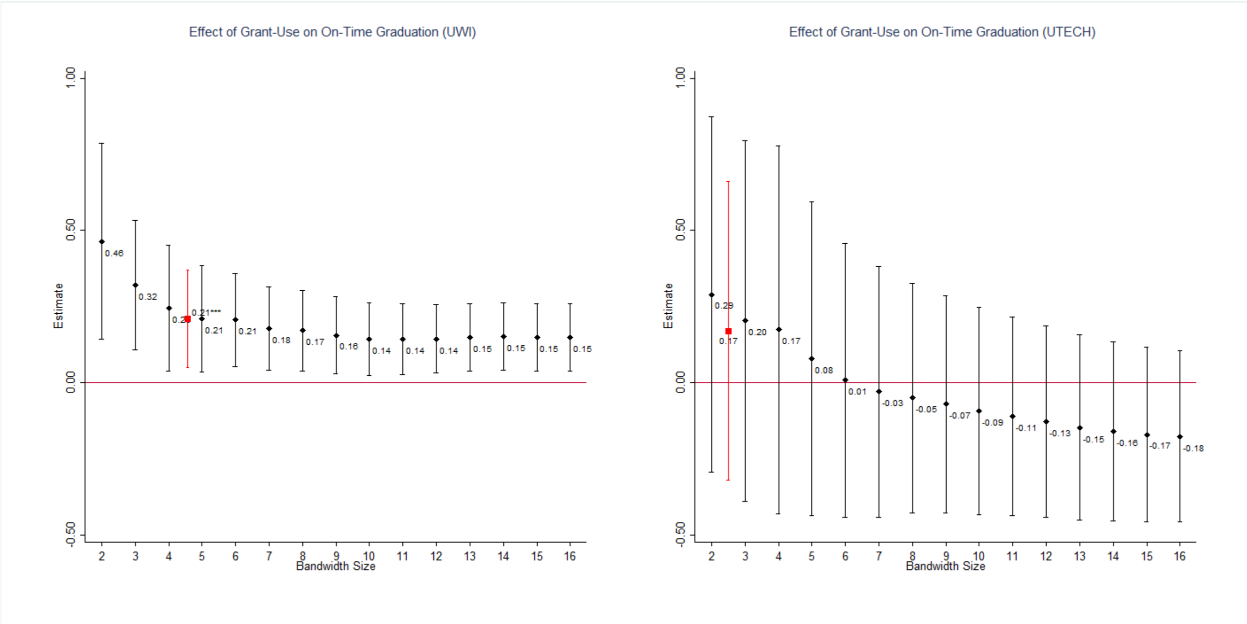


Figure 9: RD Sensitivity Analysis, On-time Graduation

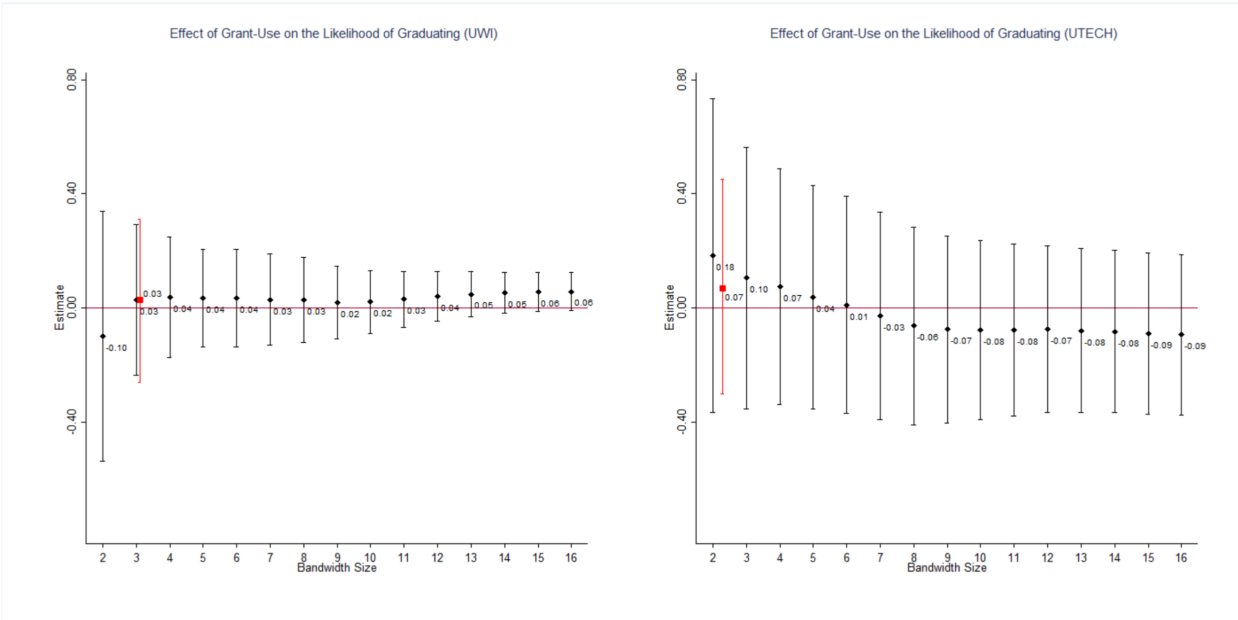


Figure 10: RD Sensitivity Analysis, Any Graduation

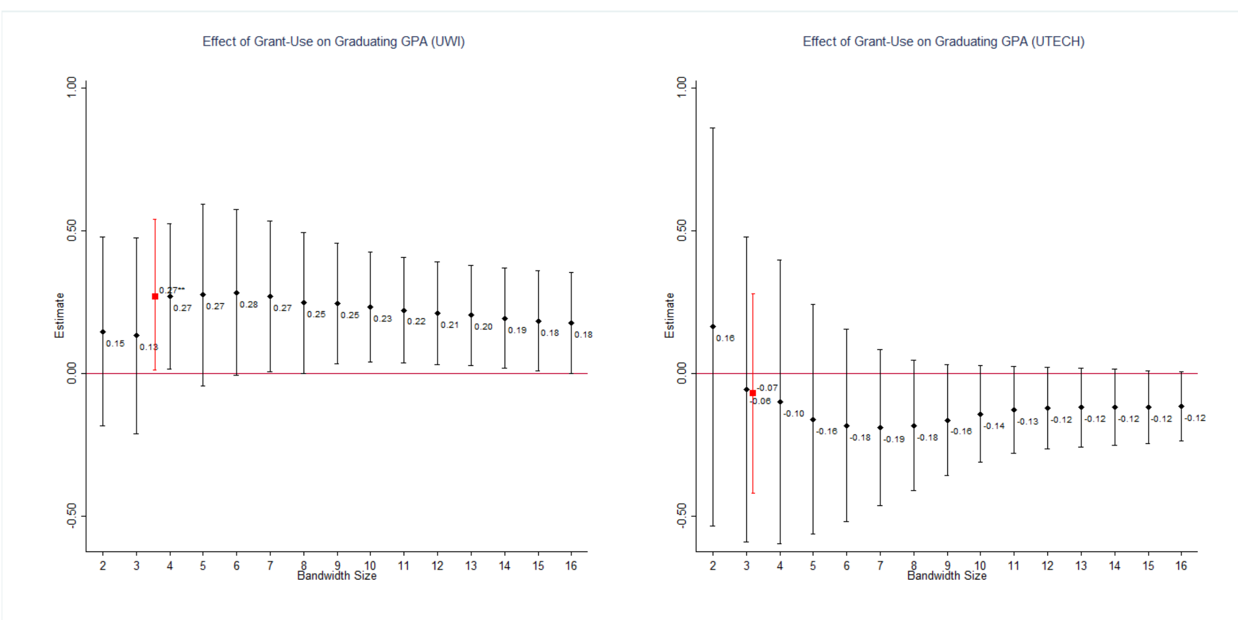


Figure 11: RD Sensitivity Analysis, Grad GPA

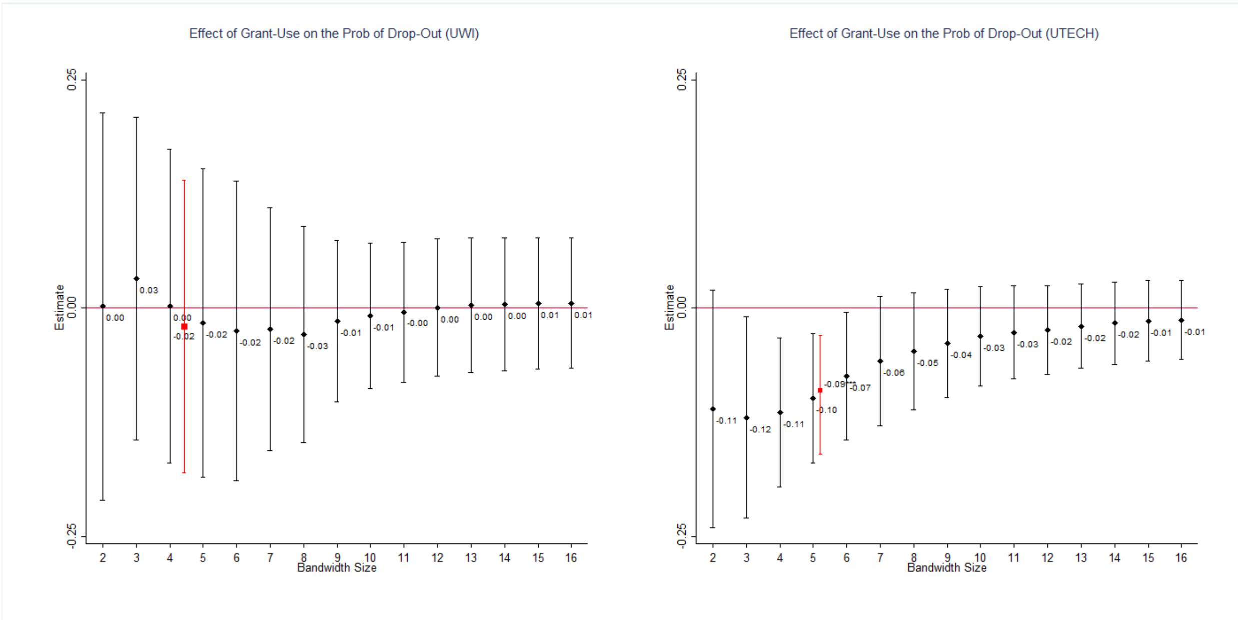


Figure 12: RD Sensitivity Analysis, Drop-out

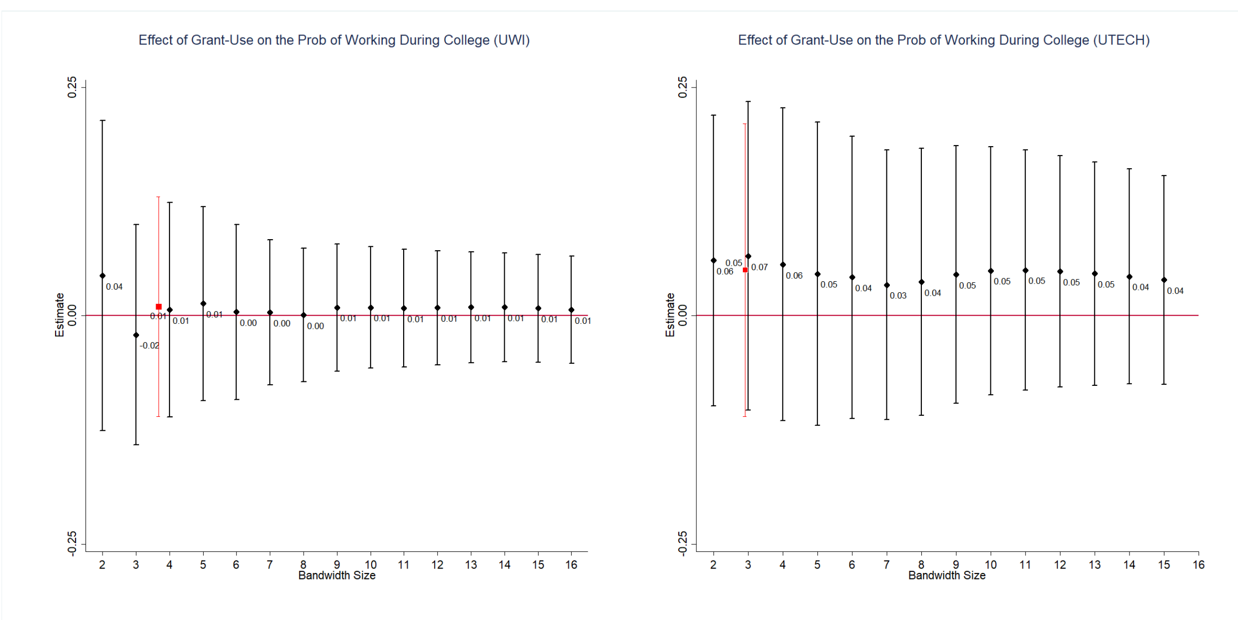


Figure 13: RD Sensitivity Analysis, Employment in College

