The Influence of Price Changes on Enrollment: Research Designs to Examine Price Responsiveness in Graduate and Professional Schools

by

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A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Higher Education)
in the University of Michigan
2023

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Dedication

First, this dissertation is dedicated to my wife, Annie, and my daughter, Eleanor. I love you both so much and could not have done this without your love, support, and endless patience. This dissertation is also dedicated to the rest of both of our families. Without their regular check-ins and *free* childcare, this endeavor would not have been possible. To my father, Tim, thank you for everything, but most importantly for being the rational voice I needed to keep me grounded throughout this process, holding me accountable by always asking how it was coming along (and genuinely being interested!), and showing me what it means to work hard and never give up, no matter how difficult it gets. Finally, this dissertation is dedicated to my dear mother, Judy, who I lost along this journey and who I miss every single day. Her passion and energy for education, constant stream of love and encouragement, and the excitement that I know she would have experienced watching me conclude this milestone are what gave me life and propelled me across this finish line, and for that, I am forever grateful.

Acknowledgements

This dissertation is the product of multiple support systems who have each played an integral role in this process. Completing this paper was a constant balancing act between family, school, and work. Different times of the year required a different division of my time, yet everyone involved not only understood, but cheered me on. Thank you all for allowing me to shift priorities so seamlessly in order to continue making progress personally, professionally, and academically.

First, to my dissertation chair, Stephen DesJardins, who from our very first conversation understood my goals and actively worked to help me achieve them. For example, shortly into our first meeting after starting the program, he asked me what my professional goals were. I recall outlining rather lofty goals with an ambitious timeline, to which he responded, "Alright. Let's do it." At the time, I had little sense of what that meant or how I would actually make anything happen, but I sensed that he was sincere about supporting me and helping me achieve my goals. From that point forward, he has been an amazing mentor and supporter of my academic and professional interests. When a full-time, professional opportunity arose for me, he was my strongest supporter, even though it meant losing a member of his research team. Even after accepting the position, we kept a regular bi-weekly meeting, no matter the country or time zone from which he was calling. Steve, I am forever grateful for your mentorship and belief in me, for being a straight shooter, and for always having my best interests at heart. Go Lions.

To my dissertation committee, I truly appreciated your support and constant availability for questions on short notice and at odd times of the day as I juggled full-time work and

dissertation writing. I could not have arrived at this point without your helpful guidance and feedback. Each of you were so approachable and never once made me feel as though any question I had was too rudimentary for this stage of my education (which, I am sure, was probably tough at times), so thank you all so much for that.

To my family, who have unconditionally supported this goal of mine and made countless sacrifices to help me accomplish it. First and foremost, to Annie, I could not have ever done this without you, and your unwavering support and understanding throughout has been nothing short of incredible. Over these last few years, you have made countless sacrifices and spent many weeknights and long weekend days solo-parenting, all so that I could find the time I needed to continue making progress on this dissertation. I appreciate so much everything that you have done to make this possible. I could not have asked for a better partner in life – I love you, and I promise to spend my new free time adding to my four-recipe arsenal.

To Eleanor, even though you joined this dissertation race halfway to the finish line, you have already provided me a lifetime’s worth of smiles and special moments. Someday, if you ever read this, I appreciated your patience and understanding when “Daddy had to work.” I have really enjoyed spending time as your “co-worker” as we both type away on our Word documents. I love you!

To my extended family, thank you to Kathy, Dave, and Laura for always being available to drop in at a moment’s notice for childcare and for asking about my dissertation. Collectively, you have all provided me with so much more availability to work on this paper, and I am extraordinarily grateful for it. Thank you, as well, for always making sure we find time to have fun as a family, especially on our trips Up North, which have always managed to recharge my motivation.

Thank you to my dad (Tim), Nick, Angie, Sarah, CJ, Drew, and Kalei for tolerating my busy schedule, especially over this past year. I am looking forward to having more free weekends to make the drive north to spend some time grilling out and watching sports. I appreciate all of you asking about my progress ("Aren't you done yet!?") and checking in after big dates and deadlines. In all seriousness, thanks for understanding and for all your support throughout this process.

Thank you to Sarah, who has been the most flexible, understanding, and supportive boss anyone could have. From the moment I accepted the job, you wanted to know what you could do to help ensure I continued to make progress on my dissertation. When I have needed time off after long bursts of writing, you encouraged me to take it. When important dissertation dates and deadlines were approaching, you checked in and made sure things were going alright and that I had the time I needed to not overextend myself. I could not have done this balancing act without your support. Thanks for your understanding and your many jokes along the way.

To my friends, all of whom have been incredibly supportive and have had endless amounts of patience as I made my way toward the finish line. Thanks for always asking about my progress, and I promise I will be able to make it to more football games in these next few years. Thank you, as well, to Chelsey for scheduling Zoom writing sessions with me over this past year to help keep me accountable—I certainly appreciated it!

Last, but certainly not least, thank you to my colleagues in the financial aid profession. I have enjoyed being a part of this professional community and having the opportunity to learn the profession from some of the very best aid professionals in the country. Thank you, especially, to Jim Eddy, who taught me the ins and outs of institutional and federal financial aid and encouraged my growth in the profession. Thank you, as well, to the Office of Student Financial

Aid at the University of Iowa, which gave me the opportunity to begin my financial aid career as a graduate assistant, which spurred the passion and curiosity that led to this dissertation.

Table of Contents

List of Tables

List of Figures

List of Appendices

Abstract

A number of studies have examined the impact of changes in price on students' enrollment decisions at the undergraduate level, but little is known about the price responsiveness of graduate and professional students. This dissertation utilizes difference-in-differences and regression-discontinuity methods to analyze student-level data for admitted students at a graduate/professional school at a highly selective public institution. Difference-in-differences is employed to evaluate the impact of a change in the scholarship name of an existing institutional financial aid program on immediate enrollment at the institution, while regression-discontinuity is utilized to evaluate the impact of merit scholarships on the same outcome. These causal research designs aim to provide more robust institution-level findings regarding the various ways in which financial aid can impact graduate/professional student enrollment decisions. The results indicate that the change in scholarship name did not induce increases in enrollments. Small and medium scholarship amounts did have an impact on enrollment decisions among high-test score and high-GPA students, respectively. Compared to no scholarship offer, admitted students receiving a small- or medium-level merit scholarship were much more likely to enroll. Also examined was the impact of the scholarship receipt among important subgroups of students, such as by race/ethnicity, sex, and residency status. This research adds to the body of student price responsiveness literature by providing the first rigorous analyses of the impact of institutional scholarships on graduate/professional school enrollment. In light of increases in graduate/professional school enrollment broadly, this paper serves as a framework for enrollment management administrators and financial aid professionals to conduct assessments of their own programs and leverage institutional aid programs.

Keywords: Price discounting, tuition, enrollment management, financial aid, regression discontinuity design, difference-in-differences methods, graduate school enrollment

Chapter 1 Introduction

Over the past few decades, much time and attention has been given to research examining the impact of financial aid on undergraduate students' enrollment decisions. However, there is still very little known about the impact of finances on graduate and professional school enrollment. Between fall 2009 and fall 2020, undergraduate enrollment decreased by five percent (from 17.5 to 15.9 million students), while graduate and professional enrollment increased by ten percent (from 2.8 to 3.1 million students) and is projected to grow six percent (to 3.3 million students) more by fall 2030 (National Center for Education Statistics [NCES], 2022). The diverging trends between undergraduate and graduate enrollment underscore the urgency to learn more about graduate school enrollment decision-making. As more undergraduates apply to continue their education after graduation, administrators at graduate and professional schools will need to be more strategic about who they are enrolling and at what cost.

Research on undergraduate enrollment indicates that changes in net price[1] influence students' decisions to enroll in postsecondary education (Dynarski et al., 2022; Heller, 1997; Kim, 2010; Leslie & Brinkman, 1987). The net price that a student faces when enrolling in college is directly affected by the use of enrollment management[2] (EM) strategies such as tuition

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[1] Net price is equal to the listed (or sticker) price of an institution minus financial aid. However, not all types of financial aid are identical (i.e., grants are "free money," loans require repayment, and work-study requires employment). Net price is defined differently in the literature and could be conceived by a student as being the "sticker price" less grant aid, or the "sticker price" less immediately available financial aid (grant and loan aid), or the "sticker price" less all financial aid (i.e., grant, loan, and work-study).

[2] EM models function as a way for an institution to achieve enrollment goals through strategic financial aid use, manage budgets and revenue through tuition-pricing strategies, and better enable achievement of its institutional positioning goals in the marketplace (Hossler, 2000).

increases/decreases, financial aid increases/decreases, or both. A critical review of prior research suggests that undergraduate students of different backgrounds respond differently to changes in tuition and financial aid (St. John, 1990), that aid provision at the graduate level affects career choices (Field, 2009), and non-monetary changes, such as financial aid naming conventions (Avery & Hoxby, 2004) also affect graduate student enrollments. Yet, there remains a lack of research on the impact of EM strategies on graduate and professional student enrollments, as well as uncertainty about the extent to which graduate students consider the same factors as undergraduates in their decision-making, given the diverging enrollment trends. This dissertation aims to fill that gap in the literature by examining EM strategies at a single, competitive professional school.[3] Disentangling the effects of institutional pricing and marketing approaches on graduate students' enrollment decisions broadly, as well as among various subgroups, will not only enhance the literature base but also help establish a benchmark for evaluating the transferability of findings on undergraduate enrollment. In addition, it will provide a framework for EM professionals at the graduate and professional level to evaluate and leverage EM to craft student cohorts and achieve EM goals.

Structured as a single paper, this dissertation examines the impact of multiple EM strategies implemented at a graduate school. The strategies analyzed include examining the effects on enrollments of (1) renaming a generic grant to a named scholarship and (2) examining the effect of the provision of threshold-based merit aid. The analysis noted in (1) is conducted by employing a difference-in-differences (DID) approach to examine the impact on enrollment decisions of a change in the naming convention of an institutional award (i.e., changing the name from "grant" to "scholarship"). To undertake (2), a regression discontinuity (RD) approach is

[3] Hereafter this school will be referred to as "graduate" school for ease of discussion of professional and graduate schools.

utilized to examine the impact on enrollment decisions of threshold-based merit aid offers made to students based on their test scores and undergraduate grade point averages. Where sample size allows, heterogeneous treatment effects are examined, including (but not limited to) studying any differences in the effect of the aforementioned EM policies on the enrollment decisions of graduate students based on their race/ethnicity, gender, and resident/non-resident status.

1.1 Research Project Rationale

Generating new research on student price responsiveness at the graduate level using institutional-level data can inform EM practices and provide a framework for other administrators seeking to evaluate their own institutional financial aid programs. Many prior studies examined price responsiveness at the undergraduate level using national datasets (Dynarski, 2003; Hemelt & Marcotte, 2011; Savoca, 1991; St. John, 1990) or have focused on the effects of state-run grant aid programs (Avery & Hoxby, 2004; Bruce & Carruthers, 2014; Dynarski, 2000). Others have utilized institutional-level data to examine tuition elasticities (Bryan & Whipple, 1995), price responsiveness by race (Price & Sheftall, 2015), cross-price elasticities (DesJardins, 1999), and the effects of targeted financial aid programs on non-resident (Leeds & DesJardins, 2015) and the impact of a tuition guarantee for high-achieving, low-income students (Dynarski et al., 2021). What is lacking in the extant literature is a causal examination of student price responsiveness at the graduate level to different merit-based amounts of scholarship aid to determine whether financial aid influences enrollment decisions. In addition, prior literature suggested that scholarship naming conventions may influence enrollment (Avery & Hoxby, 2004) as a result of students inferring prestige or value from particular award names (DesJardins & McCall, 2010). However, there is no rigorous analysis on enrollment responsiveness at the graduate level based on changes in the naming conventions used to promote the awarding of grant aid to students. To examine these impacts, eligibility thresholds used for award amount determination are exploited for two separate groups of admitted students (high test score and high undergraduate grade point average), and pre- and post-policy change (i.e., scholarship name change) enrollment data are analyzed, respectively.

The paper is structured as follows: the next section contains a review of the literature, followed by the inclusion of the research questions, a discussion of the theoretical and statistical frameworks guiding the empirical work, and the research designs utilized to answer the questions and extend the prior research. The paper closes by including a discussion of the results, implications for policy and practice, followed by a conclusion.

Chapter 2 Literature Review

Prior research on the impact of changes in price on college enrollment focuses on programs at the undergraduate level. Most studies examined the responsiveness of students to changes in net price that primarily resulted from changes (i.e., increases or decreases) in tuition or the receipt of some form of financial aid. Price responsiveness was mainly quantified in one of two ways: either by calculating tuition elasticities or estimating a student price response coefficient. The following sections provide background information for each of these quantification metrics and a review of the extant literature on price responsiveness to financial aid changes or the effects of specific aid programs at the undergraduate level. While undergraduate research may not be a perfect parallel to graduate student enrollment decision-making, it serves as a foundation from which to explore such effects at the graduate level and provides a helpful reference point against which findings can be compared.

2.1 Economic Concepts

This section reviews the economic concepts often used in order to provide a conceptual framing for the research conducted. My intention is to highlight the specific applications of these concepts to higher education EM and follow up that discussion with a critical review of the literature.

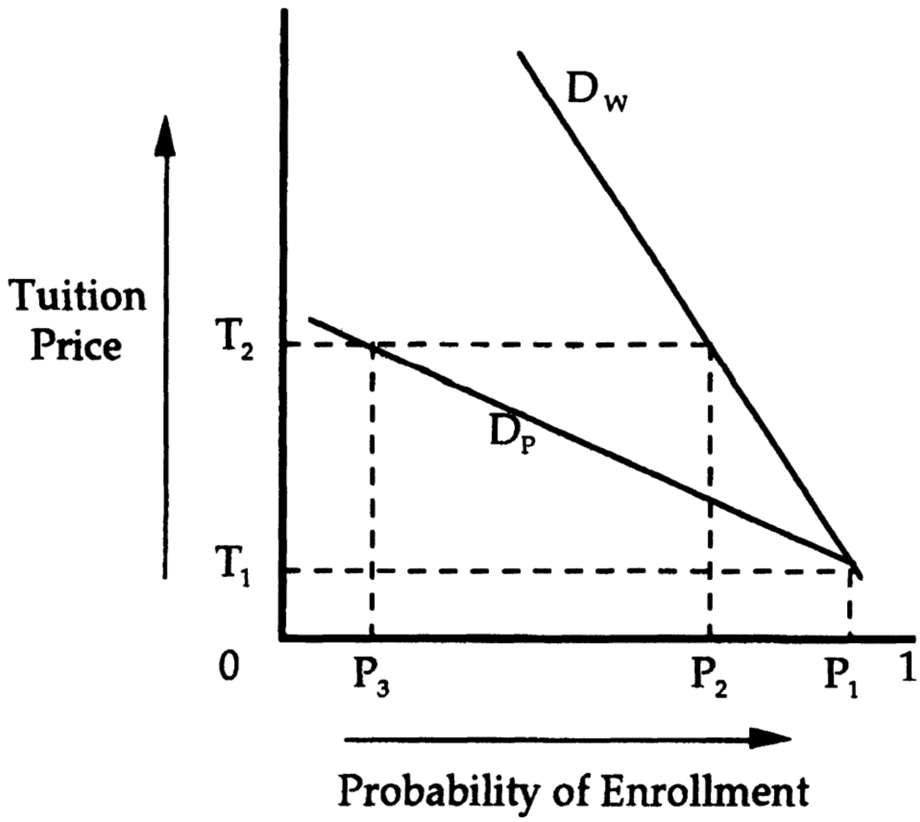
2.1.1 Demand Theory

A student's decision to enroll in college can be analyzed using concepts from economics, including (but not limited to) demand theory. Early work by Leslie and Brinkman (1987) noted that “the quantity of a particular good or service demanded is a function of price, the money income of the buyer, the prices of other goods and services, and the buyers’ tastes or preferences” (p. 181). As noted, price influences the quantity demanded because it is a component of the costs considered by an individual in the human capital framework (discussed in more detail in Section 2.4). In the higher education EM context, attention is given to the role of price (e.g., the tuition paid) and a student’s responsiveness to changes in it, and it is well established that other things equal, the quantity demanded of higher education (i.e., enrollment) by students is affected by changes in price (i.e., tuition). One reason for developing an understanding of price sensitivity as an influential factor in a student’s enrollment decision-making process is to better understand how it can be utilized as a policy lever for EM practitioners. Further, understanding student responsiveness to changes in price provides greater contextualization of empirical analyses of policy changes and enrollment shifts.

As an update to Leslie and Brinkman, Heller (1997) summarized price changes and their effect on enrollments as being inversely related to a family's discretionary income. For instance, students from low-income families have less discretionary income with which they can purchase goods and services, such as education, whereas students from high-income families have greater discretionary income to make such purchases. Therefore, an increase in real[4] tuition prices consumes a greater proportion of a low-income student’s discretionary income than for high-income students and results in lower overall enrollment rates.

[4] Reflective of the change in price after adjusting for inflation.

Figure 2.1 *Heller's (1997) Higher Education Demand of Poor and Wealthy Students*



Note: This figure shows Heller's (1997) illustration of the higher education demand curve for poor and wealthy students.
Source: Heller, D. (1997).

Demand curves are a tool that can help illustrate the responsiveness of students to changes in the price of higher education. Figure 2.1 shows Heller's (1997) example of a demand curve for higher education, tuition price (i.e., price) is situated on the y-axis, whereas probability of enrollment (i.e., quantity demanded) is represented on the x-axis. Given the assumption that education is a normal good, the demand curve is downward sloping, which suggests greater consumption (enrollment) as price (tuition) approaches zero. The steepness of a curve is indicative of price responsiveness. The flatter the demand curve (D_p), the more elastic (responsive) that individual is to changes in price of a good or service. The steeper the demand curve (D_w), the relatively more inelastic (less responsive) an individual is to changes in price. Said another way, the flatter a demand curve, the smaller the price change that is necessary to move an individual (or subgroup) from nearly full consumption (having an enrollment probability near 1.0) to near zero consumption (probability near zero of college enrollment) or vice versa. Importantly, tuition elasticities measure change in quantity demanded (of enrollment) for a specified change in price *along* the demand curve, *ceteris paribus* (Toutkoushian & Paulsen, 2016). A *shift* in the demand curve results from changes in the other factors affecting demand, such as a change in income.

As an update to Heller, Kim (2010) conducted a review of the literature of the influence of changes in price on enrollments. She concluded that increases in tuition, without commensurate increases in financial aid, will result in relatively large decreases in enrollments for low-income and underrepresented minority students, who are more responsive to changes in price than their upper-income and White counterparts. However, even commensurate increases in financial aid are unlikely to fully eliminate such enrollment decreases, because students in those subgroups are typically more sensitive to changes in tuition than financial aid (Kim, 2010; Heller, 1997).

Dynarski et al. (2022) provided a high-level review of the literature of the influence of changes in price on enrollments, persistence, and post-graduation outcomes with a focus on experimental and quasi-experimental studies. The scope of this paper focuses only on their enrollment takeaways, where they conclude, consistent with prior reviews, that students are more likely to enroll in college when the cost of enrollment is lower. Among low-income students, they concluded that evidence suggested that grants and scholarships, tuition subsidies, as well as loans and work-study can improve enrollment outcomes, but the effects were dependent upon institutional context and program design. However, similar to Kim (2010), they note that financial aid cannot be the sole equalizer for enrollment inequality between low-income students and their counterparts, as there are other non-pecuniary factors that may influence a student's enrollment likelihood, such as one's academic background and the community and familial supports that are available to them.

2.2 Elasticity Calculations

The price elasticity of demand is a calculation that measures the change in quantity demanded of a good as a result of a change in price. In the case of higher education, the tuition elasticity of enrollment serves as a measure of the enrollment responsiveness of students (quantity demanded of higher education) to changes in tuition (price). Leslie and Brinkman (1987) provide a high-level overview and definition of price elasticity of demand in equation 1:

$$\varepsilon = \%\Delta E / \%\Delta P \tag{1}$$

In this equation, ε represents the tuition elasticity, E represents enrollments, and P, the tuition price. In general, the law of demand states that, ceteris paribus, the quantity demanded of a good *decreases* as the price *increases*, and vice versa (DesJardins & Bell, 2006; Heller, 1997; Toutkoushian & Paulsen, 2016) and according to (1), the percentage change in enrollments is a function of the percentage change in price (tuition). The price elasticity is a measure by which responsiveness can be quantified, and, importantly, compared between different institutions, sectors, subgroups, etc. (DesJardins & Bell, 2006). Applying this basic understanding of the relationship between quantity demanded and price, tuition elasticities are likely to be negative across a wide range of scenarios and for most individuals.[5] As institutions increase tuition (i.e.,

[5] There are few exceptions to this negative relationship, notably the case of a Giffen (inferior) goods, where the quantity demanded for a good does not fall (or may increase) despite price increases (Bell & DesJardins, 2006; Toutkoushian & Paulsen, 2016).

sticker price), the quantity of enrollment demanded for said institutions would, cet. par., tend to decrease, and vice versa.

Understanding elasticities should be in the tool kit of EM administrators. DesJardins and Bell (2006) illustrate the practical use of them at an institutional level (see Appendix A for examples). Notably, given the consistent, one-way (increasing) trajectory of college sticker prices, the expectation is that the quantity demanded of enrollment will decrease, and tuition elasticities of enrollment will be negative. Exceptions to this assumption are rare but may reflect, in the higher education example, constrained supply (available seats) at a single institution (Toutkoushian & Paulsen, 2016) or a perception of increased institutional quality among prospective students (Millea and Orozco-Aleman, 2017).

In their work, DesJardins and Bell (2006) provided an example to illustrate the usefulness of elasticities for EM purposes. Different amounts were used to illustrate the effects of elasticity magnitudes on enrollment projections. Working with known tuition elasticities, one can estimate the effect on enrollment of a particular change in tuition (price). Likewise, if observing historical enrollment data at an institution, one can calculate tuition elasticities based on the percentage changes in enrollment over the percentage changes in price over a period of time. Utilizing known tuition elasticities is better suited for enrollment projections, but calculations based on historical elasticities is necessary to first establish an understanding of the baseline elasticities upon which the projections are based. If policymakers were unable to derive elasticities using historical enrollment data, or the higher education marketplace evolved beyond the relevance that historical data could provide, one could also utilize the literature to use approximations of elasticities or estimate econometric models to estimate such elasticities (DesJardins, 1999).

The magnitudes of tuition elasticities can also inform tuition revenue projections associated with price changes. For example, if an institution's tuition elasticity is greater than one (in absolute value), it suggests reductions in tuition revenues when tuition prices increase; elasticities less than one suggest more revenue when prices increase. Leslie and Brinkman (1987) also concluded that reducing tuition would have a larger effect on increasing enrollments than increasing tuition would have in decreasing the demand for seats. Heller (1997) notes that the calculation of both tuition and aid elasticities "can allow policy makers to predict with some degree of certainty what the impact of proposed tuition and aid changes will be on students from different income categories" (p. 640). This is precisely the reason why a thorough understanding of price responsiveness is necessary for institutional decision-makers and the motivation behind the analyses in this paper.