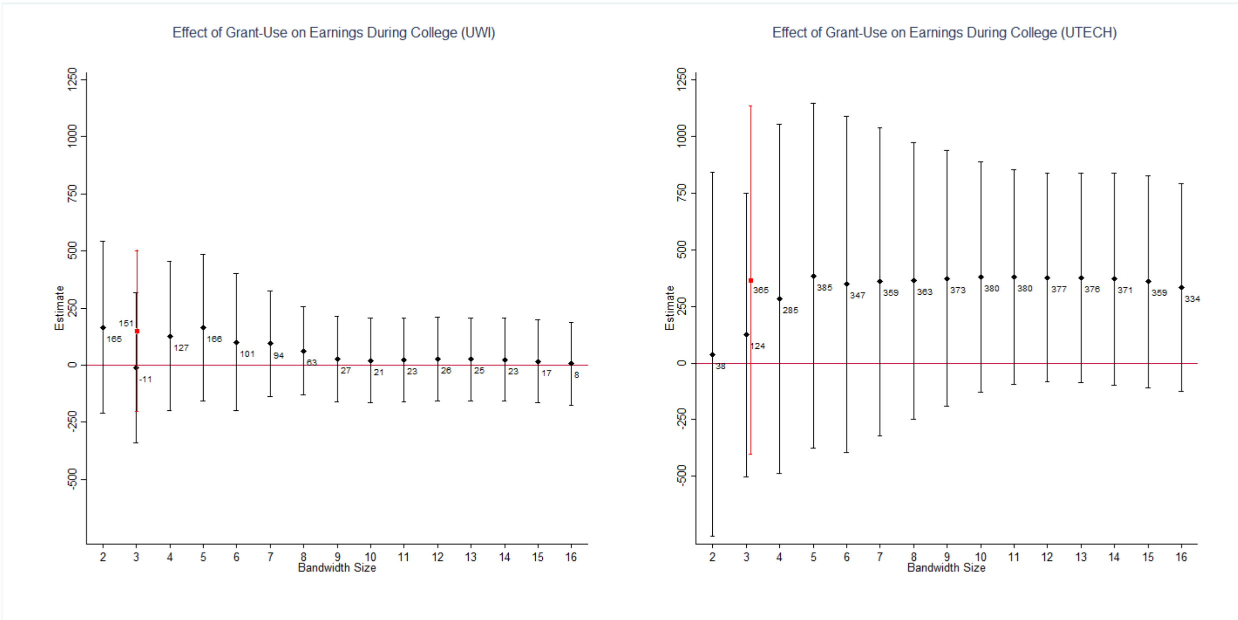


Figure 14: RD Sensitivity Analysis, Earnings in College

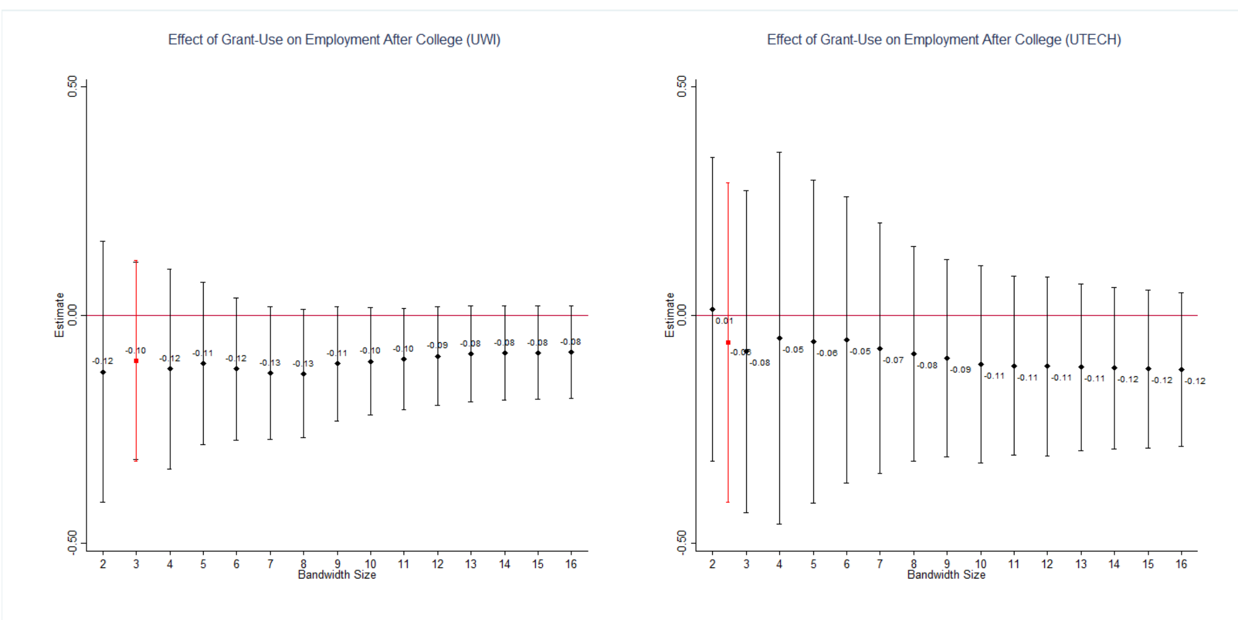


Figure 15: RD Sensitivity Analysis, Employment After College

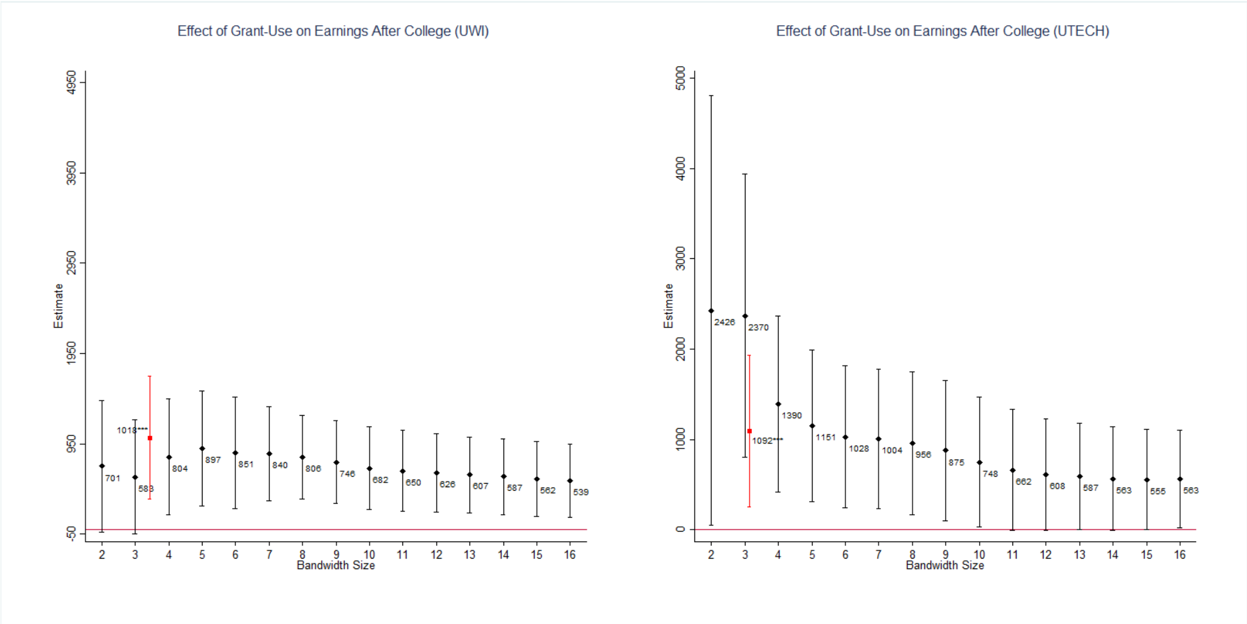


Figure 16: RD Sensitivity Analysis, Earnings After College

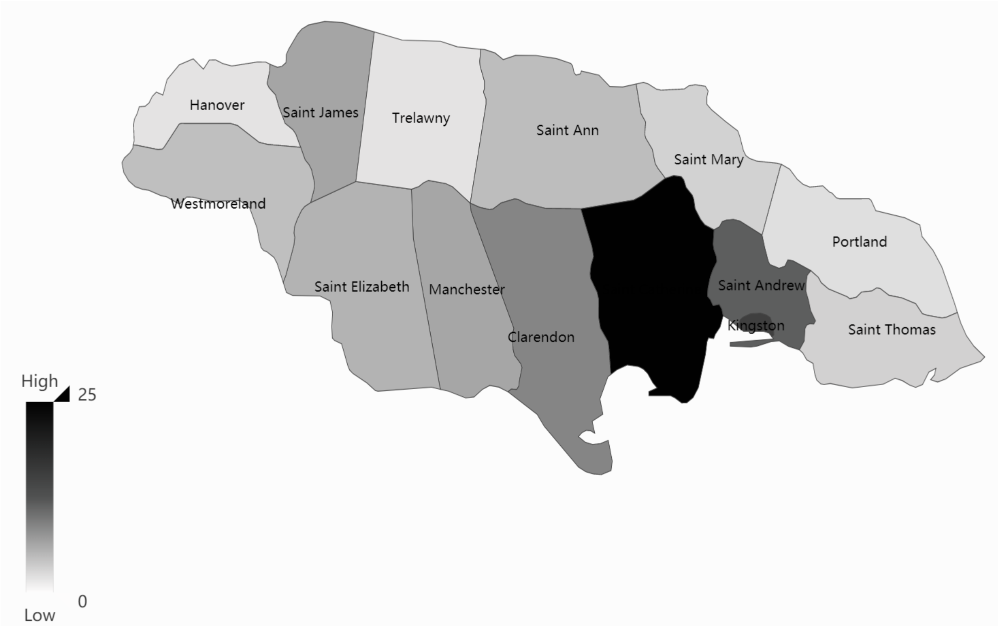


Figure 17: Average Loan-Use

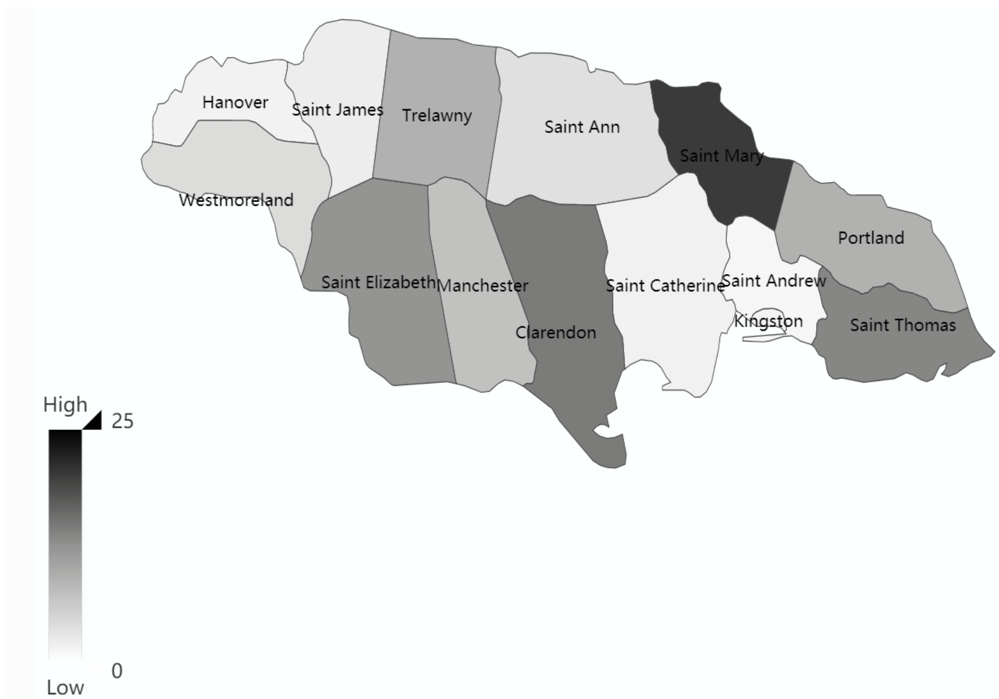
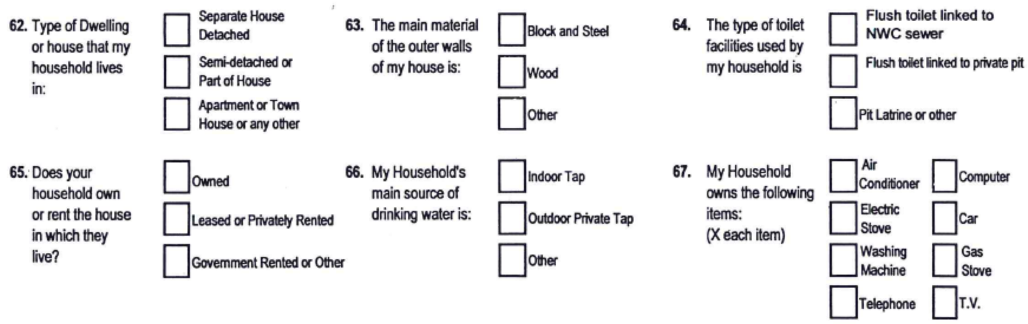


Figure 18: Average HS Visit Exposure

5.4 Appendix A4: Formulating HH Consumption


62. Type of Dwelling or house that my household lives in:
- [ ] Separate House Detached
- [ ] Semi-detached or Part of House
- [ ] Apartment or Town House or any other

63. The main material of the outer walls of my house is:
- [ ] Block and Steel
- [ ] Wood
- [ ] Other

64. The type of toilet facilities used by my household is
- [ ] Flush toilet linked to NWC sewer
- [ ] Flush toilet linked to private pit
- [ ] Pit Latrine or other

65. Does your household own or rent the house in which they live?
- [ ] Owned
- [ ] Leased or Privately Rented
- [ ] Government Rented or Other

66. My Household's main source of drinking water is:
- [ ] Indoor Tap
- [ ] Outdoor Private Tap
- [ ] Other

67. My Household owns the following items: (X each item)
- [ ] Air Conditioner
- [ ] Computer
- [ ] Electric Stove
- [ ] Car
- [ ] Washing Machine
- [ ] Gas Stove
- [ ] Telephone
- [ ] T.V.

Figure 19: Consumption Questions on Application Form

The figure above shows a snippet of the consumption and household characteristics questions asked on the student loan application form. All applicants were required to answer questions 62-67.

Questions are asked about the applicants' housing ownership status, housing type (detached, semi-detached, apartment), housing material(block or wood), sewerage system, water source and appliance ownership (Air conditioner, Computer, Car, Gas Stove, Electric Stove, Washing Machine and Television). Using these reported household attributes, a consumption index is created. For each property the household owns, the consumption index increases by one. The consumption index ranges from 0 (low consumption) to 12 (high consumption), with a mean of 6.2 property ownerships across all applicants.

6 Chapter 2 Appendix

6.1 Appendix B1: Tables

Table A1: Descriptive Statistics

	Mean	Std Deviation	Obs.
Student Characteristics			
Male	0.32	0.46	20,580
Age	19.26	1.50	20,580
Commute	0.69	0.46	20,580
Tuition ($US)	1733.3	2189.8	20,580
CXC Exam	7.84	1.92	20,580
Entry Score	40	15.35	20,580
Outcomes			
Credits	15.52	2.59	20,580
GPA	2.09	1.12	20,580
Loan	0.12	0.32	20,580
Treatment w/n bw of 0.6			
Warning Letter 1st Semester	0.33	0.47	2,175
Dean's List 1st Sem: SS	.35	0.48	1,551
Dean's List 1st Year: MPAS	0.19	0.39	1,545

Table A2: Testing Local Randomization Assumption

	Male	Age	Credits	Commute	Loan	Entry Score	Treat
Dean's List							
Social Sciences (N=625)	-0.08	-0.22	-0.04	-0.02	0.01	-6.92	0.57***
	(0.05)	(0.31)	(0.33)	(0.10)	(0.06)	(6.33)	(0.15)
Mean	0.26	19.18	14.77	0.76	0.09	44.24	0.42
MPA Sciences (N=636)	-0.04	0.17	0.31	0.11	0.06	4.70	0.61***
	(0.12)	(0.47)	(2.07)	(0.10)	(0.07)	(4.16)	(0.11)
Mean	0.34	19.26	17.72	0.67	0.10	49.24	0.26
Warning Letter							
C=0.75 & 1 (N=1,013)	0.01	-0.10	0.44	-0.01	-0.07	1.68	0.96***
	(0.08)	(0.09)	(1.04)	(0.09)	(0.05)	(2.69)	(0.05)
Mean	0.37	18.89	15.68	0.68	0.09	38.14	0.36

Table A3: Impact of the Dean's List Policy

	Cum GPA	Dean List	Credits	Loan Use
Social Sciences (N=2,723)				
	0.36***	0.22***	-0.08	-0.29***
	(0.08)	(0.08)	(0.25)	(0.07)
Control Mean	3.23	0.26	14.83	0.11
SD	0.34	0.49	2.33	0.35
MPA Sciences (N=2,157)				
	-0.01	-0.06	2.75*	-0.02
	(0.07)	(0.14)	(1.67)	(0.12)
Control Mean	3.42	0.32	18.96	0.16
SD	0.26	0.47	4.97	0.37

Table A4: Policies Impact on Course Selection Behavior

	A-rate	B-rate	C-rate	D-rate	P-rate	Easy (A,B)	Easy(P-rate)
Dean's List							
Social Sciences	0.02	0.05***	0.01	-0.04***	0.05***	0.15***	0.21***
	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
MPA Sciences	-0.03	0.07	-0.06	-0.07	-0.09	0.11	-0.01
	(0.08)	(0.10)	(0.08)	(0.07)	(0.11)	(0.25)	(0.18)
Warning Letter							
C=0.75, 1	-0.06***	-0.01	0.03	0.04***	-0.004	-0.02	-0.01
	(0.01)	(0.02)	(0.02)	(0.01)	(0.03)	(0.04)	(0.03)
C=2	0.002	0.02	0.01	-0.003	0.02**	0.05	0.09**
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.05)	(0.04)

Table A5: The Impact on Long-Term Outcomes

	Grad Ontime	Time to Graduation (Mths)
Dean's: SS	0.09	-0.89
	(0.09)	(1.28)
Dean's: MPAS	-0.08	5.94
	(0.31)	(6.51)
Warn	-0.06	0.10
	(0.11)	(2.62)

Table A6: Impact of the Academic Warning Policy

	Cum GPA	Warning	Credits	Major	Transfer	Exit
C=0.75 & 1 (N=3,210)	0.16	0.01	-0.47	0.10**	0.08**	0.11**
	(0.14)	(0.06)	(0.57)	(0.05)	(0.04)	(0.04)
Control Mean	1.87	0.21	15.50	0.08	0.02	0.06
SD	0.58	0.41	3.08	0.27	0.12	0.24
C=2 (N=2,600)	0.13*	-0.03	0.02	0.04	0.06	0.06
	(0.07)	(0.08)	(0.56)	(0.07)	(0.07)	(0.06)
Control Mean	2.22	0.20	15.80	0.08	0.04	0.19
SD	0.38	0.40	4.01	0.27	0.19	0.39

Table A7: RD Robustness: Dean's List Policy

	CGPA	Dean List	Credits	Loan Use	Obs
Social Sciences					
BW of 0.2	0.39***	0.20**	-0.14	-0.36***	2436
	(0.09)	(0.09)	(0.28)	(0.07)	
BW of 0.3	0.30***	0.21***	-0.18	-0.21**	3485
	(0.09)	(0.07)	(0.23)	(0.10)	
BW of 0.4	0.22**	0.15**	-0.24	-0.14	4523
	(0.09)	(0.08)	(0.19)	(0.10)	
BW of 0.5	0.12	0.11	-0.22	-0.06	5944
	(0.09)	(0.07)	(0.17)	(0.10)	
BW of 0.6	0.09	0.09	-0.14	-0.05	7098
	(0.09)	(0.07)	(0.15)	(0.09)	
MPA Sciences					
BW of 0.2	0.07	0.03	2.76	-0.05	1356
	(0.08)	(0.19)	(2.08)	(0.15)	
BW of 0.3	-0.01	-0.06	2.75*	-0.02	2145
	(0.07)	(0.14)	(1.67)	(0.12)	
BW of 0.4	-0.01	-0.04	2.97*	0.001	3084
	(0.07)	(0.13)	(1.57)	(0.12)	
BW of 0.5	-0.01	-0.03	3.04*	0.01	4116
	(0.07)	(0.13)	(1.56)	(0.12)	
BW of 0.6	-0.01	-0.03	3.08**	0.01	5271
	(0.07)	(0.13)	(1.55)	(0.11)	