2.3 Student Price Response Coefficients

Calculation. Many empirical pieces that describe the effects of tuition or price changes on student enrollment did so without a standardized language for interpreting magnitudes to compare across studies. Leslie and Brinkman (1987) addressed this very issue by expanding upon the standardization technique called the student price-response coefficient (SPRC) of Jackson and Weathersby's (1975) landmark study. The SPRC was a way to create a standardized metric that allowed for comparisons between findings of previous empirical work. The SPRC conveys the percentage change in enrollment per $100 change in price, as noted in equation 2:

$$SPRC = \frac{\%\Delta enrollment}{\$100 \text{ price change}} \tag{2}$$

The signs of the coefficient signal whether enrollment increases or decreases when price (tuition) changes. As an example, if enrollment *increases* at an institution when price is changed, then the SPRC is positive. Conversely, if enrollment *decreases*, then the SPRC is negative.

Converting an SPRC to an elasticity requires knowledge of a few additional data points but can be done rather simply. To convert from an SPRC to an elasticity, one would simply convert the $100 price change into a percentage change of the base price used in an analysis (Leslie & Brinkman, 1987). As an example, for a study where tuition changed from $10,000 to $11,000, equation 3 illustrates how to obtain the percentage change in price (%Δ*price*):

$$\%\Delta price = \frac{\$11{,}000}{\$10{,}000} = 1.10 - 1.00 = 0.10 = 10\% \qquad (3)$$

A key component is to refer back to the original study to calculate elasticities using percentage changes in enrollment, rather than percentage point changes (Leslie & Brinkman, 1987). This creates a standard approach for conversion and eliminates errors that may occur when comparing results between studies due to the relative nature of percentage point change values. An example of a flawed conversion occurred in St. John's (1993) application of price response measures for enrollment projection purposes, in which he instructed "To estimate price elasticities, most experts multiply SPRCs by a factor of three, because about one-third of the eligible population participates" (p. 678). Because SPRCs measure change in quantity demanded of enrollment per discrete price change, one cannot derive an elasticity without calculating the percentage change of the new price with respect to a base price (Toutkoushian & Paulsen, 2016). Failing to first calculate percentage changes of the new price relative to the base price means that St. John's estimates were not accurately standardized prior to calculating elasticities.

Understanding how to make the conversion between different units in results enables researchers to assess the plausibility of the magnitudes of the effect sizes more quickly and easily in a given study. Knowing how to interpret tuition elasticities and a SPRC, as well as convert between them when needed, provides the tools necessary to compare results between studies that may be using different measurement units. Finally, having a context for the individual components of each equation enables quick digestion and comparison of results for policymakers, such as those in EM who may be doing their own institutional assessments.

Literature Review

2.4 Tuition Responsiveness

This section discusses what previous literature has found about the impact of scholarships on enrollment at the undergraduate level. Findings from prior studies are used to inform the structure of the analysis in this paper and situate the results within the broader context of findings on the impact of scholarships on enrollment. Comparing results from this paper to findings from prior literature also helps to establish the foundation against which transferability of undergraduate findings to a graduate school setting can be determined.

2.4.1 Student Price Response Coefficients

Consistent with the law of demand, Leslie and Brinkman's (1987) synthesis of empirical studies from the 1970s and early 80s finds that enrollment declines when prices go up and increases when prices go down. On average, they found the average SPRC for a $100 tuition increase (in 1982-83 dollars) to be about 0.70 percentage points. In other words, for each increase (decrease) in tuition of $100, enrollments will drop (increase) 0.70 percentage points.

St. John (1990) provided one of the first substantial extensions of Leslie and Brinkman's (1987) work about student price response coefficients. A key component of St. John's (1990) work was that, in addition to adding to the findings on tuition price responsiveness, it provided price response coefficients for various types of financial aid. Among all applicants analyzed in the High School and Beyond (HSB) sophomore cohort for the 1982-83 academic year, he found an SPRC of 0.28 percentage points for a $100 increase in 1982-83 dollars. This was notably lower than that found by Leslie and Brinkman (1987). However, St. John's (1990) finding could be a result of his use of data which utilized students' top choice institutions from the spring of their senior year to match against future enrollment records; some students may have already known where they were going to attend at that point, biasing their sensitivity to tuition prices downward.

St. John (1990) then split SPRCs by income group, finding greater price responsiveness to tuition increases in the bottom three (of four) income groups, with the top group having a relatively inelastic SPRC of 0.14 percentage points per $100 tuition increase. His findings provide support for differential price responsiveness by family income, with high-income students being relatively inelastic to changes in price. Kane's (1995) findings also lend support to this notion of differentiation in enrollment responsiveness by income. While he finds high public tuition to be associated with lower college entry rates overall, he finds gaps between high- and low-income enrollment to be greatest in high-tuition states and within states that implemented tuition increases.

Heller's (1997) follow-up synthesis to Leslie and Brinkman (1987) found an average SPRC of about 0.5 to 1.0 percentage points per $100 increase (in constant 1994 dollars) in tuition across all studies examined. This provided further support for the average estimate put forth by Leslie and Brinkman (1987) and suggested similar price responsiveness by students despite facing higher entry costs to begin enrollment in postsecondary education. The obvious detraction of utilizing SPRC for comparison among empirical studies, rather than elasticities, is that SPRC are often calculated in constant dollars.[6] Elasticities, on the other hand, are unitless, and the

[6] This indicates that the calculation was performed using dollars that are indexed to a base year for consistency within a given study (Heller, 1997).

magnitudes are able to be compared to each other if they are measuring percentage changes for the same items in the numerator and denominator (Toutkoushian and Paulsen, 2016).

Other authors have performed descriptive analyses of previous empirical work to ascertain price response. McPherson and Shapiro (1998) used data from the 1992-93 academic year in the National Postsecondary Student Aid Surveys (NPSAS) and gathered enrollment statistics from the National Center for Education Statistics (NCES) to describe recent enrollment trends (through 1994). McPherson and Shapiro (1998) provided important takeaways that illustrated the trends at that present time of their study and associated responsiveness of students to price. They found significant effects of financial aid on enrollment, specifically for students from lower-income families, which they define as having incomes below $20,000 (in 1990 dollars). The data for the analyses was time-series, allowing them to draw conclusions about lower-income student responsiveness to increases in net cost over time. Using a slightly different metric to present their findings than Leslie and Brinkman's (1987) student price-response coefficient (described in further detail below), they found that a $150 increase (in 1993-94 dollars) in net price decreased lower-income student enrollment by 1.6%. They describe the consistency of their finding to the 1.8% decline per $150 increase in net price found in the broader cross-sectional literature findings. Finally, similar to Leslie and Brinkman's (1987), McPherson and Shapiro (1998) found no evidence of changes in enrollment for affluent families as a result of increases in net cost.

Additional support was found for relatively low tuition responsiveness of students who are likely to attend selective colleges. Avery and Hoxby (2004) surveyed students who had very high college aptitude and were "likely to gain admission to and attract merit scholarships from selective colleges" (p. 245). The authors' study included high school seniors from 510 high schools in the United States during the 1999-00 academic year who were randomly selected by their high school counselors. Using data from the College Admissions Project, they estimated a conditional logit model and found a $1,000 increase in tuition *decreases* a student's probability of matriculating by 2%, *ceteris paribus*. In other words, the SPRC for a $100 tuition increase (in 1999-00 dollars) was 0.20,[7] which is lower than results found in previous studies, but consistent with prior findings that suggest students at selective institutions are relatively more tuition inelastic.

A more recent study found an SPRC slightly lower than those from research done on tuition responsiveness in preceding decades. Hemelt and Marcotte (2011) used IPEDS data to examine tuition increases at U.S. public universities from 1991-1992 to 2006-2007 and found that a $100 increase (in 2006 dollars) in tuition induces enrollment declines of 0.25%. Expressed as a tuition elasticity, they estimated it to be approximately -0.10. They note that the effects were larger for Research I institutions than Research II or comprehensive universities and public liberal arts colleges. Interestingly, they found students at non-Research I institutions to be relatively inelastic to price changes, but that they simultaneously increased their reliance on financial aid to subsidize increased costs. This finding suggests differences in price responsiveness by students by selectivity, though is counter to what is expected based on Heller's (1997) and Leslie and Brinkman's (1987) findings regarding the concentration of high-income students at highly-selective institutions and low-income students at less- or non-selective institutions.

[7] SPRCs are often reported in terms of absolute value since it is assumed that tuition increases.

2.4.2 Tuition Elasticities

Tuition elasticities can be used to create an optimal pricing model at the institutional level using current students. Bryan and Whipple (1995) estimated tuition elasticities for current students at Mount Vernon Nazarene College (MVNC) in Ohio using student surveys. The surveys were structured to capture students' first-choice switching options using three nearby competitor colleges. Students who were already enrolled at MVNC were presented with tuition values that increased in $500 increments from a value of $6,000, which was close to the existing tuition rate at the institution. Bryan and Whipple (1995) estimated tuition elasticities to range from -0.12, when tuition was presented as $6,000 at MVNC, to -0.30 when tuition was set to $8,000. The estimated tuition elasticity jumped from -0.12 to -0.21 when tuition was increased to $7,500 from the base value of $6,000. The authors concluded that the net revenue from increased tuition would be optimized at a level of $7,000. These tuition elasticity estimates found were lower than average tuition elasticities typically estimated for new entering students, but consistent with what would be expected for continuing students who are typically less responsive to changes in price (Heller, 1997; Leslie & Brinkman, 1987). Though the study used student surveys, rather than actual decisions made by students, and a confined alternative choice set for the hypothetical situations, it illustrates how tuition elasticities can be used to optimize tuition revenue.

Often EM decision-makers are concerned with influencing enrollment at their own institution, rather than enrollment at *any* institution. Carter and Curry (2011) attempted to add nuance to the tuition elasticity of demand studies with their examination of how changes in tuition levels influence enrollments at *specific* institutions in a student's choice set, rather than enrollment broadly. They hypothesized that an examination of university-specific demand instead of nationwide demand would generate differences in the respective tuition elasticities. They studied eleven colleges that made up the primary academic function of a major public university in the United States. They solicited respondents for a survey of students who were already enrolled and utilized discrete choice modeling to examine students' school choices when the school indicated as their top choice was compared to other schools and tuition amounts were allowed to varying among the choices. They found, on average, tuition elasticities of enrollment at focal schools to range from -5.38 to -2.17.

Several issues exist with the Carter and Curry (2011) study. First, they surveyed students who were already enrolled at the institution, a limitation which they acknowledge. Second, students are self-selecting into the pool of respondents, so it is possible that tuition elasticity estimates are biased upward (in absolute value). It is plausible that students are more likely to respond to the survey if they are second-guessing their decision to enroll, whereas students might not respond if they are content with their choice to attend the study institution. This might have the unintended consequence of garnering responses only from students who would be relatively more tuition elastic than peers who are content with the focal school. Finally, the study utilizes one survey that asks students retrospective questions.

2.4.3 Residency Status

Millea and Orozco-Aleman (2017) examined tuition elasticities of enrollment for a subset of states in the southeastern United States. They expanded on prior studies that utilized institutional-level data in two meaningful ways. First, their models for resident and non-resident enrollment included pricing of competitor four-year public institutions. Second, they developed an institution-specific weighting scheme that utilizes macroeconomic and demographic controls from a non-resident student's state of origin. Important for takeaways relevant to enrollment managers, they estimated tuition elasticities for first-time, full-time freshman, who the authors describe as being most sensitive to price changes at the time of school selection (Millea & Orozco-Aleman, 2017). The authors utilize IPEDS data from 2003 to 2010, which includes information on all public, four-year institutions in their selected states of Alabama, Louisiana, Mississippi, and Tennessee. To examine resident enrollment, they estimated an ordinary least squares (OLS) regression model with fixed effects by year and institution and found enrollment to be inelastic with a tuition elasticity of enrollment of -0.39. However, they found a one percent tuition increase at competitor institutions within the state to induce an enrollment increase of 0.73% at the institution of interest, ceteris paribus. In other words, the cross-price elasticity[8] for resident students was 0.73. Using their non-resident enrollment model, they found a tuition elasticity of enrollment of -0.10, though it was not significant for the overall sample.

Of importance to many EM decision-makers, especially those at public institutions where tuition subsidies for residents exist, is whether price responsiveness differs by residency status. Millea and Orozco-Aleman (2017) found large differences among states with respect to tuition elasticities of enrollment for non-resident students. Among the four states they examined, they found changes in tuition to have a significant influence on non-resident enrollment for Louisiana, Mississippi, and Tennessee, with estimated tuition elasticities of 1.59, 2.02, and -3.12, respectively. These differences suggest that, on average, a one percent increase in tuition in each state was associated with 1.59 and 2.02% *increases* in non-resident enrollment in Louisiana and Mississippi, respectively. Whereas, on average, a one percent tuition increase in Tennessee was associated with a 3.12% *decrease* in non-resident enrollment. These findings illustrate how responsiveness of students to changes in price can vary by state, and presumably, across

[8] A measure of the change in quantity demanded of enrollment at focal institution as a result of price changes at a competitor institution (Toutkoushian & Paulsen, 2016).

institutions within each state. This has revenue implications for EM offices. For instance, if these estimates are true, it suggests that institutions in Tennessee should exercise caution when contemplating tuition increases, as they will likely result in a reduction in tuition revenue. Whereas institutions in Louisiana and Mississippi appear to experience non-resident enrollment increases with tuition increases, which suggests that there are opportunities for increasing revenue. Though an odd finding of increased enrollment with increased tuition prices, Millea and Orozco-Aleman (2017) suggest that this could be the result of the perception of an increase in quality by prospective non-resident students. Notably, the period of their study (2003-2010) overlaps with launch of the Tennessee Lottery Scholarship Programs (Bruce & Carruthers, 2014), which could have mitigated the impact, at least in Tennessee, of possible non-resident tuition revenue losses by way of guaranteed and increased revenue from resident enrollees. Nevertheless, their findings for tuition elasticities for non-resident students highlight the importance for EM purposes and revenue implications of calculating own-price elasticities.

2.4.4 International Findings

In an international context, the magnitude of price responsiveness to tuition and fees is consistent with consensus estimates. Hubner (2012) exploited the introduction of tuition and fees in seven of the sixteen German states over the course of two years beginning in 2007 to examine the effect of changes in price on student enrollments. Prior to 2007, students paid no tuition and fees to attend universities in the sixteen states.[9] Hubner (2012) used a difference-in-differences approach and estimated that a €1,000 increase in tuition and fees decreased the probability of enrollment among first-year students by 2.7 percentage points. Adjusting for spillover effects,

[9] Enrollment at private institutions was very small in quantity, comprising only two percent of all enrollment in 2005.

Hubner estimated the true average treatment effect to be that a €1,000 increase in tuition and fees was associated with a 4.7 percentage point decrease in initial enrollment. If an assumption is made that students in Germany perceive €1,000 to be approximately equivalent to how students in the U.S. perceive $1,000, then treatment effects are in line with those found in prior literature about U.S. enrollment (Deming & Dynarski, 2009; Heller, 1997; Leslie & Brinkman, 1987; Toutkoushian & Paulsen, 2016). However, the estimated effects of tuition and fees in this study may be overstated due to the timing of the fee introduction, which occurred during the onset of a global recession. The introduction of tuition and fees may have had less of an impact if the economic conditions were different during these years. In addition, he noted that some states were quick to revert back to fee-free after new politicians took office. If the public was aware of political foreshadowing of the policy reversal, then the estimated effects may be biased downward, as students may not have been discouraged from what may have been perceived as a temporary fee.

2.4.5 Tuition Responsiveness by Race

Another important consideration for policymakers is price responsiveness differences among subgroups. Notably, Gallet's (2007) more recent meta-analysis supports Heller's (1997) conclusion of higher tuition elasticities (in absolute value) by non-White students compared to students in general. Different from the five major existing meta-analyses that synthesize findings of students' responsiveness (Dynarski et al., 2022; Heller, 1997; Jackson & Weathersby, 1975; Kim, 2010; Leslie & Brinkman, 1987), he utilized estimated price and income elasticities of higher education as dependent variables, with attributes of the associated studies serving as independent variables. Rather than arriving at a consensus estimate of price elasticities of demand for higher education, he identified the modeling procedures from the studies and their associated effects on estimated elasticities. Gallet (2007) used sixty studies of higher education to conduct his meta-analysis. The mean tuition elasticity of all of the studies used in his meta-analysis was -0.60, with a standard deviation of 1.00. As he notes, this suggests quite large variation in the existing literature, which is what warrants further review. He estimated a meta-regression model to incorporate the variety of studies from prior literature and found short-run estimates to be more inelastic than long-run estimates for both tuition and income elasticity estimates. Importantly, he found how quantity demanded and price are measured, as well as the estimation method utilized, to be important determinants of the resultant tuition elasticity estimates in the studies. His findings suggest exercising caution when interpreting any single elasticity estimate from the literature. Instead, calculating own-price elasticities of quantity demanded at a specific institution of interest will generate the most accurate information for enrollment managers to identify and remedy differences in price responsiveness to financial aid offers among targeted populations. Furthermore, using a range of own-price elasticity estimates would enable enrollment managers to better simulate implications of tuition pricing strategies on student enrollment and revenue projections.

Price and Sheftall (2015) examined tuition and loan elasticities of enrollment among first-year freshman students at Morehouse College, a selective, private all-male historically Black college. Different from other studies that look at simply whether or not a student enrolls, they examined enrollment *intensity*, as measured by credit hours for which a student is enrolled. They attempted to exploit variation in the hours for which a student enrolled as a result of changes in price. They utilized institutional administrative student admission and financial aid data for freshmen who were newly admitted to Morehouse College during the 2009-2010 academic year. Notably, their study looked only at students who had already been admitted, measuring only responsiveness at the intensive margin. They utilized a count data regression model to incorporate the probability of zero for the number of credit hours in which a student might choose to enroll. They suggested that this possibility exists in reality, so a count data regression served to be a more unbiased tool that a standard regression analysis. Controlling for student ability, student and household financial characteristics, Parent Loans for Undergraduate Students (PLUS), Sallie Mae Loan, College Work-Study, and Federal Work-Study, the authors estimated tuition elasticities that ranged from 0.15 to 0.30 across all specified models, indicating that the freshmen are price inelastic.

Price and Sheftall (2015) also estimated a loan elasticity of enrollment using the availability of the Federal Direct Parent Loans for Undergraduate Students (PLUS). They found the estimated PLUS loan elasticity to be 0.16, which suggested that the students at Morehouse were at least as sensitive to the *availability* of PLUS loans as they were to changes in tuition and fees. Given that their study examined only one year, their findings are not surprising. Their estimated elasticities are consistent with what Gallet (2007) found as being the likely result in short-run studies. This study likely would have benefitted from examining tuition elasticities of freshman cohorts over time, especially since the 2009-2010 was situated within the Great Recession. Additionally, Price and Sheftall (2015) exploited variation in enrolled credits, which could underestimate the true responsiveness of students to price for one of two reasons. First, the students were *already enrolled*, which typically indicates a more limited sensitivity to price, either because price considerations had already been made pre-enrollment or because the price of enrolling for additional credits is less than the opportunity cost of not doing so (Heller, 1997; Leslie & Brinkman, 1987). Second, the typical student in the sample came from a family with higher-than-average income compared to Black males writ large (Price and Sheftall, 2015), so the estimated tuition elasticity might underestimate price responsiveness of Black males who were from average or below-average income backgrounds.

Heller (1997) provides three explanations for why sensitivities to tuition and financial aid changes appear to vary by subgroups, specifically race. He notes that, if students of different races tend to be concentrated at the ends of income distributions, then elasticities for a given race might closely reflect those of the income group as a whole. This concept applies to any subgroup (not just race) that is concentrated in a particular income group. Second, students from different races might react differently based on perceived differences in ability level. He describes this in relation to responsiveness by noting that students from higher ability levels tend to have higher enrollment rates overall, even after controlling for income differences among them. This would suggest that low-income students of higher ability might be less responsive (relatively more inelastic) to price changes than similarly situated students with lower ability levels. The third explanation that Heller (1997) provides is that different racial groups might have different demand curves as a result of different preferences for higher education. This is consistent with Toutkoushian and Paulsen's (2016) and Leslie and Brinkman's (1987) description of differences in preferences that might emerge among particular subgroups along unobserved qualities (such as a desire to be close to home or in a smaller learning environment), irrespective of income or ability.

2.4.6 Institutional Diversity

Further empirical work examined the effects of tuition increases on diversity at public colleges and universities. While prior reviews of the literature have established greater tuition elasticities among racial and ethnic minorities (see Heller, 1997; Hemelt & Marcotte, 2011; Kim, 2010; Leslie & Brinkman, 1987), less is known about the influence of tuition increases on subsequent institutional diversity. Building on previous studies that highlighted differentiation in price responsiveness by race and ethnicity, Allen and Wolniak (2019) examined the effects of tuition increases on racial and ethnic diversity at public institutions over time and whether those relationships differed by institutional selectivity. The authors created a standardized measure of institutional diversity to measure changes in class composition as a result of tuition increases. They utilized IPEDS data from 1998-99 to 2011-12 and constructed their diversity index measure based on the *U.S. News and World Report* Diversity Index. Importantly, and consistent with Heller (1997), they found increases in college tuition to result in less diversity on campus. Specifically, they found that a $1,000 tuition increase at four-year, non-selective public institutions is associated with a 4.5% *decrease* in campus diversity among full-time freshman, *ceteris paribus*. They found that, on average, a one percent increase in in-state tuition and fees was associated with a 0.14% decrease in racial/ethnic diversity at non-selective, public four-year institutions among full-time undergraduates, implying a tuition elasticity of institutional racial/ethnic diversity of 0.14. The authors found greater (in absolute value) tuition elasticities of institutional racial/ethnic diversity among first-time freshman of 0.24, consistent with empirical evidence regarding differential price responsiveness by racial/ethnic minorities (Gallet, 2007; Heller, 1997; Kim, 2010; Leslie and Brinkman, 1987).

2.4.7 Cross-Price Elasticity

Changes in the tuition (price) at one four-year, in-state institution could affect enrollment (the demand for seats) at a comparable four-year, in-state institution. This concept is referred to as the cross-price elasticity of demand, which Toutkoushian and Paulsen (2016) define as representing "the percentage change in the demand for one postsecondary option due to a one percent change in the price of another option" (p. 181). This notion deserves mentioning because of its relationship to a student's perception of price and its influence on a student's enrollment decision-making. It also highlights the interconnectedness of a single institution to the larger higher education marketplace. As an example, this elasticity can measure the percentage change in enrollments at the University of Iowa as a result of tuition increases at the University of Illinois. Cross-price elasticities are relevant for institutions to determine who is moving between institutions of different sectors, selectivity, or choosing to not enroll (Heller, 1997; Leslie & Brinkman, 1987). Hemelt and Marcotte (2011) suggest this also likely occurred in their study. They found patterns of enrollment resulting from tuition increases to be suggestive of substitution occurring for top-tier[10] research institutions.