Table A8: RD Robustness: Academic Warning Policy, GPA Threshold of 0.75

	CGPA	Warn	Credits	Major	Transfer	Exit	Obs
BW of 0.2	0.14	0.007	-0.74*	0.03	0.08**	0.09**	2313
	(0.11)	(0.06)	(0.44)	(0.05)	(0.03)	(0.04)	
BW of 0.3	0.16	0.02	-0.51	0.06	0.08*	0.09**	3210
	(0.14)	(0.06)	(0.59)	(0.05)	(0.04)	(0.04)	
BW of 0.4	0.16	0.03	-0.36	0.08	0.08*	0.08**	4576
	(0.14)	(0.06)	(0.57)	(0.06)	(0.04)	(0.04)	
BW of 0.5	0.17	0.03	-0.24	0.06	0.08**	0.10***	5845
	(0.12)	(0.06)	(0.51)	(0.05)	(0.04)	(0.04)	
BW of 0.6	0.16	0.03	-0.22	0.05	0.07**	0.11***	6977
	(0.10)	(0.05)	(0.45)	(0.05)	(0.04)	(0.03)	

Table A9: RD Robustness: Academic Warning Policy, GPA Threshold of 2.0

	CGPA	Warn	Credits	Major	Transfer	Exit	Obs
BW of 0.2	0.09	-0.02	-0.166	-0.03	0.04	0.02	1746
	(0.09)	(0.10)	(0.69)	(0.10)	(0.09)	(0.09)	
BW of 0.3	0.13*	-0.03	0.02	0.04	0.06	0.06	2600
	(0.08)	(0.08)	(0.56)	(0.07)	(0.07)	(0.06)	
BW of 0.6	0.11**	-0.01	0.36	0.08*	0.07*	0.06	5332
	(0.06)	(0.06)	(0.38)	(0.04)	(0.04)	(0.04)	
BW of 1.0	0.10**	0.001	0.26	0.04	0.04	0.08**	8451
	(0.05)	(0.04)	(0.30)	(0.04)	(0.03)	(0.04)	