Students' cross-price elasticities of demand can be inferred without directly calculating them. DesJardins (1999) examined an issue for the state of Minnesota relating to a tuition reciprocity agreement held with Wisconsin. Reciprocity agreements, he notes, "are designed to increase student college choice by providing the residents of participating states with an opportunity to attend college outside of their state of residence at tuition prices less than typical non-resident rates" (p. 705). Briefly, the reciprocity program in the study provided students from Minnesota the ability to study at the University of Wisconsin – Madison for the price of tuition at the University of Minnesota – Twin Cities campus, and vice versa. The latter group (students from Wisconsin studying at University of Minnesota – Twin Cities) are the focus of his study. From DesJardins' (1999) estimated enrollment effects, inferences can be made about the cross-price elasticities of the focal student population. To recoup a differential that had developed in recent years from an influx of Wisconsin students studying in Minnesota at rates below what was being charged to Minnesotans, policymakers considered the addition of a 25% surcharge on the

[10] These are considered to be Research 1 institutions as well as those that are included in the top 120 public universities in *U.S. News & World Report* rankings.

amount of the tuition differential. The surcharge was designed to reduce the tuition discount received (relative to what was being charged to Minnesotans) by the Wisconsin students at the Twin Cities campus by $196. Naturally, policymakers were concerned with the effect that this price change might have on enrollments of reciprocity students at the Twin Cities campus.

The enrollment projection estimations by DesJardins (1999) highlight implicit cross-price elasticities of demand. Breaking students into three categories based on financial need, he projected a decline of about 8 students (from 1,019 to 1,011) from Wisconsin,[11] largely because of the relatively inelastic tuition elasticity of Wisconsin students attending the University of Minnesota - Twin Cities campus. The findings suggest a low (but positive) value for the cross-price elasticity of demand for those students when comparing the University of Minnesota to other institutions in the state of Wisconsin, where high values would have suggested high rates of substitution with institutions in Wisconsin and greater (relative) price elasticity of demand (Toutkoushian & Paulsen, 2016). It could also be that the students used for DesJardins' study would not have been admissible to the University of Wisconsin – Madison or were intent on moving away from home, so it was not a viable substitute for them, even at a slightly lower price. Wisconsin students also had relatively high financial resources, so were more inelastic to price changes, consistent with Leslie and Brinkman (1987) and Heller (1997).

Noorbakhsh and Culp (2002) provide an example of the pitfalls of engaging in tuition-setting behaviors without proper knowledge of price responsiveness. They examined the effects of the decision of the Pennsylvania State System of Higher Education to increase non-resident tuition between 1991 and 1993 by an average of 19.3% per year. Using OLS, the authors

[11] Using figures from DesJardins (1999), one could calculate the average price elasticity of demand based on enrollment projections and price changes using the 25% surcharge. Using the provided change in enrollment and associated change in price, the average price elasticity of demand for Wisconsin students is -0.11 (author's calculation). This reinforces the expectation of their relative inelasticity to changes in price.

estimated tuition elasticities of non-resident students of -1.15, whereas no significant effects of changes in tuition prices were detected for resident students. Likely as a result of inattention to tuition elasticities of non-resident students during the decision-making process that led to large tuition increases, enrollment of non-resident students plummeted by nearly 40% between 1991 and 1996. The Pennsylvania State System endured substantial losses of non-resident enrollment and the tuition revenue generated by them. This serves as an example of how and why tuition elasticities should be considered before major policy shifts, as it better prepares the institution or system for the implications of policy changes.

Despite the inferential example derived from DesJardins' (1999) study about tuition reciprocity, capturing *where* students shift enrollment to as a result of price increases is difficult given the large variation in institutional types within and across states. While related to student decision-making regarding enrollment, cross-price elasticity would be a topic covered more thoroughly by future researchers focused on discerning higher education substitutes for various student populations.[12]

2.5 Financial Aid Responsiveness

Understanding how students respond to different types of financial aid awards is integral to the EM process and a method for addressing negative side effects of tuition increases. Financial aid administration provides enrollment managers with an approach to help offset direct costs of attending college by reducing the net price of attendance for students (DesJardins & Bell, 2006; Hossler, 2000). This section examines the responsiveness of students to various types of financial aid, including grants (both need-based and merit), loans, and work-study. Overall,

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[12] See Kane (1995) for cross-price elasticity results between two- and four-year public institutions as a result of price changes.

findings from empirical works suggest that financial aid has a non-negative influence on enrollment for first-year students, ranging from no effect to large, positive effects depending on financial aid type. The magnitudes of responsiveness to different types of aid also varies among subgroups, which is a key takeaway for EM practices.

One important difference between tuition and financial aid elasticities is the direction of the signs. Whereas tuition elasticities are typically negative, suggesting lower enrollments with increases in tuition, financial aid elasticities are typically positive, suggesting higher enrollments with increases in financial aid awards (though this may not be the case for loans). Financial aid elasticities less than one indicate that students are inelastic, or not particularly responsive to an aid award offer, while elasticities greater than one indicate that students are elastic, or highly responsive to that particular type of aid offer. St. John (1993) refers to financial aid elasticities as cashflow elasticities of student aid, signaling the role of financial aid of providing additional means (income) to students for tuition payments.

A second important difference between changes in tuition and financial aid is how each is related to price. Tuition often represents the advertised price, or sticker price, prior to any administration of financial aid. As such, tuition elasticities can many times be thought of as sticker price responsiveness. Financial aid represents reductions to sticker price, or net price, after financial aid awards are considered. Thus, in some ways student responses to financial aid awards can be thought of as net price responsiveness. Further, the types of financial aid are functionally different from each other. Though all reduce the sticker price, at least in the short run, grants and scholarships are awards that do not need to be repaid, whereas loans are funds provided to students that eventually need to be repaid, and work-study provides no initial funding, rather just an opportunity to earn money via employment over the course of one's studies. These differences in financial aid type provide some context to understand how financial aid elasticities might differ by type and student subgroup.

2.5.1 Large-Scale Studies

Savoca (1991) examined how the composition of financial aid affected the enrollment decision-making of high school seniors in 1972. Using a 1 in 10 random sample from the National Longitudinal Survey of the High School Class of 1972 (NLS72). She estimated a multinomial logistic regression and found that students were more likely to enroll in college relative to labor force participation with the receipt of grants, work-study, or loans. However, she found students to be most responsive to grants, with work-study as a close, but lesser, substitute. While all aid options were estimated to increase the probability of enrollment, loans were approximately one-third the magnitude of both grants and work-study. Though this study does not estimate financial aid elasticities, it does suggest an order of responsiveness to financial aid. Grants were estimated to induce the greatest enrollment response followed by other types of aid, which is consistent with Leslie and Brinkman (1987) and Heller (1997). The large magnitude finding for work-study could be a function of the years analyzed with the NLS72 dataset, as tuition was lower and work-study awards would likely have covered a greater proportion of a student’s cost of attendance than it does now (Ma & Pender, 2022). As a result, students may have viewed it as only slightly less preferable to grants since it would have enabled attendance without incurring debt.

St. John (1990) gives further support to the positive association of financial aid with enrollment. Using the HSB sophomore cohort follow-up from the 1982-83 academic year, St. John employs a logistic regression model to estimate the influence of grants, loans, and work-study on enrollment decisions for high school graduates of 1982. Expressed as SPRCs in 1982-

83 dollars, he found that each $100 increase of grants, loans, and work-study increased the probability of enrollment by 0.43 percentage points, 0.38 percentage points, and 0.46 percentage points, respectively, *ceteris paribus*. Similar to Savoca's (1991) finding about work-study, the large estimated effect could reflect the proportion that work-study was able to cover of tuition in 1982-83 and its status as a debt-free option.

More striking than the average effects of financial aid on enrollment are the large subgroup differences found by St. John (1990). He split the students into low-, lower middle-, upper middle-, and upper-income groups. For low-income students, he found a $100 increase in grant aid increased the probability of enrollment by 0.88 percentage points, *ceteris paribus,* while loans and work-study were not significant. For lower middle-income students, he found a $100 increase in loans to increase the probability of enrollment by 0.53 percentage points, while a $100 increase in grants increased the probability of enrollment by only 0.39 percentage points. Similar to lower middle-income students, he found the largest response for upper middle-income students to be to a $100 increase in loans, which increased their enrollment probability by 0.63 percentage points, while an increase of the same dollar amount in grants increased the enrollment probability by 0.31, all *ceteris paribus*. Finally, he found financial aid to not have a significant influence on the probability of enrollment for upper-income students. These findings are broadly consistent with findings in the previous meta-analyses of student price responsiveness that suggest low-income students are relatively more elastic than their high-income peers, as well as middle-income students being the most responsive to loan options (Gallet, 2007; Heller, 1997; Kim, 2010; Leslie & Brinkman, 1987).

Grant-Based Aid. Dynarski (2000) estimated the causal effects of a state-run merit financial aid program on the rate of college enrollment. She utilized data from the Current

Population Survey to estimate the impact of the Georgia HOPE Scholarship on college-going rates in Georgia. Using a difference-in-differences approach, she estimated that the program likely increased college attendance among all 18- to 19-year-olds by 7.0 to 7.9 percentage points. However, the effects were concentrated among White students, who experienced an enrollment rate increase of 12.3 percentage points relative to Whites in nearby states that were used as the control group. She estimated no significant change in enrollment for Black students in Georgia. Broken down by income, she estimates that the attendance increased for upper-income students by 11.4 percentage points relative to peers in nearby states. Per $1,000 in merit aid awarded, she estimated the overall effect to be between 4 and 6 percentage points for middle- and upper-income families relative to peers in nearby states. Said another way, she estimated an SPRC of 0.40 to 0.60 per $100 increase (in 1998 dollars) in merit aid. Her empirical strategy was well specified and executed.

Dynarski (2003) examined the effects of the receipt of grant aid on college enrollment behavior by exploiting the elimination of the Social Security Student Benefit Program in 1982. Using a difference-in-differences approach, she found that an offer of $1,000 in grant aid increased the probability of college enrollment of high school graduates by 3.6 percentage points. If we assume that the money received via the death benefits serves as a true proxy for regular grant aid, then Dynarski's finding suggests a grant aid elasticity of enrollment of about 2.07.[13] Her difference-in-differences approach successfully estimated the causal effect of the grant aid that was eliminated from the program. The one drawback of her study, which she

[13] For Dynarski's (2003) finding, I first converted to percentage point change per $100, per standard SPRC conventions. This suggested that a $100 increase in aid would result in a 0.36 percentage point increase in enrollment. To calculate percentage changes for price and enrollment, I divided $100 by $1,900, the average tuition and fees used in her study, and calculated the percentage change of enrollment by dividing 3.6 by 33.0, which was the base enrollment rate used by Leslie and Brinkman (1987), which also applied to Dynarski's study.

acknowledges, is that it features an average award amount of $6,700 per recipient. Given that the average price of attending a public university was only $1,900, the incentives were quite large to do so. Despite the large award amounts, her finding was still broadly consistent with prior findings (Kane, 1995; Heller, 1997; Leslie & Brinkman, 1987).

Avery and Hoxby (2004) estimated even larger student enrollment responses to offers of financial aid. In their study of high aptitude students who were likely to enroll and receive merit aid offers at selective colleges, all other things equal, they found that a $1,000 increase in grants increases the probability of a student matriculating by 11%. This is notably larger than the estimates by Dynarski (2000; 2003), but not surprising given it indicates a student's likelihood of enrolling at one school in their choice set over another, rather than ever enrolling. Avery and Hoxby (2004) also estimated that a $1,000 increase in loans would increase the probability of enrollment by 7%, while a $1,000 increase in work-study would increase enrollment probability by 13%, *ceteris paribus*. In other words, the SPRCs for $100 increases (in 1999-00 dollars) in grants, loans, and work-study were 1.10, 0.70, and 1.30, respectively. These are large estimated effects of non-grant aid, but similar in magnitude to those found by St. John (1990) for lower- and upper-middle-income families (loan SPRCs of 0.53 and 0.63,[14] respectively).

In contrast to positive associations of grant aid with college enrollment, Bruce and Carruthers (2014) found only positive substitution effects. They examined the Tennessee HOPE Scholarship, which is a state-administered merit aid program for which students qualify based on ACT score *or* high school grade point average. Using ACT score as their running variable, they estimated an regression-discontinuity (RD) model and found that the merit scholarship did not actually increase rates of enrollment overall, but rather just increased enrollment at four-year

[14] St. John's (1990) SPRC calculations utilized 1982-83 dollars, so while they do not directly compare, they illustrate a responsiveness that is similar in nature to those found by Avery and Hoxby (2004).

institutions at the expense of two-year enrollment for students at the eligibility threshold.[15] Substitution effects were concentrated among FAFSA-filers who were low-income, Pell recipients, who they estimated were 2.9 percentage points less likely to enroll in two-year institutions, *ceteris paribus*. Consistent with their overall findings, Pell recipients were 2.4 percentage points more likely to enroll in four-year public institutions, *ceteris paribus*. They found no significant effects of substitution among non-Pell eligible students. Their estimates likely represent the lower bound, as they did not have access to high school GPA data, which limited their ability to identify eligible students with certainty. One drawback of their empirical strategy, which is often the case for studies examining local average treatment effects, is the inability to estimate effects for students away from the ACT eligibility threshold. For instance, it could be true that the scholarship induces low-income students near the top end of the distribution of ACT scores to enroll in college who otherwise would not have. However, those effects are often difficult to accurately estimate using an RD approach.

Hurwitz (2012) found that the awarding of institutional grant aid can vary greatly depending on institutions' consideration of home equity in financial aid applications. He exploited variation across institutions in the amount of parents' home equity[16] considered for institutional grant eligibility. Hurwitz (2012) used admissions and financial aid data from thirty highly selective private colleges and universities. Using home equity as his instrumental variable, he estimated a choice elasticity of 1.66, on average. In other words, he estimated that a $1,000 increase in institutional grant aid among the set of schools to which a student was admitted

[15] They estimated a decrease of 2.2 percentage points in the likelihood of enrolling in a two-year college and a 2.7 increase in the likelihood of enrollment at a four-year institution, with an estimated range of treatment effect on the treated of 4.2 to 9.6 percentage points.
[16] Home equity is defined as the value of one's home less the mortgage debt owed, if any (Hurwitz, 2012). It is required on the College Board's Financial Aid Profile application.

would increase the likelihood that they would enroll at the sample school by 1.66 percentage points. The magnitude of responsiveness was greatest among students from the lowest income bracket from families with incomes less than $50,000, who had a choice elasticity of 3.04. In contrast, among students from the highest family income bracket ($250,000 or more), the choice elasticity was only 0.54 and not significantly different from zero. The identification of home equity as a source of exogenous variation in institutional aid awarding was a unique and well-reasoned approach to instrument for causal effects of institutional grant aid. However, one should also be cautious when assuming that home equity measures are unbiased. For instance, if the figures are self-reported by the families, there may be underreporting of home values to deflate home equity amounts among families who might understand the implications of having large assets for financial aid eligibility. If anything, if it is assumed that this method of gaming financial aid eligibility criteria by underreporting assets occurs with families with the greatest resources (and thus greatest opportunity to underreport), then it would actually bias Hurwitz's (2012) choice elasticity downward. In other words, awarding $1,000 in grant aid to high-income students who are less price sensitive (Heller, 1997; Leslie & Brinkman; Kim, 2010) would underestimate the true influence of the award on enrollment at a specific college.

Castleman and Long (2016) examined the impact of the need-based, state-run Florida Student Assistance Grant (FSAG) on initial enrollment. Students were eligible for FSAG if they completed a FAFSA by March 1 of their senior year and had an expected family contribution (EFC)[17] at or below the program threshold. Utilizing a sharp RD, they found that eligibility for FSAG had a positive impact on enrollment at 4-year public institutions. They estimated that enrollment increased by 2.5 percentage points for every $1,000 (in 2000 dollars) in grant aid.

[17] An EFC is a metric calculated for each FAFSA-filer based on income and asset information submitted by the student and parent, if applicable.

The authors noted that some students who met the eligibility criteria did not receive the award because they failed to submit a FAFSA by the deadline during their senior year of high school, which could suggest an even larger impact of grant receipt on enrollment.

2.5.2 Single Institution Studies

Single institution studies can provide EM decision-makers with more helpful, detailed reference points by using individual institutions (and institutional characteristics) with which they may be more familiar. Moore, Studenmund, and Slobko (1991) analyzed a single institution, Occidental College, to examine changes in the demand for enrollment based on changes in price. Their study built on work done by Ehrenberg and Sherman (1984), including both financial aid applicants and non-aid applicants in the class entering in all 1989. Where Ehrenberg and Sherman (1984) found a net price elasticity of enrollment of -1.09 for admitted students at Cornell, Moore et al. (1991) estimated the net price elasticity at Occidental to be only -0.72. Among non-financial aid applicants, Moore et al. (1991) estimated net price elasticity of -0.35, which is in line with previous findings that suggests students from higher income families are less responsive to changes in price (Heller 1997; Kim, 2010; Leslie & Brinkman, 1987), though they did not have actual parental income for non-applicants. Moore et al. (1991) also found no effect of loans or work-study on enrollment decisions of admitted students. Among subgroups, they also found that estimated net price elasticities of enrollment were larger in absolute value for Whites and for those from higher parent income backgrounds, which could suggest the presence of viable, higher-quality alternatives for these students. For instance, Occidental College may have served as a backup option for these students who may have already preferred other, more prestigious institutions. While Moore et al. (1991) expanded well on Ehrenberg and Sherman (1984) with the inclusion of non-financial aid applicants, they were unable to parse out differences in elasticities among non-applicants. While some students may not have applied for aid because of high family resources, other students may have had low family resources and lacked the information necessary to apply. Finally, they also assumed that a reduction in tuition was equivalent to the offer of a scholarship for the same amount. If students respond to sticker price instead of net price, then reductions in tuition would elicit greater responsiveness among admitted students, and net price would an adequate way to measure price elasticity of demand.

Other empirical work attempted to address the joint nature of the financial aid application and enrollment processes through model specification. Curs (2008) estimated a random utility model[18] for applicants to the University of Oregon from 1996-97 to 2004-05, modeling the decision to apply for financial aid and enroll jointly to address concerns about the offers of financial aid being endogenous. He estimated the effect of need-based grants, merit aid, and loans on the decision of needy students to enroll at the University of Oregon. He found that a $1,000 increase in merit aid increases the likelihood of enrollment for in-state students by 6.8% and 2.5% for out-of-state students. For need-based aid, he found no effect on the likelihood of in-state enrollment and only 1.2% for out-of-state students. Finally, for loans, he found no effect on the likelihood of enrollment of in-state students and a *decrease* in the likelihood of enrollment of out-of-state students by less than 1%. His approach of modeling the decision as joint with application for financial aid is consistent with the suggested approach by Curs and Singell (2002). Notably, he chose to examine responsiveness to loans by grouping subsidized and unsubsidized loans into a single loan variable. Breaking down responsiveness by subsidized and unsubsidized status could uncover whether need-based subsidized loans induced enrollment

[18] Curs (2008) describes the random utility approach as one that models the joint process of sequential decisions (completing the FAFSA and/or enrolling at the University of Oregon) for a student such that an outcome is observed if the utility of the decision exceeds the utility of the next best opportunity.

among needy students. Hypothetically, the null and near zero finding on loans could represent an average between a positive effect of subsidized and negative effect of unsubsidized loans on enrollment. However, because Curs (2008) found need-based grants to have no effect and only a small effect on in-state and out-of-state students, respectively, it is unlikely that subsidized loans would have elicited a response greater than grants, especially among needy students.

Another study both estimated net price elasticities and simulated the effects of two different financial aid pricing strategies on initial enrollment and tuition revenue generation. Curs and Singell (2010) used data from the University of Oregon admissions office for a single-institution study of enrollment elasticities and simulation of pricing models. They focused on first-time, fall-term freshman applications from Fall 2000 through Fall 2004. The authors found estimated net price elasticities of enrollment of -0.87 for in-state students and -1.20 for out-of-state students. Notably, their elasticity estimates utilize net price, which makes assumptions about students' abilities to navigate and understand how financial aid applies to and lowers the sticker price of attendance. This approach should actually overestimate price responsiveness if students are not properly understanding financial aid. As such, their elasticity estimates should serve as the lower bound of the true responsiveness to changes in price. Alternatively, this could be measured using elasticities from a prior academic year and comparing them to actual changes in enrollment and tuition in a subsequent year.

Curs and Singell (2010) utilized estimated net price elasticities to simulate the effects of different pricing models on enrollment. Adopting pricing strategies that consisted of high-tuition, high-aid (HH) and low-tuition, low-aid (LL), they expanded on the application framework provided by DesJardins and Bell (2006) to illustrate how own-price elasticities can inform EM practices. Curs and Singell (2010) utilized estimated own-price net price elasticities of enrollment from nine subgroups of need and ability to demonstrate how, compared to actual 2005 enrollment data, an adoption of an HH model would likely result in an overall decline in revenue. This result is likely because the enrollment profile of the incoming class would shift in a way that induced more needy students to enroll, but not enough to offset the loss in tuition revenue from the anticipated enrollment declines of the somewhat- and non-needy students. They highlight the tradeoffs with the LL model, as well, projecting that total tuition revenue would increase by $1.22 million between resident and non-resident students, but would come at the expense of declines in enrollment among both needy and able students. Their simulation of tuition increases on future enrollment using estimated net price elasticities illustrates the utility of this as a tool for EM. By further breaking down net price elasticities by financial need, enrollment managers could utilize this tool to address institutional enrollment goals to work to close enrollment gaps by family income on individual campuses.

Grant-Based Aid. Causal findings estimated by Van der Klaauw (2002) showed large effects of financial aid on enrollment. He examined the effect of financial aid offers on enrollment for students admitted to a college on the East Coast, referred to as College X, from 1989 to 1993. To obtain causal estimates, he utilized an RD approach. This approach was made possible by the intervals of financial aid eligibility into which students were assigned based on a ranking system used by the college. For his study, he considers financial aid to be institutional grant aid awarded to each student. Because of his sample, this means that the institutional grant aid was not entirely need-based, as students could receive it without having completed an application for federal aid. Van der Klaauw (2002) broke his sample of admitted students into two groups, filers and non-filers, based on their completion of financial aid applications. He found that filers have an estimated financial aid elasticity of enrollment of 0.86, while the estimated financial aid elasticity for non-filers was just 0.13. In other words, for every one percent of financial aid offered to each group, on average, College X could expect enrollment among filers to increase by 0.86% and among non-filers to increase by 0.13%. One shortcoming of this study was that it utilized admitted students. While that may be a practical limitation of the data, students have still self-selected into being eligible for an aid offer from College X by virtue of having applied for admission. This has the potential to overstate the enrollment effects of financial aid offers made to the filers, as these students had already navigated time hurdles related to federal and college-specific financial aid applications. Non-filers, on the other hand, completed neither application. The financial aid elasticity estimate for non-filers might more closely resemble a merit-aid elasticity of enrollment for high-income, non-need-based students.

A different study in the Northeast found no overall effect of financial aid on enrollment decisions. Linsenmeier, Rosen, and Rouse (2006) utilized a difference-in-differences approach to analyze a major policy change implemented at a major, private university in the northeast. The policy change entailed a shift from meeting low-income students' full financial need through institutional grants, loans, and work-study until 1998, to meeting their full financial need with institutional grants beginning in 1998. In total, the policy change did not result in a change in the amount of financial aid, only the composition of the award package. The authors found that the program increased matriculation by 3 percentage points, but it was not significant. They did note that the program effect among low-income minority students was between 8 and 10 percentage points and statistically significant at the 10% level. While this is not a compelling finding, it does seem to suggest that the low-income minority students were comparatively more elastic that their White counterparts to changes in grant aid. As the authors mention, the amount of the award was fairly small compared to what was already being awarded to low-income students. On average, low-income students were receiving $25,734 in financial aid, of which, $20,000 was already grant aid. Replacing $4,000 of loan for grant in the financial aid package may have limited the responsiveness except for those who were most averse to loans and/or borrowing for their education. After all, students who were admitted to this selective major northeastern university presumably had other available education substitutes. It is plausible that the most important aspect of a financial aid package to this particular subset of students was seeing that they would be provided with enough financial aid to attend, regardless of composition. As such, a change in composition would have little effect.

Some enrollment managers might wonder whether merit-based financial aid will assist in their efforts to attract the most academically desirable applicants. Monks (2009) exploited a natural experiment at a private, most selective, mid-Atlantic liberal arts college to determine the effect of a merit aid offer on enrollment for randomly selected aid recipients. Unbeknownst to the admitted students, the institution randomly selected recipients for a merit aid award who were not receiving need-based aid or already receiving a merit aid award from the institution. The students who received the $7,000 award enrolled at significantly greater rates than those who were considered but did not receive the award (7.1% to 3.2%, respectively). However, there was no data available regarding family income for the treatment and comparison group, which could have plausibly influenced yield among non-recipients if the groups were not balanced by family income. In other words, there was no assurance the groups were equal in expectation (of yield) prior to treatment. If, by chance, one group contained individuals whose family incomes were concentrated just above the cut-point to qualify for need-based aid, and the other group contained individuals with family incomes substantially greater, then one might expect different yields at baseline, even without merit aid. Nonetheless, this study demonstrates that there may be differences in enrollment responses between similarly situated students when one group receives no scholarship money, and the other group receives *some* amount of scholarship money.

Leeds and DesJardins (2015) provided additional causal evidence that estimated the effect of merit aid on the probability of initial enrollment. They utilized admissions and financial aid records from the University of Iowa from 2004 to 2011 to test the effect of their National Scholars Award (NSA), which is a rule-based merit-aid award available only to non-resident student applicants who meet or exceed baseline eligibility criteria for the award. Because of how eligibility was established for the award, Leeds and DesJardins (2015) were able to utilize an RD approach to establish a causal estimate of the impact of the award. They found that students who received an NSA were significantly more likely to enroll than their counterparts immediately below the eligibility cutoff for the award. The authors estimated that receipt of NSA increased the probability of enrollment at Iowa among non-residents by 5 percentage points relative to ineligible non-resident applicants, *ceteris paribus*, which marked a fairly substantial increase from the baseline non-resident enrollment of 25 percent. Estimates of the effect of NSA receipt on White students was smaller than the average for all non-resident students, which Leeds and DesJardins (2015) note as implying greater responsiveness to NSA offers for non-resident minority students. Interestingly, they found students from Illinois to be less responsive to an NSA offer than other non-resident students. The authors suggest that it could have simply been a result of students from Illinois enrolling at greater rates at Iowa than students from other states at baseline. However, it could have also been that there were parallel financial aid programs available to students from Illinois, either at other public institutions or through the state, that had a greater influence on their enrollment decision-making than students from other states. In addition, another limitation was the lack of financial aid data pertaining to amounts of other scholarships and/or other federal financial aid. Knowledge of those details would have better enabled the authors to discern subgroup differences and estimate enrollment elasticities.

2.5.3 Cross-Price Subsidization

Institutions can also leverage the economic concept of cross-price subsidization to provide programs to students at a price less than the cost of production. Cross-price subsidization occurs when some students are charged more than others, and then the institution utilizes the revenue from the higher-paying students to subsidize the cost of providing services to lower-paying students. Technically, all students who attend in some capacity already receive some amount of subsidy, since the price is less that the cost of providing instruction (Winston, 1999). While the concept of all students receiving at least some subsidy may be true in principle, it is also worth considering the value of cross-price subsidization as a mechanism for individually pricing programs that serve to maximize the overall utility of the institution and further its strategic goal achievement. This concept could be practically borne out via differential pricing, which DesJardins and Bell (2006) describe as a mechanism that could leverage differences in elasticities between lower- and upper-level undergraduate students. This is largely the result of upper-level students being more established at an institution (thus making it more costly to find a substitute for their degree program) and lower-level students having more available options for transferring (substitutes) due to the relative lack of credit hour accumulation. As such, an institution can leverage this information to set differential tuition prices that maximize revenue based on differences in elasticities between lower- and upper-level students.

2.5.4 Scholarship Names

Very little research exists on the impact of scholarship naming on students' initial enrollment decisions. Revisiting Avery and Hoxby (2004), using survey data collected from very high aptitude high school seniors' financial aid offers, they found that calling a grant a "scholarship" increased the probability of a student choosing to enroll by 86 percent. Notably, this effect was concentrated among students whose parents did not have high incomes, suggesting that naming convention responsiveness, much like tuition and price responsiveness, is inversely correlated to parental income.

Findings from a related study on the impact of different types of financial aid on student departure lends support to Avery and Hoxby's work. DesJardins and McCall (2010) disaggregated the types of financial aid into loans, work-study, scholarships, and grants and examined the impact of each on reducing the probability a student's decision to depart, or stopout, from college. In other words, they assessed whether the persistence of students was affected by the type of financial aid they received. They found that all but grants had a significant impact on decreasing the likelihood of stopping out. Furthermore, not only did grants have no impact on persistence, but scholarships also had the largest impact of all aid sources. Taken together, this supports the notion that students respond differently when financial aid is specifically presented as a "scholarship" compared to a "grant."

2.6 Research Questions

Research questions derived from the discussion above include the influence of changes in institutional pricing and naming policies on students' enrollment decisions. To discern how differences in price responsiveness among student subgroups can apply to EM, it is best to examine the following questions at a single institution (Carter & Curry, 2011):

1. Does merit-based financial aid influence admitted graduate student enrollment decisions?
2. Do differences in the amount tuition is discounted via financial aid affect admitted graduate student enrollment probabilities?
3. Do changes in the naming conventions of financial aid awards affect admitted graduate student enrollment chances?
4. How do any such effects noted above differ by important individual characteristics, such as residency, race/ethnicity, and gender?

The research questions are examined using rigorous, non-experimental methods. Having access to institution-level data enables robust causal research to be performed, which includes examination of differences in price responsiveness by financial aid type, price discounting, and award naming, in addition to examining subgroup differences by residency status, racial/ethnic, and gender. Conducting this research is an important addition to the literature which can provide information for EM administrators about how to utilize data to improve financial aid awarding processes and maximize yields of targeted student populations. Finally, the proposed research designs, while developed with a single institution in mind, are crafted for broad transferability to any institution operating similar financial aid programs to encourage the proliferation of institutional-level information about graduate student price responsiveness.

2.7 Theoretical Framework

Human capital theory (HCT) provides an overarching framework to explain the influence of the benefits and costs of higher education on a student's decision to enroll in college. HCT suggests that the decision to acquire additional human capital (formal education) is based on evaluations of these direct and indirect benefits and costs (Becker, 1962; 1994; Mincer, 1958; Schultz, 1961). The direct cost consideration made by students pertains to the amount they have to pay in order to enroll in postsecondary education. To examine student price responsiveness, this paper focuses only on the influence of changes to one component of the direct cost, price, which includes tuition increases/decreases and financial aid provision. Indirect costs, such as foregone earnings due to postsecondary education (Becker, 1962) or the non-pecuniary time expenditure required for pursuing a college education (Toutkoushian & Paulsen, 2016), are not part of these analyses, since they are not traditionally manipulable via EM methods.

In addition to HCT, demand theory explains how changes in the price of education influences enrollment decisions. As the price of a normal good increases (decreases), consumption decreases (increases) (Toutkoushian & Paulsen, 2016). Therefore, increases (decreases) in the price of postsecondary education will, other things equal, lead to decreases (increases) in enrollment. Using demand theory to explain students' enrollment decisions, this paper examines how changes to the price component of direct cost (through financial aid administration) can affect enrollment decisions at the institutional level.

Finally, gift-exchange theory provides the framework to explain why students may respond differently to a named scholarship than a generically-named grant award. Borrowed from anthropology, this theory describes the tendency of individuals to adhere to norms of reciprocity when presented with what may be perceived as a gift (Mauss, 1924), which suggests that receipt of a gift is repaid in some way to the donor. In other words, the institution awards a student a scholarship, which conveys monetary and social value, so the student seeks to reciprocate by demonstrating their gratitude by choosing to enroll. In the case of a named scholarship, it represents not just a gift of money but an award that bestows upon the student the value associated with the name attached to it (Shurmer, 1971). A student who receives a named scholarship may feel motivated to reciprocate the institution's gift-giving by selecting it as the place where they choose to enroll, which would serve to maintain the relationship for future gift exchanges (Sherry, 1983), such as student monetary donations to the program and connections to an established alumni network.

2.8 Statistical Framework

Each statistical approach used in this dissertation is grounded in the counterfactual framework. To determine whether any of the treatments being evaluated had an effect on enrollment (the outcome of interest), one would ideally compare enrollment decisions between students who received the treatment to the same students in a counterfactual world where they did not receive the treatment (Flaster & DesJardins, 2014; Holland, 1986; Murnane & Willett, 2011). To accomplish this, a researcher would need to first assign the treatment to a group of students (e.g., award a scholarship) and measure their resulting enrollment decisions. Then, the researcher would need to travel back in time and measure the enrollment decisions of the same group of students without ever assigning the treatment. Since time travel is not possible, implementing a true counterfactual framework in my situation is not feasible. Rather, it is necessary to utilize rigorous research methods to approximate the ideals of the counterfactual framework.

Randomization of individuals into treatment and control groups is one way to create a plausible counterfactual (Flaster & DesJardins, 2014; Shadish, Cook, & Campbell, 2002). An experiment, or randomized controlled trial (RCT), is optimal because it will tend to yield an unbiased estimate of the average treatment effect. However, for this dissertation (and many other education research studies), an RCT is not a viable or ethical approach due to the evaluation of the policies (treatments) occurring after their implementation, as well as benefits being withheld from students in need, respectively. Instead, quasi-experimental research methods are employed.

Compared to RCTs, quasi-experimental methods are the most rigorous tools available to education researchers (Shadish, Cook, & Campbell, 2002) to assess the effects of treatment-like regimes. What Works Clearinghouse[19] (WWC) has established guidance and standards[20] against which causal studies, such as quasi-experimental approaches, are evaluated (What Works Clearinghouse [WWC], 2022). Quasi-experimental approaches evaluate outcomes between treatment and control groups by employing strategies that capture the intent of the counterfactual framework without requiring a time machine or employing an RCT. The two approaches employed in this paper include regression discontinuity (RD) and difference-in-differences (DID) designs. Each design utilizes a control group that closely mirrors the intent of the counterfactual framework, which allows for causal inferences to be made regarding any outcome differences (e.g., in enrollments) among the treatment and control conditions. Should all WWC (2022) standards for each approach be fully satisfied, an RD is eligible for the highest research rating (*Meets WWC standards without reservations*) whereas the DID is eligible for the second highest (*Meets WWC standards with reservations*). Each approach is discussed in more detail in the next chapter.

[19] The WWC is an initiative of the Institute for Education Sciences within the U.S. Department of Education meant to establish a standard for reviewing the rigor of causal studies and promote evidence-based decision-making.

[20] The most recent edition is the What Works Clearinghouse Procedures and Standards Handbook, Version 5.0, which can be accessed here: https://ies.ed.gov/ncee/wwc/Docs/referenceresources/WWC-HandbookVer5.0AppIES-508.pdf. Please see the manual for details related to the standards for RD and DID.

Chapter 3 Data and Methods

3.1 Data

The data for these analyses consists of eight cohorts (2014 to 2021) of institutional, student-level data from students admitted to a graduate school at highly selective public university in the Midwest. The dataset includes basic demographic information (e.g., self-reported race/ethnicity, gender, residency, and undergraduate institution), standardized test scores, and undergraduate grade point averages for 8,883 students. The data also includes all financial aid offers (i.e., merit- and need-based) made to admitted students and their subsequent enrollment decisions. It also contains indicator variables for first generation status and socioeconomic disadvantage, where the former is generated based on the student's application for admission and the latter based on a holistic review of their application materials. This chapter proceeds first with a discussion of the descriptive statistics, sample restrictions, and outcome variable of interest, followed by a discussion of the methods, assumptions, and background of the programs being evaluated.

3.1.1 Descriptive Statistics

Table 3.1 shows the descriptive characteristics of all admitted and enrolled students from 2014 to 2021. I further delineate admitted students by cohort year. From a high-level view, across all cohorts, rates of female and male admission and enrollment are approximately equal. The majority of admitted and enrolled students are White (60% and 66%, respectively), whereas non-White students enroll at rates lower than their admissions rate (5% vs. 8% for Black students, 7% vs. 9% for Hispanic students, and 11% vs. 13% for Asian students). Resident students also enroll at rates that are far higher than their admission rate (22% vs. 8%, respectively), which matters for how institutions think about targeting different financial aid programs (e.g., having select scholarships that are for non-resident students only).

For two separate analyses in this paper, the focus is around the score threshold at which students gain eligibility for merit-based scholarships. At this institution, eligibility for merit-based scholarships is traditionally established by having a high GPA and meeting or exceeding the minimum test score *or* by having a high test score and meeting or exceeding the minimum GPA. There are some instances where, at the discretion of administrators, exceptions are made to the awarding criteria where students below a score threshold may receive a scholarship and students above may not. At the minimum score threshold, scholarship award amounts are intended to change from zero to small (at the minimum GPA threshold) or medium (at the test score threshold) dollar amounts.

At the threshold, comparisons are able to be made between individuals who just meet and just miss eligibility for a program based on a particular score (Lee & Card, 2008; McCall & Bielby, 2012). Individuals above a threshold are treated (eligible), whereas individuals below not treated (ineligible). We assume that individuals who are just above and below the threshold are similar, except for very small differences in a score variable, which determines their treatment status. To conduct proper analyses, there must be a sufficient number of individuals above and below (i.e., statistical power) to produce a reliable estimate of the impact of receiving the treatment. As a result, three things related to scholarship eligibility are worth highlighting for the potential impact they may have on the analyses. The mean standardized test score across all cohorts is 168.7 for admitted students and 167.2 for enrolled students, with median scores for each just modestly higher. The scholarship eligibility threshold across all cohorts for students with high GPA scores is 167, which suggests that there will likely be many students situated about that threshold, which should provide enough power for a causal evaluation of scholarship eligibility at that threshold (McCall & Bielby, 2012). On the other hand, the mean undergraduate GPA across all cohorts is 3.73 for admitted students and 3.64 for enrollees, with medians for each slightly higher (3.80 and 3.77, respectively), which could portend power issues for analyses about scholarship eligibility around the GPA threshold (3.30) for high test-score students. Similar circumstances appear with need-based aid eligibility, where only 4 percent of all admitted students qualify.

Examining each analytic sample (see Table 3.2), across all cohorts, only 347 students received need-based aid offers. Need-based aid recipients have disproportionately fewer White students than those who did not receive need-based aid (41% vs. 61%, respectively), fewer Asian students (10% vs. 13%, respectively), and are overrepresented among both Hispanic (19% vs. 9%) and Black (18% vs. 7%) students. Need-based aid recipients also have much higher rates of socioeconomic disadvantage than their peers (which is not surprising, given that the qualification nature of the aid itself), are more likely to be resident students, first-generation, and have below-mean standardized test scores. Interestingly, despite lower test-scores, they have higher GPAs, on average, than their non-need-based counterparts (4.05 vs. 3.71, respectively). The demographics and characteristics for need-based aid recipients were stable and approximately similar across each cohort.

Table 3.1 *Descriptive Characteristics of Admitted Students, 2014-2021*

			2014-2021							
	All Admits	**All Enrollees**	**2014**	**2015**	**2016**	**2017**	**2018**	**2019**	**2020**	**2021**
Number	8,883	2,511	1,310	1,265	1,221	1,179	1,151	960	937	860
Student Characteristics										
Sex										
Female	51%	49%	50%	49%	50%	50%	53%	56%	53%	53%
Male	48%	50%	50%	51%	50%	49%	46%	43%	47%	46%
Did Not Indicate	1%	1%	*	*	*	*	1%	1%	1%	1%
Race/Ethnicity										
White	60%	66%	60%	63%	63%	63%	60%	62%	56%	48%
Black	8%	5%	7%	6%	7%	7%	9%	8%	9%	12%
Hispanic	9%	7%	6%	8%	9%	8%	9%	10%	12%	14%
Asian	13%	11%	15%	13%	13%	11%	13%	11%	14%	14%
Two or More	5%	6%	4%	4%	3%	4%	4%	6%	7%	10%
Not Indicated	5%	5%	8%	6%	5%	6%	5%	3%	2%	2%
In-State Resident	8%	22%	7%	6%	6%	7%	8%	10%	9%	13%
First Generation Student	5%	6%	-	-	-	-	11%	10%	12%	14%
Socioeconomic Disadvantage	11%	11%	-	12%	16%	13%	11%	14%	11%	15%
Qualified for Need-based Aid	4%	8%	3%	3%	4%	3%	4%	5%	4%	5%
Test Scores										
Median Highest Test Score	169	168	169	169	169	170	170	169	169	171
Mean Highest Test Score	168.7	167.2	168.5	168.3	168.3	168.7	169.0	168.6	168.9	169.4
Undergraduate GPA										
Median Highest Undergraduate GPA	3.80	3.77	3.77	3.79	3.80	3.80	3.80	3.81	3.81	3.85
Mean Highest Undergraduate GPA	3.73	3.64	3.69	3.70	3.70	3.68	3.83	3.73	3.73	3.76
Mean Enrollment Rate	28%	100%	24%	21%	25%	27%	31%	33%	33%	36%

***Source*:** Author's analyses of institutional graduate school datasets

Notes: Due to rounding, totals may not equal 100%. Please note, first generation status and socioeconomic disadvantage began to be recorded in 2018 and 2015, respectively. Socioeconomic disadvantage is indicated by administrators following a holistic assessment of their application materials. Racial categories totaling <1% of the student population (American Indian and Alaskan Native/Native Hawaiian or Pacific Islander) are represented with an asterisk in the table.

Table 3.2 *Descriptive Characteristics of Analytic Samples, 2014-2021*

	Total	Need-Based Aid Students	Non-need-based Aid Students	Total	Below GPA Threshold	At or Above GPA Threshold	Total	Below Test Score Threshold	At or Above Test Score Threshold
Number	8,883	347	8,536	2,973	97	2,876	3,812	971	2,841
Student Characteristics									
Sex									
Female	51%	53%	51%	47%	37%	48%	55%	61%	53%
Male	48%	47%	48%	52%	62%	52%	44%	39%	46%
Did Not Indicate	1%	1%	1%	1%	1%	1%	1%	*	1%
Race/Ethnicity									
White	60%	41%	61%	68%	45%	69%	67%	57%	71%
Asian	13%	10%	13%	15%	24%	15%	12%	8%	13%
Hispanic	9%	19%	9%	6%	14%	6%	8%	14%	6%
Black	8%	18%	7%	2%	6%	2%	4%	11%	2%
Two or More	5%	5%	5%	4%	6%	4%	4%	5%	4%
Not Indicated	5%	5%	5%	5%	4%	5%	5%	5%	5%
In-State Resident	8%	15%	8%	6%	10%	6%	9%	16%	6%
First Generation Student	5%	27%	4%	5%	10%	4%	5%	8%	4%
Socioeconomic Disadvantage	11%	59%	9%	8%	14%	8%	10%	17%	8%
Qualified for Need-based Aid	4%	100%	0%	1%	11%	1%	3%	8%	1%
Mean Highest Test Score	168.7	164.3	168.9	173.3	173.6	173.3	168.9	163.1	170.9
Mean Undergraduate GPA	3.73	4.05	3.71	3.73	3.15	3.75	3.95	4.08	3.91
Mean Enrollment Rate	28%	61%	27%	18%	32%	17%	19%	29%	16%
Mean Scholarship Amount	$25,901	$7,373	$26,654	$37,772	$11,913	$38,644	$30,019	$6,202	$38,159
Schol. as % of Tuition/Fees	43%	12%	44%	62%	20%	64%	49%	10%	63%

***Source*:** Author's analyses of institutional graduate school datasets

Notes: Due to rounding, totals may not equal 100%. Please note, first generation status and socioeconomic disadvantage began to be recorded in 2018 and 2015, respectively. Socioeconomic disadvantage is indicated by administrators following a holistic assessment of their application materials. Racial categories totaling <1% of the student population (American Indian and Alaskan Native/Native Hawaiian or Pacific Islander) are represented with an asterisk in the table. Scholarship percentage of tuition/fees is calculated using a weighted average of tuition/fees across all years in the study.

The analytic sample of students eligible to be treated at the GPA margin mostly reflects the makeup of the whole sample, except on a few dimensions. First, the analytic sample has a much lower proportion of Black students than the full sample (2% vs. 8%, respectively). Second, the group below the threshold is quite small (n = 97) across all cohorts. This may be related to necessary decisions related to sample restrictions, which are discussed in the next section, but it suggests that any analysis conducted at the scholarship eligibility threshold might lack power.

I also draw attention to a few key details of the analytic sample used for evaluating the impact of scholarship eligibility at the test score margin among high GPA students. The proportion of females is greater than males for the whole analytic sample (55% vs. 44%, respectively), but it is most pronounced among the students situated below the test score threshold, where 61% are female. Non-White students are also overrepresented below the test-score threshold. Also, we see that students below the threshold have substantially lower scholarship amounts, on average, than the group above the threshold ($6,202 vs. $38,159). This is perhaps the most important detail as the analysis is designed to determine the impact of receiving a scholarship offer on enrollment. Students below the threshold are not, with any regularity, receiving adjustments or exceptions to the eligibility criteria resulting in significant scholarship amounts, which suggests that the test score eligibility threshold is a strong predictor of scholarship receipt in the sample.