6.2 Appendix B2: Figures

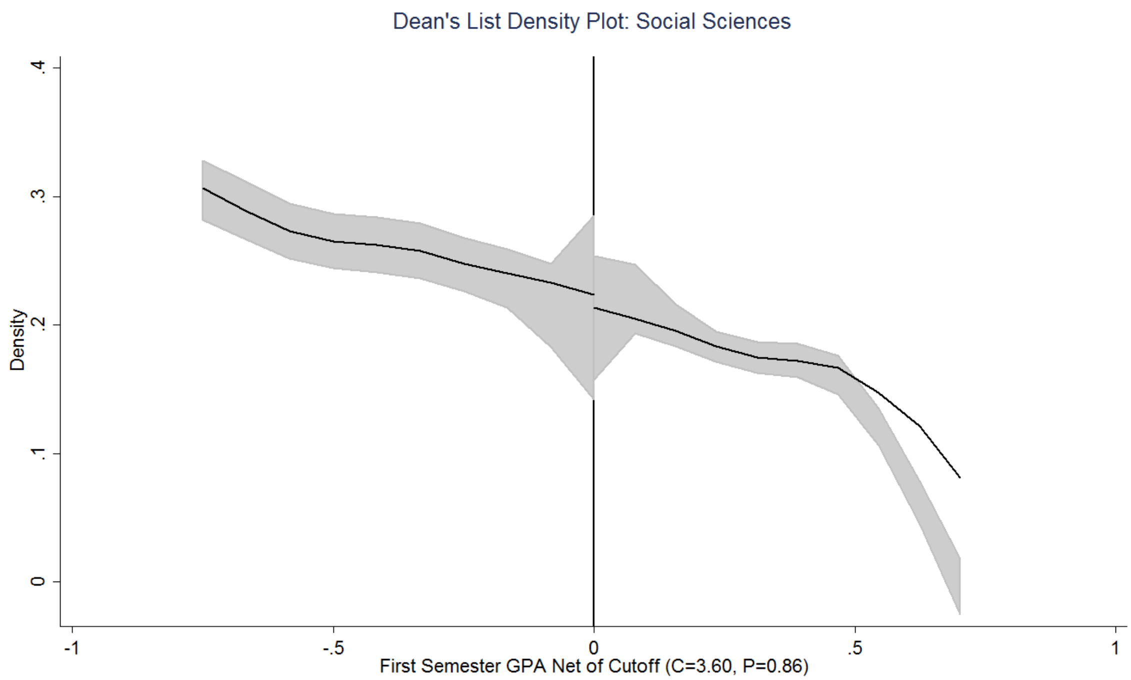


Figure 20: Social Sciences, RD Manipulation Plot

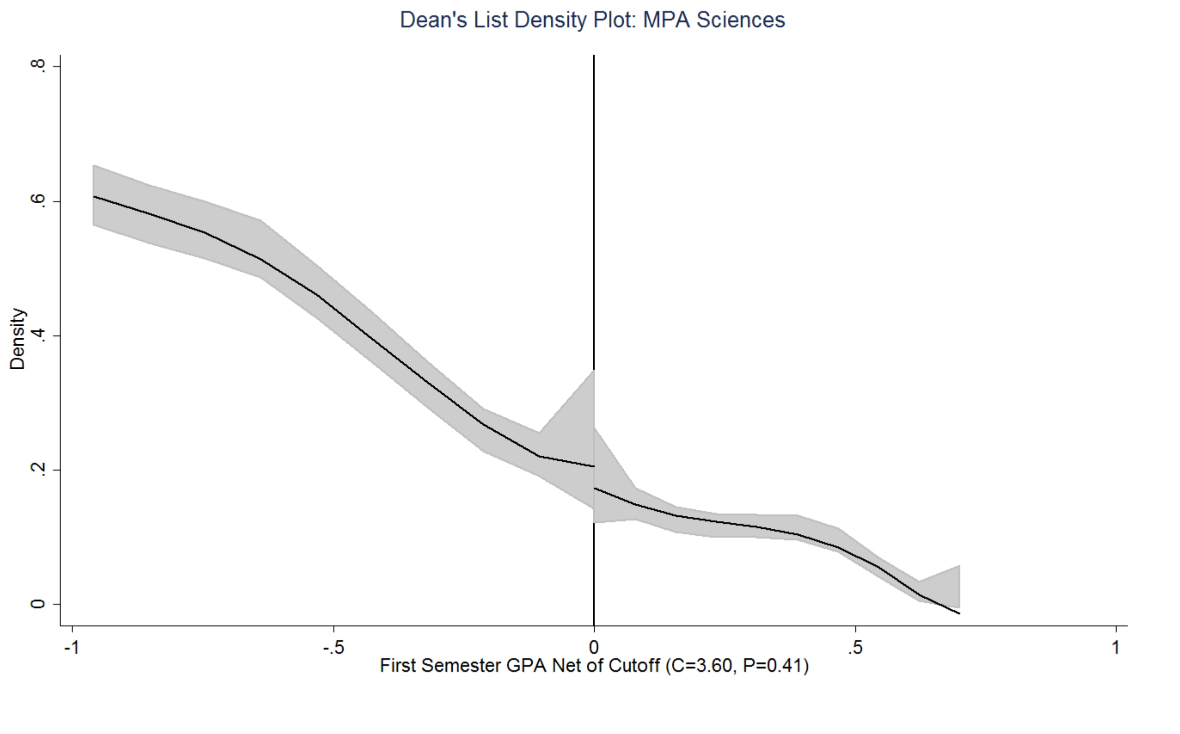


Figure 21: Medical, Pure and Applied Sciences; RD Manipulation Plot

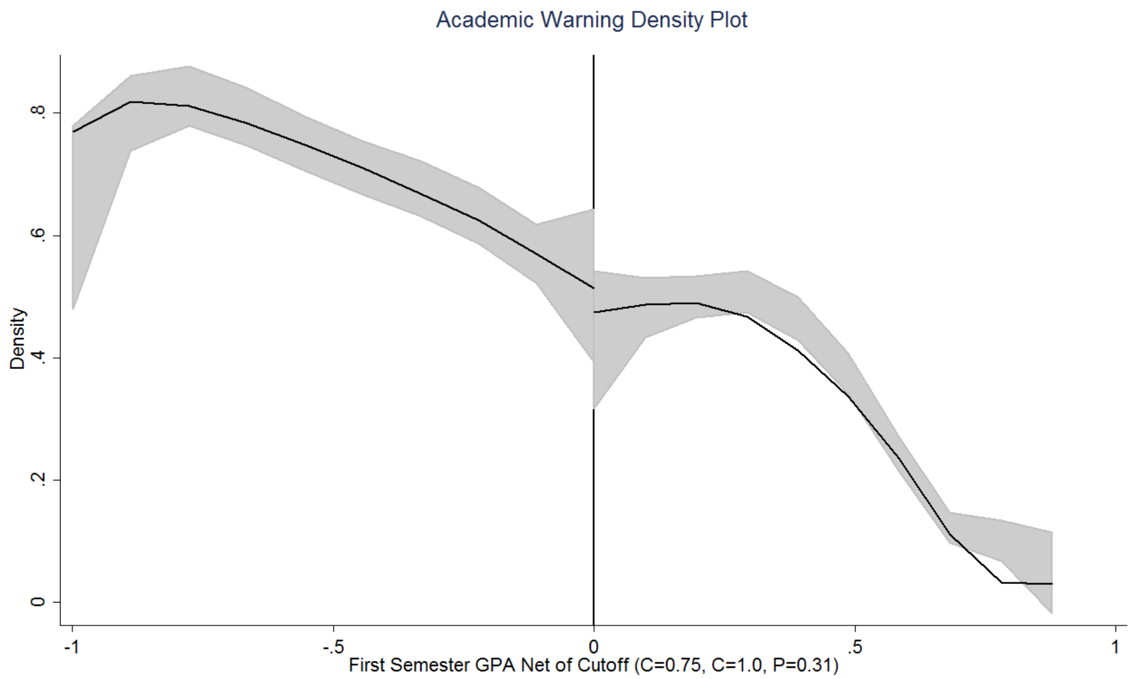


Figure 22: Warning Letter, RD Manipulation Plot

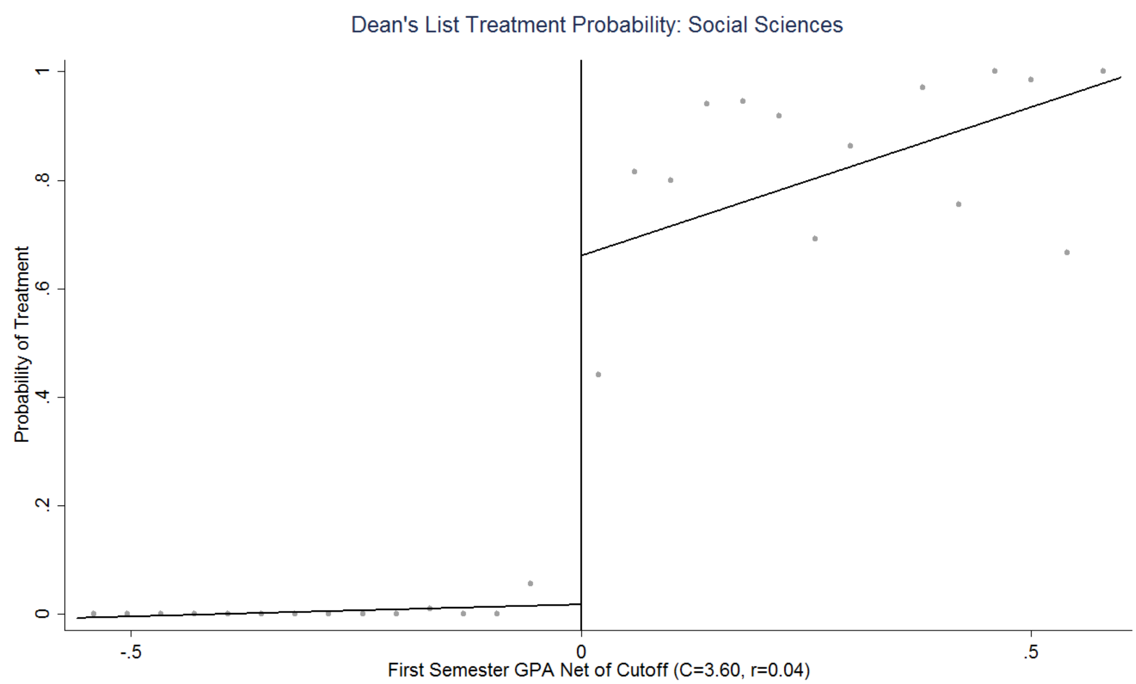


Figure 23: Social Sciences, Discontinous Jump Plot

(Each dot represents the fraction of students treated within evenly spaced GPA bins)