Tables 3.3 and 3.4 provide additional financial context for each cohort in the study. Table 3.3 shows the weighted mean tuition and fees[21] and cost of attendance[22] (COA) for each cohort in nominal dollars. In general, the institution's tuition and COA are comparable to its immediate

[21] Hereafter tuition and fees will be referred to as tuition for ease of reference.
[22] Cost of attendance includes tuition and fees and living expenses. Living expenses are intended to capture the estimated cost of living during the period of a student's enrollment and include components such as room (rent) and board (food), utilities, transportation, and personal/miscellaneous expenses. All figures are in nominal dollars.

peer schools against which it competes for students. The institution utilized different tuition rates for residents and non-residents, where non-resident students paid approximately $3,000 more than resident students during each academic year. Across the eight cohorts being examined in this study, the weighted mean tuition and COA are approximately $60,850 and $80,994, respectively. Tuition and COA steadily increased each year until 2021-2022, when tuition was frozen in response to the Covid-19 pandemic. However, as shown in Table 3.4, the percentage of tuition and COA that the mean scholarship covered among all admitted students and enrollees remained relatively stable from Fall 2015 forward.

Table 3.3 *Weighted Mean Tuition and Cost of Attendance: By Cohort*

Academic Year	Weighted Mean Tuition	Weighted Mean COA
2014-2015	$53,775	$71,805
2015-2016	$55,546	$74,096
2016-2017	$57,583	$76,593
2017-2018	$59,869	$79,379
2018-2019	$62,147	$82,493
2019-2020	$64,152	$85,222
2020-2021	$66,567	$88,467
2021-2022	$66,255	$88,751
Totals	$60,850	$80,994

Notes: Author's calculations using institutional figures for tuition/fees and cost of attendance (COA). Annual weighted means are calculated based on the number of resident/non-residents in each cohort. The total weighted mean is calculated based on the proportion each cohort comprises of the total sample. All amounts are in nominal dollars.

Average scholarship coverage of tuition and COA was sizeable among admitted and enrolled students. Table 3.4 shows that, among admitted students, the mean scholarship award covered just less than half of tuition in most years and approximately one-third of a student's COA. Among all students who enrolled, the scholarship coverage percentage declined, but still accounted for more than one-third of tuition expenses and more than one-quarter of their total COA. Similar to peer institutions, the majority of students at this institution often chose to finance their unmet COA (also known as their net price) with federal student loans.[23] A quick, back-of-the-envelope calculation suggests that an average student who receives the mean scholarship could still end up relying on upwards of $40,000 per year in federal loans for tuition expenses or $60,000 per year if borrowing loans for their entire remaining net price.

Table 3.4 *Mean Scholarship Coverage of Tuition and COA: By Cohort*

Academic Year	Mean Scholarship Admits	Percentage of Tuition	Percentage of COA	Mean Scholarship Enrollees	Percentage of Tuition	Percentage of COA
2014-2015	$18,961	35%	26%	$15,213	28%	21%
2015-2016	$22,424	40%	30%	$18,956	34%	26%
2016-2017	$25,769	45%	34%	$21,285	37%	28%
2017-2018	$27,785	46%	35%	$24,104	40%	30%
2018-2019	$28,343	46%	34%	$24,618	40%	30%
2019-2020	$27,993	44%	33%	$23,598	37%	28%
2020-2021	$29,441	44%	33%	$23,265	35%	26%
2021-2022	$29,731	45%	33%	$26,014	39%	29%

Notes: Author's calculations using institutional figures for tuition/fees and cost of attendance (COA). Percentages are calculated by dividing mean scholarship offers by weighted averages based on resident/non-residents within each cohort. All amounts are in nominal dollars.

3.1.2 Sample Restrictions

The main sample is restricted in a number of ways that are fundamental to the identification strategy employed. First, students who were admitted to the institution as “early decision” (ED) applicants (*n* = 217) were removed. ED students agree to submit an enrollment deposit and attest to the fact that, if admitted, they will withdraw from all other institutions upon admission to this school, and, more importantly, they pledge to enroll and must submit an enrollment deposit soon thereafter. This entire sequence occurs long before students become

[23] Anecdotal based on conversations with administrators within the department and cross-referenced with publicly-available accreditation data.

aware of any scholarship aid (merit- or need-based) that may be available to them later in the process, and as such, they are removed to limit bias in the estimates.

Also removed from the analyses are students who engaged in and received a "competing aid" scholarship (*n* = 1,234 students). This process is available to students who do not otherwise qualify for any merit scholarships. Upon learning of their ineligibility for merit scholarships, they are invited to submit scholarship offers that they may have received from peer institutions. In some cases, this graduate school opts to "compete" with other schools for these students, which results in a competing aid offer. These students were removed to reduce noise and downward bias potential in the first stage estimates, because students who elect to engage in the competing aid process may be different than their peers in unobservable ways, such as having such a strong desire to attend a particular institution that they are willing to take the extra effort to collect offers and submit them to a school which initially issued no scholarship. Despite the fact that these students comprise 14% of the sample (and enroll at a rate of 49% vs. 28% for the whole sample), the mechanisms by which they qualify for scholarships and enroll are sufficiently different than their peers, providing the rationale for excluding them from the sample in an effort to limit bias in the main analyses.[24]

3.1.3 Outcome Variable

The outcome variable of interest is enrollment at this single institution in the academic year immediately following their admission. Immediate enrollment (hereinafter referred to as "enrollment") is measured as a dichotomous variable where '1' indicates enrollment and '0' otherwise. Students who defer their admission to a subsequent academic year are assigned a '0'

[24] As a robustness check for the decision to exclude competing aid students, separate models are estimated for the main analyses that include students who engaged in the competing aid process. Results are included in Tables C5 and D2 in the appendices.

for the enrollment variable. On average, 28 percent (n = 2,511) of all admitted students enroll immediately, and 2 percent (n = 214) defer admissions and enroll during a future academic year.

3.2 Methods

Two different causal research designs were employed to evaluate the impact of institutional aid policies on initial enrollment decisions of admitted students. The first approach utilizes a difference-in-differences (DID) approach to exploit changes across years in institutional aid awarding related to the naming convention of a grant aid award. Changes to the name of a scholarship (e.g., changing from "Generic Grant" to "Named School Scholarship") between admissions cycles is exploited using DID to estimate enrollment responsiveness; the process by which the changes are made are discussed in the next paragraph. Additional analyses will examine whether enrollment responsiveness differs by student subgroups, including race/ethnicity, sex, residency status, and socioeconomic disadvantage.

The name of the need-based institutional scholarship was changed from "[Field of Study] Grant" to "[Name of School] Scholarship" by manipulating background inputs in the financial aid software system.[25] Through simple toggling and updates to a record for an existing scholarship in the system, administrators are capable of altering the name that appears on a student's financial aid award notice. The administrators at this institution changed the name of the existing need-based scholarship after the Fall 2020 cohort to take effect for all eligible students in the Fall 2021 cohort. In total, the name-change process required only some technical knowledge of the process and 10 minutes to execute the updates.

[25] The need-based grant name previously reflected the general field of study followed by "Grant," and was updated for the fall 2021 cohort to reflect the specific name of the institution followed by "Scholarship." Bracketed names are used to protect identity of the study institution.

Second, the impact of institutional scholarship programs on initial enrollment was estimated by exploiting differences in students' grade point averages and standardized test scores above and below eligibility thresholds using a regression-discontinuity (RD) approach. This approach was replicated to explore differences in responsiveness by residency status (resident or non-resident) or self-reported race/ethnicity and gender status at various eligibility thresholds employed for institutional aid awarding at the school.

The research designs employed contribute to the literature in two important ways. First, each of these analyses are, to my knowledge, the first uses of such methods to evaluate the impact of institutional financial aid programs in selective graduate school admissions. Second, this study builds on Porter et al.'s (2014) initial study of graduate enrollment responsiveness by providing the first quasi-experimental assessments of the external validity of prior undergraduate research findings to selective graduate school enrollment. Finally, this research generates helpful policy implications and practical insights that can be leveraged by enrollment managers and policymakers to induce enrollment among targeted student subgroups.

The following sections present examples of how each design is implemented using the institutional, student-level data from the graduate school. Whereas each analytic approach utilizes details that are specific to the institution in the data, such as the aid awarding parameters and associated software, the general approaches to evaluating eligibility-threshold financial aid programs at this school may be generalizable to other institutions. Next is a discussion of the methods used to analyze the research questions.

3.3 Difference-in-Differences Designs

When students in one admissions cycle are subject to a different policy than similar students from previous cohorts, one can determine whether the policy change had an effect on an outcome of interest if certain conditions are met. In practice, these types of policy changes present themselves with reasonable frequency in higher education. For example, new financial aid programs might be launched that target specific subsets of student populations (such as resident/non-resident or low-income/middle-income families), or even just simple institutional policy changes, like new program deadlines for need-based financial aid, tweaking of financial aid awarding parameters, or the rebranding of existing financial aid programs. Policy changes happen often in financial aid offices and being able to evaluate the impact of such changes by exploiting the timing of the implementation of such policies, helps to evaluate whether these changes achieved intended goals.

Changes in institutional financial aid policies across admission cycles present an opportunity to implement a difference-in-differences (DID) design (Ashenfelter & Card, 1985; Card & Krueger, 1994) to examine how shifts in policies (or eligibility criteria) across years affect student enrollment responsiveness. A DID design examines an outcome of interest and estimates a treatment effect by comparing two groups along the same time path where group is treated and one is untreated (i.e., a comparison group). Equation 4 formally presents the DID in a regression model:

$$y_{itc} = \alpha + \beta_1 POST_t + \beta_2 TREAT_i + \beta_3 (POST_t * TREAT_i) + \beta_4 X_{it} + \gamma_c + \varepsilon_{itc} \qquad (4)$$

where y is initial enrollment outcome for student i in year t; α is the baseline average rate of enrollment, $POST_t$ is a dummy indicating the years after the policy change; $TREAT_i$ is a dummy variable indicating that an individual was subject to the new aid awarding policy; β_3 indicates the effect of the treatment on enrollment in the year(s) after policy implementation; X_{it} is a set of covariates (e.g., individual characteristics of students, such as race, sex, and residency status) designed to improve precision of the estimate; γ_c denotes cohort-based fixed effects; and ε_{itc} is the error term which is assumed to be uncorrelated with the outcome.

To implement this approach, it is necessary to establish a proper comparison group that does not receive the treatment in order to estimate the baseline (non-treated) change (i.e., first difference) in enrollment between years (Card & Krueger, 1994). Then, assuming any potential exogenous shocks occurring simultaneously are not differentially experienced among students in the two groups, the policy change between admission cycles can be exploited to estimate the effect of a shift in the eligibility threshold for merit or need-based aid between admission cycles (i.e., the second difference) on treated students' enrollment behaviors. In simplest terms, subtracting the first difference of a comparison group from the second difference of the treatment group generates the causal estimate sought in Equation 4. This approach has been utilized to examine the effects of changes in minimum wage on employment (Card & Krueger, 1994), as well as the effects of the introduction of the state-run Georgia HOPE Scholarship Program on initial enrollment (Dynarski, 2000), and the substitution of public four-year colleges for private four-year colleges with the Adams Scholarship in Massachusetts (Goodman, 2008). The following sections first discuss assumptions of DID, as well as how this approach is utilized to examine effects of changes in the name of an institutional need-based scholarship program.

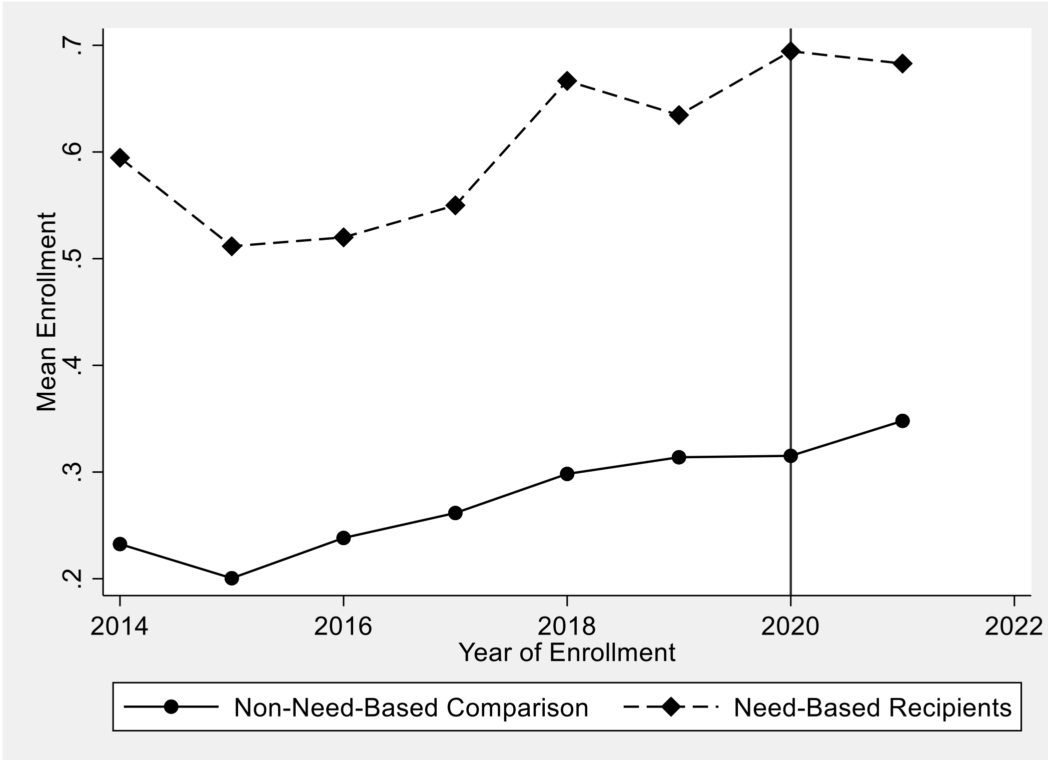
3.3.1 DID Assumptions

The parallel (or common) trend assumption is a crucial component of establishing a convincing argument of the validity of a DID (Angrist & Pischke, 2008). To evaluate the treatment effect of any policy change in the "post" implementation, a researcher (or practitioner) would first need to establish that the trend of the outcome (enrollment) for the treatment group is similar to that of control group prior to the treatment. Importantly, the trend does not need to be identical in mean outcome each year; it need only be parallel, or share a common trend, between the two comparison groups.

Figure 3.1 shows the enrollment trends of the treatment (need-based aid students) and control (non-need-based aid students) groups between fall 2014 and fall 2021. The treatment group (corresponding to the diamond line) represents students who were eligible for and received a need-based grant offer. The comparison group (circle line) represents students never eligible to receive a need-based grant offer. Along the x-axis, numbers range from ‘4’ for fall 2014 to ‘11’ for fall 2021. The scholarship name change occurred following the admissions cycle for fall 2020, so the fall 2021 cohort was the first to be exposed to the treatment.

Despite having mean enrollment rates that are substantially different between treatment and control groups during each admissions cycle, the overall trends of enrollment are approximately similar over the seven-year “pre” scholarship name change period. Mean enrollment rates for both treatment and control groups dip in 2015 before steadily increasing, on average, from 2016 through 2021. Visual inspection gives confidence that, barring a differentially experienced exogenous shock occurring simultaneously with the scholarship name change, causal inferences can be made from the results (Angrist & Pischke, 2008).

Figure 3.1 *Enrollment trends by need-based aid receipt, 2014-2021*



3.3.2 Scholarship Name Change

Software used for financial aid packaging presents an opportunity to manipulate the names of institutional aid awards to examine the effect of such changes on enrollment probabilities. Institutions regularly utilize digital student records systems to organize student business and transactions. Often embedded within each software package that an institution chooses is a financial aid awarding/packaging component that digitizes all aspects of the awarding and notification processes.[26] Using coding manipulation, scholarship names can be revised across cohorts. When there is a change in award names, such as from a generic to a proper named scholarship, this change can be exploited using DID to evaluate the impact of the

[26] Some institutions may choose to utilize third-party processors to operate the financial aid reviewing, awarding, packaging, and verification processes. Third-party options offer a standardized version of the financial aid awarding process that is not the subject of this section.

change on enrollments. The rationale behind this strategy, as detailed earlier in this paper, is that the naming of awards may initiate a gift-exchange relationship between the school and the student where the student may feel more valued or connected to an institution and thus choose to enroll at rates higher than those offered an identical amount of generic-named grant aid (Akerlof, 1982; DesJardins & McCall, 2010).

Named Scholarship Awards. Changes in an office policy to the naming convention of an institutional scholarship award enabled evaluation of the impact of award names on initial enrollment decisions, which informs EM practices related to aid awards. Eligibility criteria used for the need-based awarding process remained the same over the years studied so that students with practically identical eligibility[27] from fall 2014 to fall 2021 received financially equivalent financial aid packages with only semantic differences in the *names* of the institutional grants awarded. For example, from fall 2014 to fall 2020, when Student A received a need-based grant in any amount, it was generically titled "[Field of Study] Grant." In fall 2021, Student B, with (nearly) identical eligibility to Student A, received a grant, but the name was changed to, "[Proper Name of School] Scholarship." Thus, practically identical students received the same amounts and types of financial aid but simply different names on the awards.

Using the policy change scenario described above, a DID was estimated to evaluate its impact in a way that yields meaningful information to administrators to guide decision-making. Using this policy change as an example, EM administrators could utilize findings to optimize financial aid packaging practices to target awards (by name) to maximize enrollment yields, such as by changing scholarship or grant names for resident and non-resident students. While each

[27] Need-based aid is awarded based on a holistic review of a student's financial profile, both during their undergraduate years and in the current year of application. Based upon the criteria they fulfill, they receive need-based aid awards that correspond to one of six categories, ranging from approximately one-twelfth the value of tuition to one-half.

institution may have different capacities regarding the amount of grant aid that can be offered to students, policies instituting simple name changes can also be a politically palatable option to implement given its inexpensive nature (only time costs of existing staff[28]). Finally, it may also be that natural policy changes are simply more present and accessible for evaluation by administrators than threshold-based policies, which will be discussed in greater detail in the next section.

3.4 Regression-Discontinuity Designs

Two evaluations in this paper are performed using an RD design. Among causal inference research methods, RD is considered to be the premier non-experimental option (McCall & Bielby, 2012) due to its ability to closely resemble the random assignment nature present in a randomized controlled trial. RD enables the comparison of a treatment and counterfactual group above and below some version of a score-based eligibility threshold (Lesik, 2008; McCall & Bielby, 2012; Thistlethwaite & Campbell, 1960; Trochim, 1984). In an RD, assignment into the treatment or counterfactual group (or the probability of receiving the treatment in a fuzzy RD) is determined by an individual's score or value on a running (or forcing) variable (Lee & Card, 2008; Lee & Lemieux, 2010). In this paper, crossing the score threshold is not deterministic of treatment receipt. As such, a fuzzy RD (FRD) must be employed to estimate the impact when the probability of treatment at the threshold does not go from '0' to '1' (McCall & Bielby, 2012). Once plausible treatment and counterfactual groups are identified, the local average treatment effect (LATE) can be determined by comparing estimates of the

[28] The name-change idea is made with institutions such as the graduate school or others using similar software. In these instances, "in-house" staff members typically update the coding/programming that dictates the financial aid packaging rules, so adding an additional item type is common practice and would not require much, if any, additional time commitment.

outcome of interest for those just above and just below the threshold. This chapter discusses the two applications of a FRD design to evaluate the impact of receiving a scholarship offer on enrollment. The first utilizes undergraduate grade point average (GPA) as the running variable, where students who cross the GPA threshold substantially increase their probability of receiving a merit-based recruitment scholarship. The second RD design replicates the same concept but uses an admissions-based standardized test score as the running variable.

The following sections in this chapter provide a detailed description of the identification strategy used for each sample and its corresponding awarding processes, as well as descriptions of the formal equations used for estimating the impact of scholarship awards on enrollment.

3.5 Scholarship Awarding Background

Schools often utilize scholarships as a tool to recruit students and convince them to enroll after they have been admitted. The scholarship awarding at this graduate school is consistent in this approach and takes place entirely as a post-admissions process. Once students are admitted, they are automatically considered for merit scholarships based on the information included in their application and do not need to submit a separate application. During any given application cycle, admissions decisions are typically made on a rolling basis over a four-month period from November through February. Students are then given a few months to consider scholarship offers (if any) and are required to submit an enrollment deposit during the spring.

3.5.1 Merit Scholarships

There are two different ways in which admitted students can qualify for merit scholarships. First, students can receive a scholarship offer automatically based solely on the information included in their application. In the sample, the majority of students (~67%) qualify in this manner, meaning that they do not need to take any extra steps after they have been admitted in order to receive a scholarship offer. To qualify during any admissions cycle, a student must either have a test score at or above the target median test score and meet the minimum GPA threshold of 3.30 *or* have a GPA at or above the target median GPA and meet the minimum test score threshold of 167. If students do not qualify automatically based on application information, they have the option to submit scholarship offers that they have received from other institutions for review by the financial aid and admissions team. If upon review of the other scholarship letters it is apparent that a scholarship offer is needed to be competitive in the recruitment process, they will be made an offer. About 14% of all students qualify for a scholarship using this method. The latter process, which we will call the "competitive scholarship" process, requires additional engagement and effort by the student. However, if a competitive scholarship is offered, the terms of the award itself are no different than if the award had been awarded during the automatic process.

3.5.2 Need-Based Scholarships

An admitted student must navigate a few hurdles during the financial aid process prior to receiving a need-based grant offer. First, they are considered for merit scholarships using the information included in their application for admission. Second, if they receive a merit award and it exceeds an established threshold,[29] they are not eligible to apply for need-based aid. Next, if their total merit aid is below the threshold, they must complete an institutional need-based aid questionnaire. Upon completion of the questionnaire, depending on their responses, they are either denied outright and directed to information about supplemental federal and private loans,

[29] An institutionally-established threshold that ensures there is enough need-based institutional aid for all eligible applicants. The threshold is approximately half of tuition in any given year.

or they are invited to submit the College Scholarship Service (CSS) Profile Application to provide additional income and asset information. Finally, if the responses on the CSS Profile satisfy the eligibility criteria, the students are notified that they eligible for the institution's need-based aid. If the responses are unsatisfactory, then they are notified via email that they are not eligible for need-based aid and referred to the same supplemental federal and private loan information as the students who were denied outright. While appeals are allowed, they rarely result in a change of eligibility for need-based aid.

3.6 Identification Strategies

The RD designs in this paper utilize separate samples to exploit the minimum GPA and standardized test score thresholds at which students become eligible for a recruitment scholarship. The awarding process for recruitment scholarships involves a matrix which contains multiple thresholds to determine scholarship eligibility, and if eligible, the amount of an award that a student may receive. There are many thresholds within the matrix that, if a student crosses it, they may receive a marginally higher scholarship offer. However, the focus of this paper is on the minimum threshold to determine the impact of any scholarship amount relative to no scholarship amount. Future research could extend this paper by utilizing similar quasi-experimental methods to explore whether a marginal increase in a scholarship offer has an impact on enrollment among students who are already receiving some amount of scholarship, which would add to the work done by Porter et al. (2014).

Students may qualify for a recruitment scholarship in one of two ways. First, if they have a standardized test score at or above the target median score during a given admissions cycle (this will be referred to as the "high test score group"), they may gain eligibility by having an undergraduate GPA that exceeds the minimum GPA threshold. Alternatively, if students have an undergraduate GPA at or above the target median GPA during a given admissions cycle (this will be known as the “high GPA group”), they may gain eligibility by having a standardized test score than exceeds the minimum test score threshold. The following sections will discuss each group and the corresponding samples in greater detail.

3.6.1 High Test Score Group

High test score students may qualify for a scholarship offer at the GPA score threshold by having a high enough test score (i.e., at or above the target median in a given year) to be eligible to receive a scholarship offer (the treatment). The sample for this group includes students who could have possibly been treated using only GPA as the determining factor (*N = 3,157*). Undergraduate GPA is a continuous variable where all figures have been converted to a 4.0 scale[30] by a central application processor,[31] and it is exploited as the running variable to estimate the impact of crossing the minimum GPA threshold on the outcome (enrollment) among high test score applicants. This sample represents an intent to treat group (ITT) when using GPA as the running variable.

3.6.2 High GPA Group

An alternative way for students to gain eligibility for a scholarship is by having a high enough GPA to receive a scholarship offer based on their admissions test score. The high GPA sample is comprised of students who had an undergraduate GPA at or above the target median GPA during their respective admissions cycle. The target median GPA was normalized to zero

[30] A 4.0 represents an “A” average for applicants. The scale ranges up to 4.3 to accommodate institutions where students are graded on a 4.0 scale but are eligible to receive grades of “A+.” For the purposes of the analyses in this paper, grades above a 4.0 were left unchanged so as to not artificially bias any results.
[31] Students submit application materials, including transcripts and test scores, through a central application processor. Once application materials are standardized, they are then routed to each participating institution.

for each cohort, which represents the minimum GPA that an applicant can have to be exposed to the treatment at the test score threshold in a given year. This sample represents the ITT using test score as the running variable and is used to estimate the impact of receiving a scholarship offer on enrollment among high GPA applicants.

3.7 RD Design

Among both the high-test score and high GPA groups, crossing the score threshold does not result in a perfect (0 to 1) change in the probability of receiving a scholarship. Given the imperfect assignment, a fuzzy regression discontinuity design (FRD) is employed to estimate the impact of the scholarship receipt on enrollment for admitted students (DesJardins & Flaster, 2014; McCall & Bielby, 2012; Trochim, 1984).

A sharp RD assumes that crossing the score threshold is deterministic for receipt of the treatment (DesJardins & Flaster, 2014; Lee & Card, 2008; Lesik, 2008; McCall & Bielby, 2012). If this were true in this setting, all individuals above either threshold (GPA or test score) would receive the treatment and no individuals below the cutoffs would (i.e., perfect compliance), and assignment to treatment groups would reflect the following:

$$D_i = \begin{cases} 0 & if \quad X_i < c \\ 1 & if \quad X_i \geq c \end{cases}$$

where D_i is a dummy variable indicating a ‘1’ for scholarship receipt if a student’s value (GPA or test score) for either running variable, X_i, exceeds the threshold and ‘0’ if it did not. However, perfect assignment to treatment and counterfactual groups is not present for either analysis for the reasons discussed above about the scholarship awarding process. Thus, a fuzzy regression discontinuity design is employed to determine the impact of the treatment (scholarship) on the outcome of interest (immediate enrollment at this particular institution).

To implement the FRD, one must obtain estimates from two separate equations, similar to a two-stage least squares (2SLS) or instrumental variables (IV) method (Jacob et al., 2012). The first stage estimates the probability of receiving the treatment at the threshold using a local linear regression. The description of the formal first-stage is:

$$TREAT_i = \alpha_i + \gamma_0 D_i + f_1(X_i) + \varepsilon_i \tag{5}$$

where $TREAT_i$ is '1' if a high test score (high GPA) student receives the treatment (i.e., receives a scholarship) and '0' if not, D_i is a dummy where '1' indicates that they were assigned to the treatment based on the GPA (test score) threshold and '0' if not, $f_1(X_i)$ represents the relationship between the running variable, undergraduate GPA (test score) score re-centered at '0' and the treatment for student i, and ε_i represents the error term in the first stage, which is assumed to be random and identically distributed.

The second stage of the model utilizes the predicted value generated from the first stage (obtained via the undergraduate GPA or test score instrument, depending on the sample) to estimate the impact of the treatment on the primary outcome of interest. The second-stage equation is formally defined as:

$$Y_i = \alpha + \beta_0 TREAT_i + f_2(X_i) + \mu_i \tag{6}$$

where Y_i is the outcome of interest (immediate enrollment at the school) for student i and is '1' if they enroll immediately during the fall semester following the immediately preceding application period and '0' otherwise, $TREAT_i$ is the first stage estimate of the probability of receiving the treatment at the test score (GPA) threshold for a high GPA (high-test score) student, $f_2(X_i)$ represents the relationship between the running variable (GPA or standardized test score centered at '0') and the outcome for student i, and μ_i represents the error term for the second stage regression and is assumed to be random and identically distributed.

The second stage outcome represents the estimate of the impact of the scholarship on enrollment, which serves as the local average treatment effect (LATE). Using a two-stage IV setup in a statistical software package, the LATE is calculated automatically by dividing the second stage by the jump in probability of treatment from the first stage (Hahn, Todd, & van der Klaauw, 2001; Jacob et al., 2012; Lee & Lemieux, 2010; McCall & Bielby, 2012). To rely on estimates obtained from an RD, including the LATE estimate, one must satisfy the assumptions of an RD design, which are discussed in the following section.

3.8 RD Design Assumptions

For RD estimates to be a compelling and considered as mimicking the results of an experiment, the design must satisfy three primary assumptions. The assumptions include 1) local randomization of students in the narrow window around the score threshold, 2) the inability of participants to precisely manipulate the score used as the running variable, and 3) that there is no simultaneous treatment that utilizes the same threshold (DesJardins & Flaster, 2014; Lee & Lemieux, 2010; McCall & Bielby, 2012). This section discusses each assumption in greater detail and provides evidence that each RD model used in this paper satisfies both assumptions.

Local randomization is where students in the sample are distributed around the score threshold in a manner that is considered as good as random (DesJardins & Flaster, 2014; Lee & Lemieux, 2010; McCall & Bielby, 2012). Practically, this means that the areas near the score threshold must have a random distribution of the baseline characteristics of students within the sample. If the distribution of students is approximately random, then we can assume that students on each side of the threshold are approximately equal to each other (on average), except for any small difference in the running score variable at the threshold (DesJardins & Flaster, 2014; McCall & Bielby, 2012).

The local randomization assumption can be examined in multiple ways. First, one can plot the baseline characteristics/variables to visually inspect whether there is a smooth distribution (i.e., no discontinuity) through the threshold (DesJardins & Flaster, 2014; Lee & Lemieux, 2010; McCall & Bielby, 2012). One can also empirically test the difference between the means of baseline variables on either side of the threshold (McCall and Bielby, 2012). To do this, one would first have to determine an appropriate window (or bandwidth) to examine above and below the threshold. Then, one would perform *t*-tests to determine whether there is a significant difference between the two means (proportions).

As an alternative to *t*-tests, one can utilize one of two different regression approaches. First, one can regress a dummy variable for being above the threshold (i.e., eligible for the treatment) on a set of variables for baseline characteristics followed by an *F*-test of joint significance (McCall & Bielby, 2012). Finally, one can estimate a regression model where the outcome variable is regressed on a set of baseline variables. From that model, predicted values will need be calculated and plotted against the running variable to determine whether there is a jump in any predicted value at the score threshold (DesJardins & Flaster, 2014; McCall & Bielby, 2012).

3.8.1 RD balance checks

The local randomization assumption is assessed using visual plot of baseline characteristics and *t*-tests to determine whether differences between groups above and below the threshold are significant. Tables 3.5 and 3.6 show the *t*-tests results for significant differences in the means of the pretreatment characteristics above and below the GPA and test score thresholds, respectively. The results in Table 3.5 indicate no significant differences between pretreatment characteristics within 0.25 points of the GPA threshold, which is the optimal bandwidth based on bias and variance tradeoffs (Imbens & Kalyanaraman, 2012).

Table 3.5 *Different in Means Around GPA Threshold, 0.25 Point Bandwidth*

	Mean Below	Mean Above	Difference in Means	Standard Errors
Female	0.403	0.405	-0.001	(0.049)
Male	0.589	0.591	-0.003	(0.049)
White	0.540	0.625	-0.084	(0.049)
Asian	0.194	0.185	0.009	(0.039)
Hispanic	0.105	0.072	0.033	(0.027)
Black	0.040	0.018	0.023	(0.015)
Two or More Races	0.065	0.037	0.028	(0.020)
Hawaiian	0.008	0.002	0.006	(0.006)
No Race Indicated	0.048	0.058	-0.010	(0.023)
Resident	0.056	0.076	-0.019	(0.026)
Non-Resident	0.944	0.924	0.019	(0.026)
Total Observations	638	638	638	

Note: Standard errors are in parentheses. T-tests were performed to evaluate the differences in the pre-treatment characteristics of groups within 0.25 points above and below the GPA threshold.
* $p<0.05$ ** $p<0.01$ *** $p<0.001$

The results in Table 3.6 report balance checks examining the difference in pretreatment characteristics between groups at or within two test score points of the threshold. There is only one instance where the null hypothesis of no difference in proportions cannot be rejected. The proportion of Hispanic students above the threshold is 3.6 percentage points ($p < 0.05$) higher than the proportion below the threshold. Whereas such a pattern of imbalance could threaten the validity of an RD, this finding is likely the result of chance given the number of differences being tested (a Type I hypothesis-testing error) and not a true threat (Agresti & Finlay, 2009). To confirm, another *t*-test was performed separately for Hispanic students at or within 4 test score points of the threshold (approximately the optimal bandwidth used in the analysis). I was unable to reject the null in this instance, which further supports the Type I error likelihood, providing some confidence in the extent of randomness around the cutpoint.

Table 3.6 *Different in Means Around Test Score Threshold, Two Point Bandwidth*

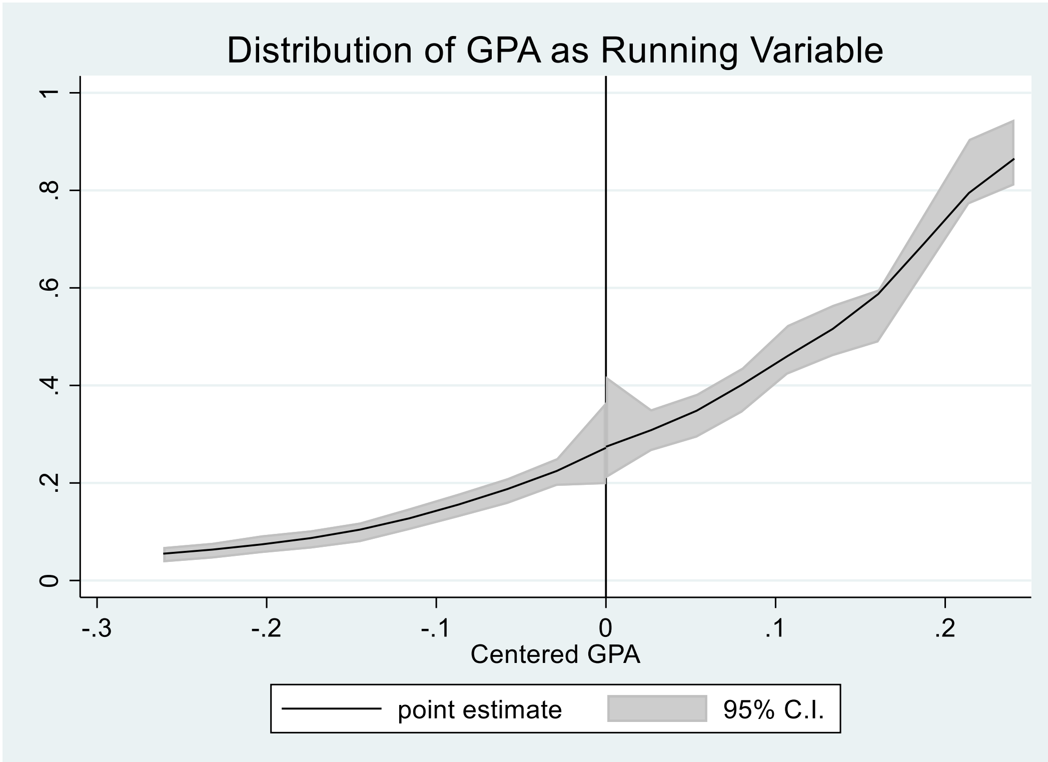
	Mean Below	Mean Above	Difference in Means	Standard Errors
Female	0.578	0.537	0.041	(0.026)
Male	0.418	0.456	-0.038	(0.026)
White	0.648	0.679	-0.031	(0.024)
Asian	0.114	0.135	-0.021	(0.017)
Hispanic	0.109	0.072	0.036*	(0.014)
Black	0.035	0.023	0.012	(0.008)
Two or More Races	0.050	0.037	0.013	(0.010)
Hawaiian	0.000	0.003	-0.003	(0.002)
No Race Indicated	0.042	0.050	-0.008	(0.011)
Resident	0.088	0.078	0.010	(0.014)
Non-Resident	0.912	0.922	-0.010	(0.014)
Total Observations	1719	1719	1719	

Note: Standard errors are in parentheses. T-tests were performed to evaluate the differences in the pre-treatment characteristics of groups within two test score points above and below the threshold.
* p<0.05 ** p<0.01 ***p<0.001

A check of the second assumption involves both visual inspection and empirical testing. The second assumption assumes that for each running variable and corresponding model, individuals cannot precisely control what score or value they receive (Lee & Lemieux, 2010; McCall & Bielby, 2012), preventing them from manipulating their assignment to the treatment or control group. McCrary (2008) introduced an empirical test for manipulation of the running variable which tests for discontinuities at the threshold, which would be evidence of score-bunching on one side of a threshold (i.e., manipulation).

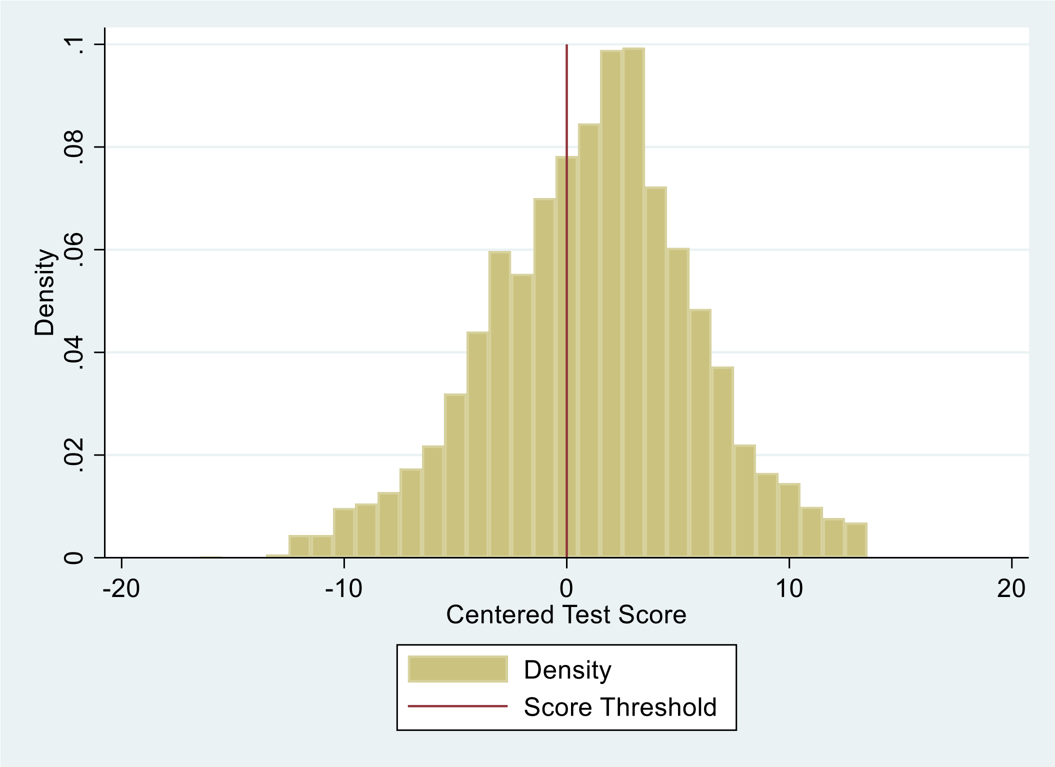
For the GPA running variable, the null hypothesis of no manipulation was not rejected. Figure 3.3 shows the density of the GPA running variable with 95% confidence intervals, which provides no evidence of manipulation. Taken together, the visual and empirical test support the use of GPA as a running variable in the model.

Figure 3.2 *Distribution of the GPA Running Variable*



Empirical tests for manipulation of the test score running variable were, however, unsuccessful, likely due to its highly discrete nature, which necessitated an alternative approach (McCall & Bielby, 2012). Visually examining the distribution of the test score running variable in Figure 3.4 shows no evidence of unnatural bunching around the threshold. Based on the annual median test scores in the sample, one might expect a frequency distribution with greater density above the threshold, as is reflected in 3.4. Using these evaluations, the null hypothesis of no manipulation cannot be rejected, which supports its use as running variables in the test score RD model.

Figure 3.3 *Distribution of the Test Score Running Variable*



There are some measures that researchers can take to guard against manipulation, should it occur. Using a first test score when it is otherwise difficult for a student to obtain a specific, targeted value (see Bruce & Carruthers, 2014) can avoid manipulation that may result from taking the same test multiple times (DesJardins & Flaster, 2014; McCrary, 2008). Using thresholds that are not otherwise well-known or publicized to students is also a valuable way to minimize the possibility of manipulation. In addition to there being no evidence of manipulation for either threshold used in this paper, undergraduate GPA and standardized test scores are unlikely to be able to be precisely manipulated in a way that would affect systematically treatment assignment. Furthermore, neither threshold is known to the public in advance of the application period (when the scores might still have potential to increase or decrease).[32]

[32] The admissions process is done on a rolling basis. Students may be admitted months after the first round of admissions acceptances are issued. In any event, no score thresholds for scholarship purposes are made public.

The third assumption of an RD is that there are no other interventions occurring simultaneously to the treatment that utilize the same threshold (Lesik, 2008; McCall & Bielby, 2012; Shadish et al., 2002). Each analytic sample exploits a threshold at which no other treatment occurs at the study institution. However, it is possible that peer schools to the study institution, by chance, utilize the same thresholds to determine eligibility for their merit-based scholarships, but information on other institutions' scholarship awarding parameters is not publicly available. If other institutions utilized the same thresholds to determine scholarship eligibility, it would not pose a threat to the findings of the studies in this paper. Rather, use of the same score threshold elsewhere would strengthen the takeaways from the enrollment effects at this institution, as it would potentially bias the estimates toward a null effect, since scholarship offers at peer schools would likely mitigate (rather than enhance) the impact of scholarship offers for decision-making purposes.

No evidence of manipulation with the running variables used in the RD analyses give confidence that students above and below the thresholds can be compared without obvious bias in the estimates. Balance checks confirm that, along with no manipulation, the models used for the analyses can be trusted to produce reliable estimates of the impact of scholarships on enrollment.

Chapter 4 Results

Results from all three analyses provide takeaways for researchers, policymakers, and EM administrators. The results in this paper provide the first causal analysis of the impact of the naming convention of grant aid, as well as the first evidence of the impact of merit scholarships on graduate school enrollment for the marginal student (i.e., moving from $0 to a low or medium scholarship amount). The analysis of the scholarship name change (DID) provides a glimpse of a low-cost intervention and the impact of it on a subset of students' enrollment decision-making. Evaluating the impacts of scholarships on two distinct populations (high GPA or high-test score) along different eligibility margins helps to distinguish the differential impact of scholarship dollars among subgroups of students. In addition, for practitioners and EM administrators who are wondering, "Does this scholarship eligibility cutoff actually matter to our enrollment numbers?" or "How big of a difference does our new scholarship program make on our yield?"—these analyses and statistical methods provide a framework from which they can answer those questions.

This chapter will proceed with a discussion of the results for the DID, followed by a presentation of the results for each RD. Each section will begin with a presentation of the overall findings, followed by subgroup analyses and limitations.

4.1 Scholarship Name Change DID

The rebranding of the need-based scholarship at this institution was intended to develop a stronger connection with students during the phase between admission and enrollment (i.e., submitting an enrollment deposit) and increase the probability of enrollment among recipients.

This simple change was intended to activate a “gift-exchange relationship” (p. 517) between the student and school (DesJardins & McCall, 2010) where because of the awarding of the scholarship students would “repay” the school initially by choosing it as their graduate school destination, and then once again after graduation as donors to the school (Sherry, 1983). However, this paper examines only the impact of the name change on enrollment.

The decision to change the name of the scholarship to something appearing more personal and valuable (“[Name of School] Scholarship”) from a generic grant (“[Field of study] Grant”) relies on a couple of assumptions about graduate student enrollment behavior. First, I assume that students have a preference for status conferred by a named scholarship (Shurmer, 1971) and will respond positively via their decision to enroll. While there is some evidence that suggests that this may be true (Avery & Hoxby, 2004; DesJardins & McCall, 2010), to my knowledge it has never been rigorously tested. Second, we assume that peer schools, with whom this institution may compete for students, did not alter their scholarship programs in a similar manner simultaneous to this policy change. A review of peer offers submitted by students in the competing aid process suggest that other institutions’ scholarship programs remained unchanged from the year prior to the policy change at this institution.

4.1.1 Main Results

Equation (4) was used to examine the impact of the scholarship name change on enrollments. In Table 4.1, the outcome variable of interest is the interacted treatment effect, which represents the average treatment effect on the treated (ATT) students (i.e., need-based aid recipients for Fall 2021). Column 1 shows the effects for the unrestricted model (no additional covariates included), indicating that the scholarship had no significant impact on enrollment decisions (-0.001; *p* = 0.987). In Column 2, covariates are included to improve precision of the estimates. Again, there is no evidence that the name change affected one's enrollment chances (-0.020; *p* = 0.816).