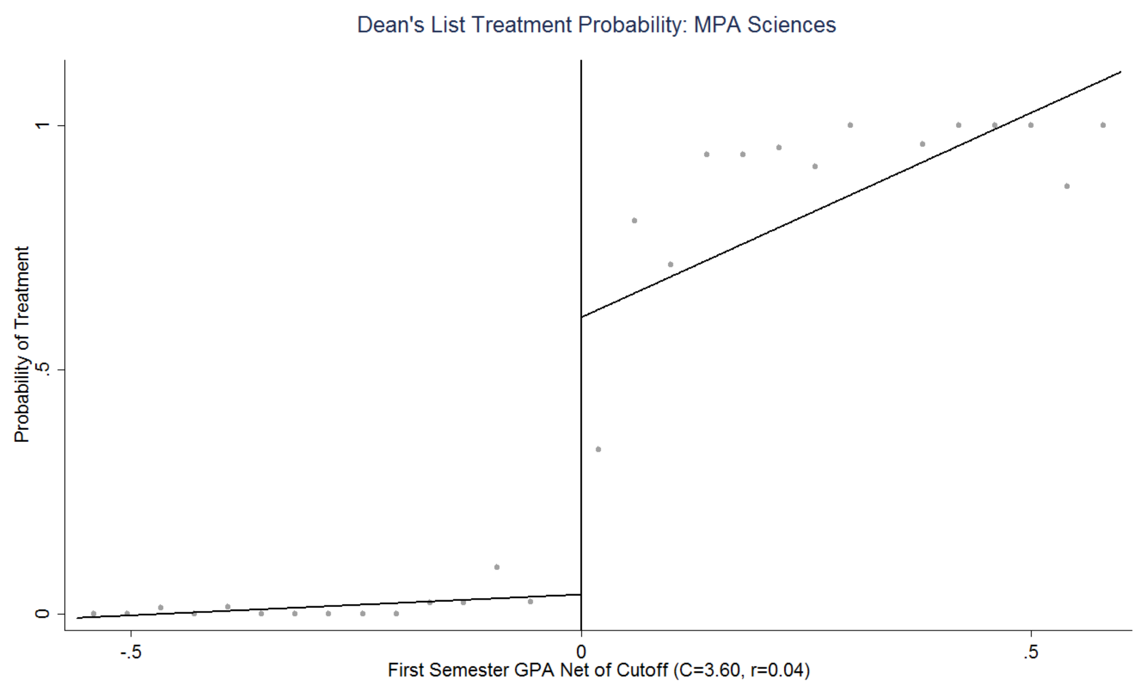


Figure 24: Medical, Pure and Applied Sciences; Discontinous Jump Plot

(Each dot represents the fraction of students treated within evenly spaced GPA bins)

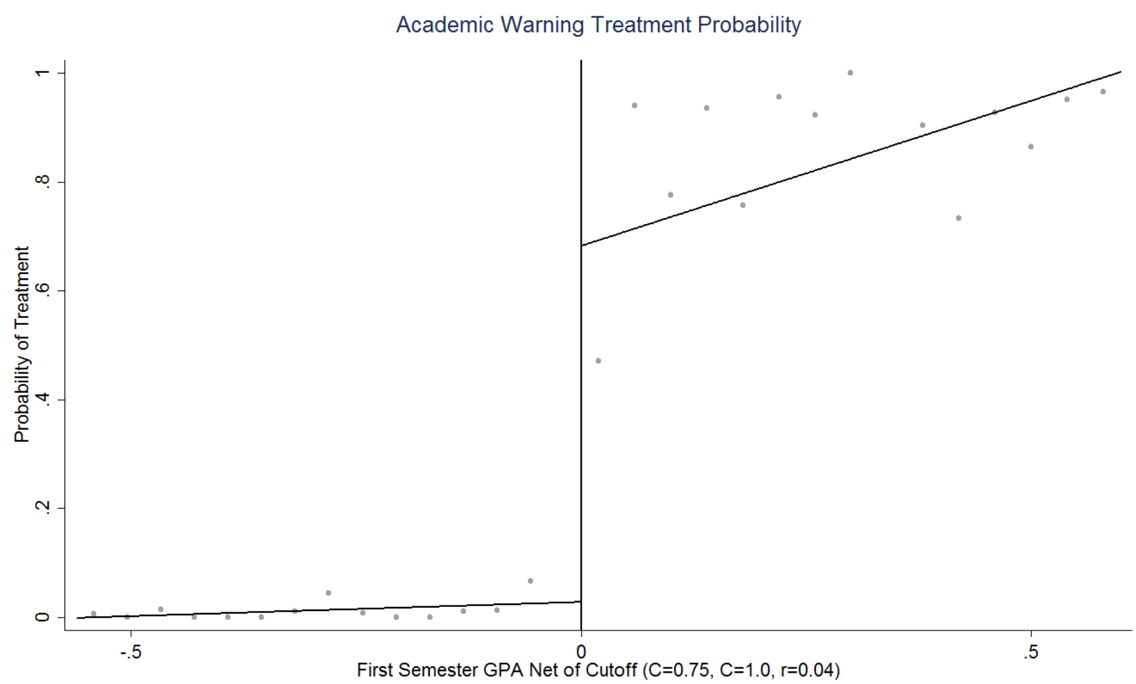


Figure 25: Warning Letter, Discontinous Jump Plot

(Each dot represents the fraction of students treated within evenly spaced GPA bins)

6.3 Appendix B3: Warning Letter

Dear { Name } ,

This is to inform you that the Board of Examiners in accordance with Faulty Regulations and the University Examination Regulations, considered your examination results for the first Semester of the 2016/2017 academic year and concluded that your performance was unsatisfactory and may indicate serious weakness in your academic preparation. The Grade Point Average system (GPA), Regulation xxx indicates that "a student whose GPA for a given semester is less than 2.00 shall be deemed to be performing unsatisfactorily, and shall be placed on WARNING. A student on warning whose GPA for the succeeding semester is less than or equal to 1.99, will be required to withdraw".

You are hereby notified that as a consequence of your academic performance, you are on now on WARNING and this is reflected on your academic record which may be viewed online. Please be aware that such performance, if repeated, would lead to your being asked to withdraw from the Faculty.

In order not to jeopardize your academic future, I am strongly recommending that if there are any significant academic or personal problems preventing you from functioning to the best of your ability, that you seek assistance by contacting your academic advisor, accessing the study skills programme offered by the Academic Support Unit, or utilizing appropriate counseling programme offered at the Health Centre.

We trust that you will take the necessary steps to rectify the situation and wish for you all the best in your future studies.

Yours sincerely,

{ Name }

Campus Registrar

VITA

Nicholas Wright hails from the rural parish of Clarendon in Jamaica. He earned a Bachelor of Science in Economics and Political Science and a Master of Science in Economics from the University of the West Indies. He has also obtained a Ph.D in Economics from Georgia State University.

His research utilizes quasi-experimental methods to study issues in education, labor, and public economics, with a primary focus on higher education policies, experimental interventions, and student financing. His most recent works utilize administrative data from Jamaica to examine the impact of need-based financing policies, performance standards, and public recognition on students' post-secondary decisions and labor market outcomes.

During his graduate studies at Georgia State, Nicholas has served as a research assistant at the Federal Reserve Bank of Atlanta and taught courses in econometrics and the economics of poverty and public policy. Within the last five years, he has received several accolades including the Prime Minister Youth Award for Excellence in Academics from the Government of Jamaica; the National Academy of Education/Spencer Dissertation Fellowship; and the Quantitative Economics Award, the Jack Blicksilver Scholarship, and the Outstanding Graduate Research Assistant Award from Georgia State University. His research has also been recognized with the 2019 New Scholar Award by The Association for Education Finance and Policy.

Nicholas will join the faculty at Florida Gulf Coast University as an Assistant Professor in Economics in fall 2019.