Table 4.1 *Effect of Scholarship Name Change on Enrollments: Main Results*

	(1)	(2)
	Enroll Immediately	
Treatment	0.319***	0.314***
	(0.029)	(0.029)
Fall 2021 (post)	0.105***	0.089***
	(0.020)	(0.019)
Interacted Treatment Effect	-0.001	-0.020
	(0.087)	(0.087)
Constant	0.217***	0.214***
	(0.012)	(0.013)
Mean Enrollment	0.266	0.266
Observations	8,666	8,666
R-squared	0.030	0.122
Covariates Included	NO	YES
Cohort-Year Fixed Effects	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Sample: Cohorts from Fall 2014-2021. Includes cohort-level fixed effects.
Note: New scholarship name was unveiled in 2021.

4.1.2 Subgroup Analyses

Also tested was whether, despite having no overall impact, the scholarship name change had differential effects on subgroups of interest. The subgroups of interest in this analysis, as well as subsequent RDs, are race/ethnicity, gender, and residency status.

Equation (5) is separately estimated for each of the three subgroups. Tables 4.2-4.4 provide the results of the subgroup analyses. All models were estimated using Eq. 5 without covariates, since the inclusion of them for the main findings did not have a substantive impact on the results. The interacted treatment effect row in each table (the DID estimate) for each subgroup indicates no evidence that the scholarship name-change affected enrollment chances for these groups of students. The standard errors for each estimate are quite large and noisy, likely due to small sample sizes for each subgroup. Because the dataset only includes one cohort in the "post" period, adding additional cohorts would likely help to reduce the noise and more accurately assess whether there are overall or heterogeneous treatment effects.

Table 4.2 *Effect of Scholarship Name Change on Enrollments: By Race/Ethnicity*

	(1) White	(2) Asian	(3) Hispanic	(4) Black	(5) 2 or More
Treatment	0.365***	0.314***	0.377***	0.349***	0.246*
	(0.044)	(0.090)	(0.068)	(0.072)	(0.132)
Fall 2021 (post)	0.194***	0.047	0.088	0.055	0.020
	(0.030)	(0.052)	(0.056)	(0.055)	(0.075)
Interacted Treatment Effect	-0.002	0.164	0.145	-0.244	0.176
	(0.148)	(0.239)	(0.173)	(0.168)	(0.309)
Constant	0.221***	0.224***	0.139***	0.140***	0.224***
	(0.015)	(0.031)	(0.040)	(0.035)	(0.058)
Mean Enrollment	0.29	0.24	0.19	0.16	0.32
Observations	5,146	1,128	802	684	441
R-squared	0.035	0.031	0.085	0.067	0.043
Fixed Effects	YES	YES	YES	YES	YES
Covariates	NO	NO	NO	NO	NO

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Sample: Cohorts from Fall 2014-2021. Cohort-based fixed effects are included.
Note: New scholarship name was unveiled in 2021.

Table 4.3 *Effect of Scholarship Name Change on Enrollments: By Sex*

	(1) Male	(2) Female
Treatment	0.310***	0.330***
	(0.044)	(0.040)
Fall 2021 (post)	0.105***	0.100***
	(0.030)	(0.027)
Interacted Treatment Effect	-0.018	0.018
	(0.122)	(0.125)
Constant	0.235***	0.200***
	(0.017)	(0.016)
Mean Enrollment	0.28	0.26
Observations	4,146	4,476
R-squared	0.029	0.032
Fixed Effects	YES	YES
Covariates	NO	NO

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Sample: Cohorts from Fall 2014-2021. Cohort-based fixed effects are included.
Note: New scholarship name was unveiled in 2021.

Table 4.4 *Effect of Scholarship Name Change on Enrollments: By Residency*

	(1) Non-Resident	(2) Resident
Treatment	0.306***	0.171***
	(0.032)	(0.051)
Fall 2021 (post)	0.071***	0.134*
	(0.020)	(0.070)
Interacted Treatment Effect	0.040	-0.201
	(0.099)	(0.168)
Constant	0.191***	0.647***
	(0.011)	(0.055)
Mean Enrollment	0.74	0.23
Observations	8,055	611
R-squared	0.026	0.035
Fixed Effects	YES	YES
Covariates	NO	NO

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Sample: Cohorts from Fall 2014-2021. Cohort-based fixed effects are included.
Note: New scholarship name was unveiled in 2021.

4.1.3 Limitations

There are several limitations to the DID analysis reported above. As with many real-world institutional policy changes, a researcher is often left to assess a policy under sub-optimal conditions. This section discusses the limitations of the scholarship name change analysis, including challenges related to the need-based aid process and eligibility determination timeline at this graduate school. Since need-based aid at this institution is disproportionately relied on by students who are underrepresented minorities, first generation, and socioeconomically disadvantaged, addressing limitations in future iterations of the program can help to improve enrollment in these target populations.

Students typically begin receiving notification of their admissions decision in late-December or early-January, which is followed closely by their merit scholarship notification, if any. That process occurs on a rolling basis throughout the winter, with a two-week lag for merit scholarships. However, the application period for need-based aid does not open until early-March, allowing much time to pass between the initial contact and when they can even apply for need-based aid. Furthermore, upon completing the need-based application, students still face some degree of uncertainty about whether or not they are eligible for such aid.[33] Students begin to learn of their need-based eligibility in late-March or early-April, with decisions continuing until the late-April enrollment deposit deadline. While this process is unfolding, many students are learning of admissions decisions and scholarship awards from other institutions, which could serve to dilute any tangible impact that a name change could have at that time. In other words,

[33] Upon completing the application, students are notified with one of two messages: "You do not qualify for need-based aid" or "You may qualify for need-based aid. Your file will now be fully reviewed to determine eligibility." If a student receives the former message of ineligibility, they are referred to a page about federal student loans.

the mechanism thought to dictate how naming a scholarship might affect decision making could get negated by the process and by other offers that the students are considering.

Need-based aid recipients may be fundamentally different than their non-need-based peers in unobservable ways. Similar to arguments made about federal financial aid application difficulties by Dynarski & Scott-Clayton (2013) and findings by Bettinger et al. (2012), it could be that the need-based aid application itself is a barrier to enrollment, so those who are eligible and choose to complete applications for such aid are different in unmeasurable ways than those who do not. It could simply be that the resilience and perseverance that is required to navigate the entire financial aid process from start to finish at this graduate school is what is driving those students to 1) enroll at higher rates than their peers and 2) not be affected by naming conventions of their grant aid.

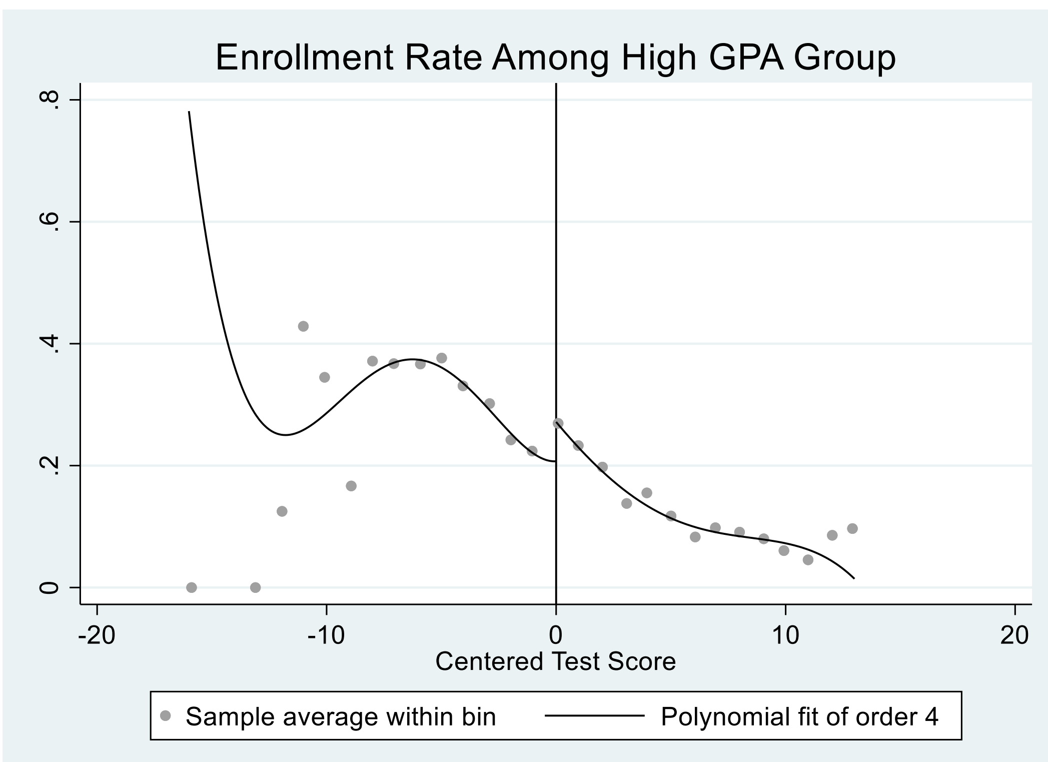
4.2 High GPA Score RD Results

High GPA students with high test scores are sought after by the study institution and its peers. Compared with high-test score students with low enough GPAs to be on the scholarship eligibility margin, high GPA students on the test score margin are very desirable to nearly all graduate schools. This means that competition for them is fiercer, and scholarship amounts typically reflect that. High GPA students on the test score margin experience a substantial change in their net price of tuition as a result of crossing the test score threshold (see Table 3.2). Because of the large change in net price, theory (HCT) and EM practice would suggest that such students, on average, would be more responsive to a relatively large change in price compared to their peers along the GPA margin who received smaller amounts. The next section first examines the main impact of a scholarship offer to high GPA students on the test score margin, followed by an examination of heterogeneous treatment effects and limitations with this analysis.

4.2.1 Main Results

Herein I provide an examination of the effect of a scholarship on enrollment for high GPA students at the test score eligibility threshold. Equations (5) and (6) estimate models for the high GPA group using test score as the running variable. A bandwidth of four is utilized for each analysis based both on optimal bandwidth properties and that students in that bandwidth window have approximately similar graduate school choices as one another. Consistent with prior research using discrete scoring running variables, standard errors were clustered for the running variable to minimize model specification error (Lee & Card, 2008; McCall & Bielby, 2012), and an optimal bandwidth method was utilized to optimize the tradeoff between bias and variance (Imbens & Kalyanaraman, 2012). Figure 4.1 illustrates the jump in the probability of enrollment at the test score threshold among students in the high GPA group.

Figure 4.1 *RD Plot of Enrollment Rate Among High GPA Group, 2014-2021*



Results are reported in Table 4.5 and indicate that scholarship receipt increased the probability of enrollment for high GPA students just above the test score threshold by 21.2 percentage points (*p* < 0.01), on average, compared to peers who did not receive the scholarship (i.e., those just below the threshold). The magnitude of the positive impact of the scholarship on enrollment is consistent with expectations based on the price responsiveness literature discussed earlier (Heller, 1997; Kim, 2010; Leslie & Brinkman, 1987).

To address critiques by Kolesar and Rothe (2018) about the clustering method of standard errors on the running variable, several robustness checks were performed. The results are robust to multiple model specifications (including optimal coverage error rate, heteroskedasticity adjustments, and alternative modeling using triangular kernel weighting) and falsification tests at different thresholds (see Tables C1-C4 in Appendix for details). Alternative models were also estimated for the inclusion of competing aid recipients in the main analysis (see Table C5 in Appendix for details), as well as for robustness checks for each subgroup analysis (see Tables C6-C8 in Appendix for details).

Table 4.5 *Effect of Scholarship on Enrollments for the High GPA Group*

	(1) Main Results
First Stage	0.390***
	(0.024)
Treatment Effect	0.212***
	(0.026)
Centered Test Score	-0.050***
	(0.004)
Constant	0.062***
	(0.020)
Mean Enrollment	0.22
Bandwidth	4
Observations	2,511
R-squared	0.042

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Sample is from Fall 2014-2021. Test score has been re-centered at 0.
Standard errors are clustered by the running variable.

4.2.2 Subgroup Analyses

Next, I examined whether there were heterogeneous treatment effects within this sample. For each subgroup of interest, Eqs. (5) and (6) were estimated to obtain first stage and second stage (treatment effect) estimates for each subgroup. Beginning with the analysis of heterogeneous effects by gender in Table 4.6, scholarship receipt above the threshold increased the probabilities of enrollment by 26.8 and 11.9 percentage points (both are $p < 0.01$) for females and males, respectively.

Table 4.6 *Effect of Scholarship on Enrollments for the High GPA Group: By Gender*

	(1) Female	(2) Male
First Stage	0.406	0.363***
	(0.017)	(0.035)
Treatment Effect	0.268***	0.119***
	(0.035)	(0.030)
Centered Test Score	-0.054***	-0.044***
	(0.005)	(0.004)
Constant	0.006	0.152***
	(0.025)	(0.024)
Mean Enrollment	0.20	0.24
Bandwidth	4	4
Observations	1,422	1,077
R-squared	0.042	0.035

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Sample is from Fall 2014-2021. Test score has been re-centered at 0. Standard errors are clustered by the running variable.

There were also heterogeneous effects of scholarship receipt on enrollment by race and ethnicity. Table 4.7 shows that crossing the test score eligibility threshold had a significant impact on enrollment probabilities for students who are White, Asian, or two or more races. Among White students above the threshold who received a scholarship, the probability of enrollment increased by 18.5 percentage points (p < 0.05), on average, compared to peers just below the threshold. Among students who are Asian or of two or more races, the probability of enrollment under the same conditions increased by 41.6 (p < 0.01) and 51 (p < 0.05) percentage points, respectively, compared to their counterparts below the threshold who did not receive a scholarship. There was no significant impact of scholarship receipt on enrollment for Hispanic and Black students at the test score margin, which could be the result of both competition by peer institutions for those particular students (above the threshold) and fewer alternative options among students below the threshold. Given that both subgroups of students are underrepresented at similar highly selective graduate schools, competition is especially fierce for students with test scores that are situated around target median scores. On the other hand, students with below-median test scores may have fewer comparably selective alternative institutions available to them. Each non-White subgroup had relatively few students in it, so additional evaluations by race/ethnicity with larger sample sizes would help improve the precision of estimates that resulted from the relatively small subgroups of non-White students.

Table 4.7 *Effect of Scholarship on Enrollments for the High GPA Group: By Race*

	(1) White	(2) Asian	(3) Hispanic	(4) Black	(5) 2 or More
First Stage	0.356***	0.554***	0.484***	0.062	0.478**
	(0.040)	(0.063)	(0.048)	(0.379)	(0.206)
Treatment Effect	0.185**	0.416***	0.159	-0.050	0.510**
	(0.083)	(0.127)	(0.103)	(1.705)	(0.200)
Centered Test Score	-0.060***	-0.097***	-0.004	0.027	-0.081**
	(0.013)	(0.031)	(0.017)	(0.215)	(0.033)
Constant	0.116	-0.159	0.007	0.126	-0.145
	(0.076)	(0.121)	(0.079)	(1.179)	(0.128)
Mean Enrollment	0.25	0.16	0.09	0.05	0.21
Bandwidth	4	4	4	4	4
Observations	1,693	323	194	63	102

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Sample is from Fall 2014-2021. Test score has been re-centered at 0. Standard errors are clustered by the running variable.

Heterogeneity of the results was also examined by residency status. Beginning in Column 2 of Table 4.8 with non-resident students, where there is substantially more power than for residents. For non-residents, crossing the test score threshold among scholarship recipients increased the probability of enrollment by 24 percentage points ($p < 0.01$) relative to non-recipients below the threshold. This finding for non-resident students is not much different than those for the whole sample, and that is likely due to the composition of the admitted students being more than 90% non-resident students. Also, there was no evidence that gaining eligibility for a scholarship at the threshold increased enrollment probabilities among resident students. This finding is also not surprising, given the very high average rate of enrollment among resident students (73%) on either side of the test score threshold used for this analysis (see Table E1 in Appendix). One could interpret this result as resident students being less price sensitive than their non-resident peers. Additional research with more data is needed to examine whether this will hold with larger samples of resident students, and if so, why that may be the case.

Table 4.8 *Effect of Scholarship on Enrollments for the High GPA Group: By Residency*

	(1) Resident	(2) Non-Resident
First Stage	0.176***	0.400***
	(0.041)	(0.024)
Treatment Effect	-0.009	0.240***
	(0.655)	(0.066)
Centered Test Score	0.001	-0.051***
	(0.014)	(0.014)
Constant	0.785	-0.016
	(0.637)	(0.059)
Mean Enrollment	0.74	0.16
Bandwidth	4	4
Observations	220	2,291

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sample is from Fall 2014-2021. Test score has been re-centered at 0. Standard errors are clustered by the running variable.

4.2.3 Limitations

The limitations are few for the analyses of the impact of scholarship receipt at the test score threshold. The analyses to determine heterogeneity in treatment effects were limited by sample sizes. The analyses for both race/ethnicity and residency were likely underpowered, which inhibited the ability to have confidence in the weakly significant findings the models produced. The exceptions to the power limitation were the subgroup analyses for males and females, as well as the non-resident portion of the residency analysis.

4.3 High Test Score RD Findings

The awarding process for merit scholarship recipients occurs prior to[34] and is more seamless than the need-based aid process previously described. The simplicity of the awarding process is due in part to the relatively straightforward awarding criteria that is used. The graduate school primarily relies on a threshold-based matrix to serve as a guide for initial decisions about merit scholarship eligibility. Staff involved in the merit scholarship decisions still diverge from the matrix when they see fit, either upward or downward in award amount. This divergence leads to imperfect compliance where students who have scores above the threshold receive a scholarship or and those below the score threshold do not (compliers), but some students with scores above the threshold do not receive scholarships and students with scores below the threshold do (non-compliers). Since the probability of receiving the treatment does not change perfectly from '0' to '1', a FRD design is employed (Lee & Card, 2008; McCall & Bielby, 2012). Scenarios such as these, where staff exercise some judgment over financial aid determination for a student are somewhat common, especially along the GPA threshold. This

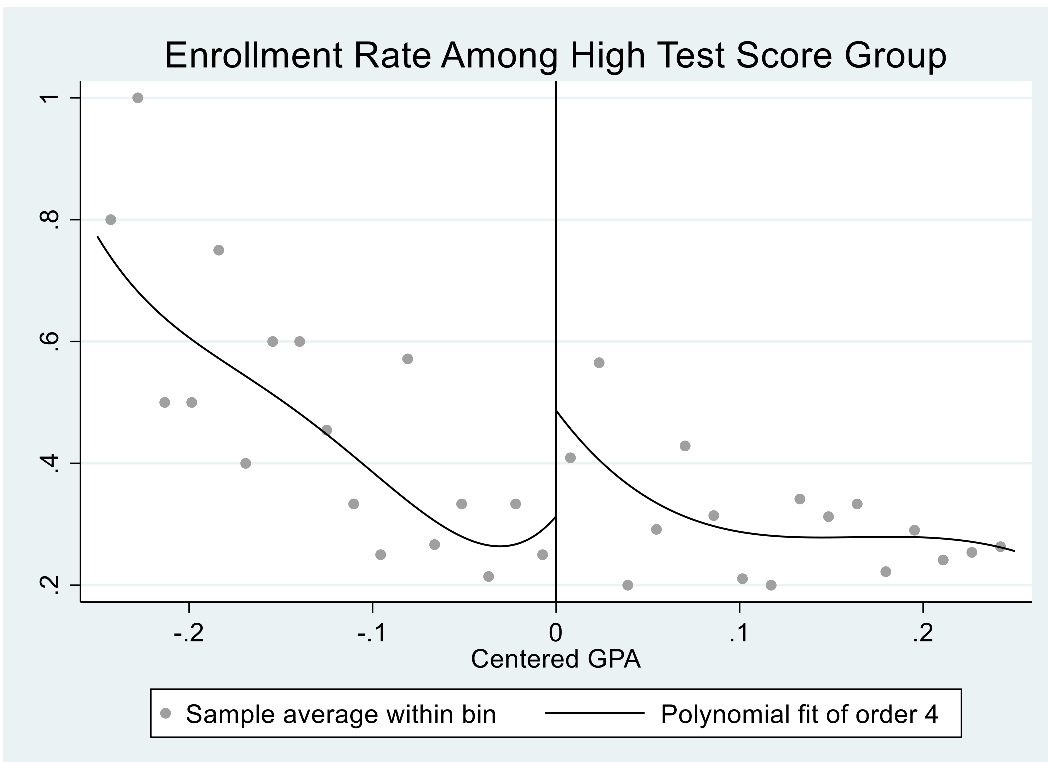
[34] Students are typically notified of merit scholarship award decisions at least two to three months prior to notification of need-based aid eligibility.

section discusses the main results of the RD used to examine the impact of a scholarship for high test-score students on the GPA eligibility margin (i.e., using GPA as the running variable). The overall result discussion is followed by subgroup analyses and limitations.

4.3.1 Main Results

Equations (5) and (6) were used to estimate the impact on enrollment of a scholarship offer to high test-score students on the GPA margin. Figure 4.2 illustrates the jump in the probability of enrollment at the GPA threshold among students in the high test score group.

Figure 4.2 *RD Plot of Enrollment Rate Among High Test Score Group, 2014-2021*



Four separate model specifications were estimated using different GPA bandwidths of 0.15, 0.20, 0.25, and 0.30. Table 4.9 provides first stage and treatment effect estimates. Columns 1 through 4 all show that scholarship receipt among high test score students had a significant and positive impact on enrollment at this institution and was robust to alternative bandwidth specifications. Column 3, the preferred model specification, shows results using the optimal bandwidth of 0.25, estimated separately by optimizing bias and variance tradeoffs (Imbens & Kalyanaraman, 2012). Focusing on the treatment effect, which shows the local average estimated impact of the scholarship at the GPA threshold, those receiving a scholarship had probabilities of enrollment at this institution 63.3 percentage points higher than those not receiving an award ($p < 0.05$). The standard errors are relatively high, and as a result, we cannot rule out at the 95% level that the treatment effect is between 6.1 and 100 percentage points. The high standard errors are likely due to variance resulting from the small number of observations in each model. However, the treatment effects are robust to bandwidth selections and alternative model specifications (see Tables D1-D3 in Appendix). This means that it is quite likely, even if just relying on the cautious lower bound point estimate at the 95% confidence level (6.1 percentage points), that scholarship receipt positively affects enrollments among high test score students at the GPA threshold.

Table 4.9 *Effect of Scholarship on Enrollments for the High Test Score Group*

	(1) 0.15	(2) 0.20	(3) 0.25	(4) 0.30
First Stage	0.355***	0.347***	0.380***	0.432***
	(0.132)	(0.116)	(0.105)	(0.097)
Treatment Effect	0.688*	0.623*	0.633**	0.430*
	(0.404)	(0.354)	(0.292)	(0.223)
Centered GPA	-1.916	-2.145	-2.360*	-1.321
	(2.161)	(1.603)	(1.233)	(0.820)
Constant	-0.220	-0.197	-0.221	-0.052
	(0.346)	(0.300)	(0.252)	(0.190)
Mean Enrollment	0.30	0.30	0.29	0.29
Bandwidth	0.15	0.20	0.25	0.30
Observations	260	372	558	695

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sample is from Fall 2014-2021. GPA has been re-centered at 0. Centered GPA is the running variable.

4.3.2 Subgroup Analyses

Additional models were estimated to determine whether there were heterogeneous treatment effects of the scholarship on various subgroups of interest. Table 4.10 shows results by race, where non-White students were pooled together to increase statistical power (McCall & Bielby, 2012). Among White students above the GPA threshold, scholarship receipt increased the probability of enrollment by 60.6 percentage points ($p < 0.05$) relative to White peers below the threshold. The scholarship had no impact on non-White enrollment. Similar to the main results along the GPA threshold, even though the results are noisy, which means we cannot rule out just a small positive impact of the scholarship on enrollment for White students, the results allow us to reject the null of no effect at the 95% level. Also estimated but not displayed, there were no heterogeneous impacts of scholarship receipt by gender or residency status.

Table 4.10 *Effect of Scholarship on Enrollments for the High Test Score Group: By Race*

	(1) White	(2) Non-White
First Stage	0.510***	0.267*
	(0.146)	(0.140)
Treatment Effect	0.606**	0.480
	(0.299)	(0.584)
Centered GPA	-1.494	-2.795
	(1.547)	(2.147)
Constant	-0.160	-0.158
	(0.252)	(0.533)
Mean Enrollment	0.32	0.25
Bandwidth	0.25	0.25
Observations	339	219

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Sample is from Fall 2014-2021. GPA has been centered re-centered at 0. Centered GPA is the running variable.

4.3.3 Limitations

The limitations with the RD analysis on the GPA margin primarily reside with the administration of the program, rather than a limitation of the modeling itself. The major limitation with this analysis was the lack of power near the threshold to sufficiently conduct subgroup analyses (McCall & Bielby, 2012). The lack of power was largely a function of the small number of students admitted with GPAs below the threshold (n = 77 using a 0.25 bandwidth in the main analysis), and an even smaller density of those who had high enough test scores to possibly receive a scholarship at the GPA margin. Power issues would likely occur with relative frequency when evaluating individual graduate programs with small numbers of students situated around a given threshold, which could limit the ability to produce robust causal estimates to inform future policy decisions. In the next chapter, I discuss how to approach limitations such as these from an administrator's perspective, which will ultimately serve both researchers and practitioners who would like to engage in this type of research.

4.4 Discussion of Limitations

The causal analysis about the scholarship name change, though novel, had many drawbacks that likely led to the null findings. First, as was discussed previously, the process by which students apply for and learn of their need-based aid eligibility, and thus have an opportunity to view the name of the award, is sub-optimal for enrollment decision-making. Due to the nature of the need-based aid program at the institution not becoming available until March (when students otherwise begin hearing about merit aid decisions in January), students sometimes do not apply for need-based aid until months after they have received a merit scholarship determination. All students with a need-based award received an initial scholarship letter (via email) with the name of the need-based scholarship award clearly displayed within a few days of the award determination. However, during the period of time between their merit- and need-based aid determinations, they may have received other merit offers at peer institutions, thus diminishing the impact of the need-based award. An additional drawback includes the small number of observations in the single post-policy cohort. Without a sufficient number of students in the post-period, there are not enough data points to establish the statistical power necessary to detect any treatment effects (McCall & Bielby, 2012). All of these things likely underpin the null findings from the DID analysis about the scholarship name change.

Scholarship receipt did impact enrollment among high test-score students at the GPA margin, but power limitations probably hinder the takeaways. As reviewed in Table 3.2, the total number of students below the GPA threshold (n = 97) probably left the main analysis underpowered. In addition to having insufficient observations, 3.2 shows an average scholarship amount *below* the threshold of nearly $12,000, which is approximately equivalent to the award they would have received (averaged over the sample) had a student crossed the threshold. Theoretically, students below that threshold should not be eligible for any merit-based scholarship, but staff possess the ability to exercise judgment to adjust scholarship eligibility as they see fit. For internal validity purposes, one would like to see better adherence to the established threshold when awarding scholarships to better establish the impact of moving from no scholarship amount to a low scholarship amount. Furthermore, given how few students are below the threshold, it may be reasonable to consider increasing the cutoff to amass additional data points and better evaluate the impact of scholarship support to high test score students. Until more precise estimates can be produced, one should rely on the lower bound of the 95% confidence interval of the results to not overstate the impact of scholarship receipt at the GPA margin. Both of which are discussed in the next chapter.

Beyond data, there are limitations to studying graduate programs that complicate the study of students' enrollment decision-making. Graduate schools are intentionally narrow in scope; students often seek a graduate school based on a particular specialization or because of its ability to help the student achieve its specific career goals. If the school or program is not a match for the student's short-term career prospects, then they are likely to seek a better fit with one of the institution's peers. Offering recruitment scholarships will have little, if any, effect on inducing enrollment if the institution cannot meet the professional needs of a student. In addition, enrollment deposit deadlines for this program (nationally) are staggered throughout the spring, which limits a student's ability to participate in the entire admissions cycle. Students may be faced with a scenario where they have to respond and submit a deposit to hold a seat at one institution while they are still awaiting an admissions or scholarship decision from another. Finally, students may approach enrollment decisions more conservatively depending on how much student loan debt they have already incurred, especially if they are faced with having to borrow an additional $40,000-$60,000 per year to enroll (see calculation from Section 3.1.1).

4.5 Overall Summary of Results

To summarize the overall results, the name of scholarships does not appear to impact enrollment decisions, but receipt of a scholarship (compared to no receipt) does increase the probability of enrollment at this graduate school. These findings extend the existing research on scholarship naming conventions while establishing the first evidence to support the transferability of the positive impact of scholarship aid on enrollment from undergraduate settings to graduate schools. Among high GPA students at the test score margin, the magnitude of effect is smaller relative to the GPA margin, but the confidence intervals at the 95% level are also much smaller (i.e., less noise), which provides for clearer takeaways and policy implications. Scholarship receipt among high test score students at the GPA margin increases the probability of enrollment by a large magnitude, but with relatively noisy results (wide confidence intervals). Even still, the null hypothesis of a scholarship having no effect at the GPA margin is rejected, which lends confidence to the positive direction of the findings and gives room for future research to utilize more data to increase precision (i.e., reduce noise). Finally, the positive impact of the merit scholarships on enrollment at both score thresholds (GPA and test score) do not appear to be experienced by underrepresented minority students, where there is increase in the probability of enrollment at either margin as a result of scholarship receipt. A discussion and implications of these findings are discussed in the next chapter.

Chapter 5 Discussion and Policy Implications

From the analyses conducted, a few key themes emerge. First, I discuss the main takeaways from all three sets of analyses. Next, a discussion of the policy implications is provided, and I make recommendations based on the findings. The dissertation ends with a discussion about directions for future researchers who seek to build on this work.

5.1 Main Takeaways

Medium scholarship amounts at the test score margin (high GPA group) had a strong impact on enrollment at this graduate school and are consistent with prior literature. The main findings at the test score margin, a 21.2 percentage point increase in enrollment, must first be converted to an interpretable value to compare to existing literature. All monetary values reported in this section are in 2021 dollars. Dividing the percentage point change by the average annual scholarship amount at the threshold over the eight cohorts ($23,125), the findings equate to an estimated increase in the probability of enrollment at the margin by 1.09 percentage points per $1,000 (in 2021 dollars) increase in merit scholarship aid. The direction of effects (positive) is indicative of the scholarships having an effect on enrollments, but the magnitude is only slightly smaller compared to St. John (1990) and Dynarski (2000) who found enrollment increases of 1.5 and 2.2 to 2.5 percentage points per $1,000, respectively. The results are also lower than Avery and Hoxby's (2004) finding of a 7% increase per $1,000 of grant aid. However, the results are very similar to the findings by Hurwitz (2012) of a 1.31 percentage point change per $1,000 increase in grant aid. This one is especially compelling given that it examined circumstances where students were choosing between schools rather than whether to enroll at all. Also, the main finding is in contrast to Porter et al.'s (2014) null finding, but they only examined enrollment responsiveness between two existing scholarship offers (rather than zero and some large amount, as in my case). Here, the impact being estimated is of scholarship receipt changing from practically zero to a substantial amount, so one would expect that, based on all prior financial aid price responsiveness literature (Dynarski et al., 2022; Heller, 1997; Kim, 2010; Leslie & Brinkman, 1987), scholarships would induce positive enrollment effects. Despite this being a study of a single, selective graduate school, the results are consistent in terms of direction (positive) with prior undergraduate literature and adds to the literature by providing rigorous evidence of the impact of scholarship offers on graduate school enrollment in a single setting.

The analyses conducted herein are available for EM administrators to employ, but there are a few things to be mindful of when considering this type of evaluation. First, a solid understanding of the data generating process (i.e., underlying mechanisms) operating in terms of the aid provision processes is fundamental. Second, one must ensure that there is sufficient data to power the analysis. Policy implications from each analysis and tips for improving statistical power for future analyses are discussed below.

5.2 Policy Implications

There are many areas where graduate school enrollment management efforts could be improved, including the timing of awarding procedures, altering merit eligibility thresholds, and revising the awarding philosophy to better target scholarship programs. This section discusses policy implications resulting from each main analysis and provides context for graduate schools looking to improve EM practices at their own institutions.

5.2.1 Scholarship Name Change Analysis Discussion

The DID evaluation of the impact of the scholarship name change, despite null findings, provides helpful insight for why administrators should utilize a similar intervention and how they can improve it from the process used at the study institution. First, the nature of the intervention makes this an appealing option to administrators who may be looking for EM strategies without having to make a significant financial or time investment. Second, many of the limitations of the DID can be mitigated in future evaluations, such as through more optimal timing of awarding procedures to better leverage the theoretical benefits of a gift-exchange relationship (Akerlof, 1982, DesJardins & McCall, 2010), and adding more data by pooling more cohorts as they become available. This section discusses the cost of the intervention, as well as areas of improvement for future administrators seeking to implement similar policies or evaluate existing ones.

The intervention evaluated in the DID analysis was low-cost and simple to implement relative to alternatives, such as the cost of providing scholarship aid that was evaluated in both RDs. This institution demonstrated that they could implement a no-cost policy change that targeted a specific group of students (need-based recipients). Changing the name of the scholarship in the financial aid system required only a single staff member to spend 10 minutes of their time (and no additional financial resources) updating background data in the system. While the intervention itself produced no impact on enrollment, it demonstrates the possibility that exists for other institutions to implement a similar no-cost approach to leverage naming conventions of scholarships in an effort to better connect with prospective student subgroups of interest. Future iterations of this intervention could consider utilizing names that appear to be even more prestigious than the name of the school, such as “Dean’s Select Merit Scholarship,” or the specific name of an individual donor-funded scholarship, such as "John Smith Endowed Scholarship." In this particular program of study (and likely other terminal graduate programs), scholarships are listed on student's resume and recognized by alumni in the broader hiring network. I would speculate that more optimal timing and prominent messaging (e.g., highlighting that name and background of the award on a scholarship letter with the admissions offer) would have a positive influence on enrollment decisions.

To do so, need- and merit-based scholarship notifications to a student should occur closer to one another (or simultaneously) to optimize the gift-exchange nature of the scholarship. At the study institution, need-based scholarship notifications can be sent weeks or even months after initial merit scholarship decisions are made due to administrative processing constraints and the timing of when a student chooses to submit the need-based aid application. Students who may be eligible for need-based aid receive merit scholarship determination letters that detail either a small merit scholarship or no scholarship at all, at which point they are invited to apply for need-based aid. By the time students are notified of their need-based aid eligibility, the window for establishing a gift-exchange relationship may have already closed. Many prospective students who were admitted to this institution were also admitted to peer graduate schools whose merit scholarship decision timelines mirror the study institution.[35] The delay in this institution's need-based aid notification means that students may not have an opportunity to evaluate the total financial aid package at the same time that they are evaluating other schools' offers. In effect, they might make their initial evaluations using an incomplete "gift" amount from the study institution, and by the time need-based aid eligibility is determined, the piecemeal nature dilutes

[35] The graduate school receives a regular report from a third-party organization to which it and peer schools belong. The report details the total number of overlapping prospective and admitted students that the study institution has with peer schools. Merit scholarship decision timelines of peer schools are widely available to administrators at this institution.

or even negates its impact. Aligning the merit- and need-based award notification processes may increase the impact of the "gift" and maximize the likelihood of it influencing a student's decision to enroll ("exchange").

Finding avenues for increasing data available for analyses will help alleviate power concerns and strengthen future analyses. One way to easily increase data for an analysis would be to add additional cohorts of students. Using the study institution, one would only need to wait one additional admissions cycle to approximately double the number of treated individuals, all other things equal. One should also seek to maximize the number of cohorts in the "pre" period of the policy intervention, as well. Taking these steps will improve precision when cohorts remain pooled for main analyses, but power issues may still persist in subgroup analyses, particularly at graduate schools who admit/enroll fewer students than the school in this paper.

Administrators may also be able to increase data by revising the conditions under which students are invited to apply for need-based aid. By reserving the need-based aid application for students who do not receive a sufficient enough merit scholarship to disqualify them from need-based eligibility, the pool of applicants is unnecessarily constrained. Inviting students to submit a need-based aid application upon admission would generate more applicants, and thus, more data points for analyses. While it may seem that the cost of such an endeavor would be prohibitive because so many more students may apply for need-based assistance, designating need-based scholarships as "last dollar"[36] would minimize any additional expenditure by only paying out additional scholarship dollars to students who do not ultimately receive a merit scholarship and would not have otherwise applied for need-based aid. The "last dollar" approach may be an

[36] "Last dollar" refers to how financial aid is considered when packaging a student's award. For example, if a need-based scholarship was a last dollar award, then a student would only be eligible for a given amount less other grant aid (such as merit scholarships) that they may be receiving. In some cases, a student may lose eligibility for their last dollar award because they are receiving sufficient funding via other awards.

attractive option for any institution that wants to establish a connection with students through simultaneous merit- and need-based aid offerings but does not have the budget to pay out both scholarships in full to each eligible student. Finally, addition to the data, it also better aligns merit- and need-based timelines, which optimizes a gift-exchange approach to enrollment.

5.2.2 RD using GPA as the Running Variable Discussion

There are three main policy implications from the GPA threshold-based scholarship program at this institution. First, the criterion used for the threshold is so low relative to the median GPA of each cohort that it applies to very few students. Consideration should be given to increasing the threshold in an effort to impact more students. Second, the point estimate from the main analysis of the impact of a scholarship at the GPA threshold should be treated conservatively. Statistical power limitations led to imprecise point estimates in the main and subgroup analyses at the GPA margin. Administrators should take care to not make policy decisions based on the magnitude of the effect and instead rely only on the positive direction of the findings. Addressing these drawbacks can help improve the impact of scholarship awards to students with high test scores who are on the GPA margin. These implications are discussed in greater detail below.

The GPA threshold used by the institution presents two problems that can be overcome by utilizing a higher GPA scholarship threshold. Elevating the GPA threshold (i.e., shifting it to where there is a higher density of students in the GPA distribution) will increase the number of student observations subject to the policy, which will enable EM administrators to reserve greater discretion about to whom they would like to award a scholarship, as an exception, below any newly established score threshold. This would enable administrators to more closely monitor budgetary expenditures and make targeted awards to students who enroll at below-average rates to better craft their cohorts.

Elevating the GPA threshold will also enable future researchers to utilize more data points to conduct each analysis, thus helping to address statistical power issues that hampered most subgroup analyses and limit the takeaways from the main RD analysis. One potential approach to addressing statistical power is to increase the GPA threshold to 3.50. As an example, a shift to a 3.50 GPA threshold using a 0.25 bandwidth (compared to 3.30 and 0.25 bandwidth) increases the number of total observations to 1,363 from 558.

However, there are tradeoffs to *increasing* the minimum GPA eligibility for the merit-based scholarship at this institution. An increase of 0.20 in the threshold (using the same bandwidths) translates into a sample that is comprised of a greater proportion of White students than the current sample (64.8% vs. 60.8%), meaning fewer proportions of underrepresented minority students would be in a position to gain eligibility for a scholarship. The downward change in the proportion of underrepresented minority students is consistent with prior literature that suggests that White students disproportionately accrue the benefits of merit-based aid programs (see Heller (1997) or Kim (2010) for reviews). As such, policy decisions related to threshold *increases* should be weighed against the effect that it may have on scholarship eligibility and access to the institution for underrepresented minority students.

5.2.3 RD Using Test Score as the Running Variable Discussion

The analysis of the impact of scholarship receipt at the test score margin provided many helpful policy takeaways. First, it established evidence of the impact of receiving a medium scholarship on graduate school enrollment compared to no scholarship receipt, which supports the transferability of HCT to a graduate school setting and can help inform future policymaking.

It also demonstrates how, compared to the analysis at the GPA margin, additional observations provide the statistical power for more precise point estimates, which enable clearer policy implications. Finally, the analysis at the test score threshold further highlights who benefits from merit-based thresholds. These implications are discussed in greater detail below.

Researchers should conduct a power analysis prior to analyzing data to ensure that estimates are sufficiently powered. When estimates are noisy, additional data improves precision (McCall & Bielby, 2012). To maximize data for analyses, researchers should consider pooling cohorts to increase power and be mindful of how the samples are restricted to limit students who are excluded from the sample. As an example, compared to the analysis at the GPA margin, the analysis at the test score margin had substantially more observations, and thus, better power. Additional data points enabled more precise estimates (i.e., narrower 95% confidence intervals), which allows for clearer policy takeaways. For example, despite the larger point estimate at the GPA margin compared to the test score margin (63.3 vs. 21.2 percentage points), at a 95% confidence level, one cannot rule out the lower-bound possibility that the estimated impacts of scholarship receipt on enrollments were only 6.1 and 16.2 percentage points, respectively. This comparison demonstrates why EM administrators should be cautious in implementing policy changes as a result of noisy findings and why they can have greater trust with data that can produce precise estimates with relatively narrow confidence intervals to better guide decision-making. The data limitations of the analyses in this paper illustrate the challenges that EM administrators may face working with small datasets and trying to develop actionable policy takeaways. Imprecise estimates (i.e., wide confidence intervals) hinder the ability for EM administrators to make policy decisions about which they can have reasonable certainty in a range of outcomes.

Medium scholarships are successful policy interventions to induce enrollment at this graduate school. HCT suggests that students make the decision to enroll after carefully evaluating the net price of enrollment relative to other available options (Becker, 1962), which may include enrollment and scholarship receipt at peer institutions. The RD analysis of scholarship receipt at the test score margin demonstrates that changes in net price as a result of a merit scholarship do affect enrollment behavior among prospective graduate students. While these findings were the result of a single-institution analysis, given the consistency of the evidence of positive impacts of scholarships on enrollment throughout the literature, we speculate that institutions similar to the study institution would also see a positive impact of scholarships on enrollment.

The impact of scholarship receipt on enrollment at the test score margin also unveiled disproportionate benefits by race. Scholarship receipt did not have an impact on enrollment for Hispanic or Black students, whereas it did for students who are White, Asian, or of two or more races. These findings could be the result of high competition among peer institutions for underrepresented students with above-threshold scores (and thus more options and more potential scholarship dollars), as well as the relative surplus of White and Asian students with above-threshold scores (and thus less competition by peers). I would also speculate that, in an effort to balance priorities related to median test scores and cohort demographics, peer institutions may choose to allocate greater financial resources for recruitment scholarships or simply negotiate or match scholarships from peer schools, which would all have the effect of dampening the impact of scholarship offers made at the study institution. As test scores increase, the proportion of White students steadily increases, while the proportions of Black and Hispanic students steadily decrease, on average. As reviewed in Table 3.2, Hispanic and Black students are overrepresented below the threshold, whereas White students are overrepresented above, meaning White students are more likely to be eligible for the scholarship program. Overrepresentation of White students among merit-aid recipients is consistent with the findings from the analysis of the merit-based GPA threshold, as well as prior literature (see Heller (1997) and Kim (2010) for reviews).

There are pathways that exist for addressing scholarship inequities as a result of reliance on standardized test scores. Compared to the disproportionately high non-White composition of need-based aid recipients, one might think that shifting the institutional scholarship awarding model to need-based approach would induce more diverse enrollment at this school. Alternatively, the institution could consider targeting aid (via similar software-based low-cost measures discussed above in the DID analysis) toward individuals who do not meet either scholarship criteria but whose rate of enrollment are disproportionately low relative to their rate of admission.

One could also imagine how merit scholarship awarding could change for institutions that do not utilize standardized test scores. Administrators could perform a more holistic review of the application that examines components such as field of study-specific work experience or volunteer experience. Similar to the experiment operated by Field (2009), institutions could choose to award scholarships based on what career area a student indicates they would like to pursue *after* graduation, with strings attached should their plans change. Institutions could also choose to do a combination of merit and need, where students can be eligible for an award if they at least meet a minimum GPA *or* a minimum standard of financial need; students who meet or exceed both thresholds could be eligible for supplemental funding. Using this institution as an example, each option mentioned above would likely promote greater equity in scholarship distribution overrepresentation of White and Asian students with above-median test scores.

5.3 Directions for Future Research

This causal study assessed the impact of scholarships at a single graduate school, but there are numerous additional avenues to explore in future research. First and foremost, future research should address power limitations above by performing a proper power analysis. Researchers should also evaluate whether scholarships awarding *within* a matrix (like the one described for this school) have an impact on graduate school enrollment decisions, rather than limiting evaluations only to the minimum thresholds.

Next, because it is assumed that all graduate students will be eligible for any amount of federal loan aid that they desire, I did not explore whether receipt of loans moderated any effects of the scholarship provided. Future researchers should make an effort to explore this avenue by obtaining data from admitted students' entire award package. I would speculate, based on publicly available national average loan data for students pursuing this graduate program of study, that the majority students are simply financing most, if not all, of their remaining net price after scholarships. If that is true, then researchers should test and compare price responsiveness between scholarship/grants and federal loans. In addition, researchers should also examine whether the presence of institutional loan repayment assistance programs has any impact on enrollment behavior, especially among students who do not qualify or qualify for a small merit scholarship award. I would speculate that having a loan repayment assistance program would ease concerns of especially debt-averse populations, particularly first-generation and low socioeconomic status students, and provide the assurance needed to enable enrollment. Framing the loan repayment assistance as a "guarantee" under certain conditions could strengthen its impact (Dynarski et al., 2021; Dynarski et al. 2022). Related to loan aid, researchers should also examine whether prior undergraduate student loan debt has any impact on enrollment behavior.

Data should be collected from students regarding the schools to which they applied. To craft a picture of a student's choice set, researchers should survey prospective graduate students to determine which schools are in an institution's true peer group. In other words, it could be beneficial for EM purposes to know the other institutions that students would choose between under similar financial conditions. Researchers should also examine whether the timing of the application during the admissions cycle influences the likelihood that a student enrolls.

Finally, future research should consider whether geography (e.g., a student's home state) or school choice set (i.e., institutions to which they were likely admitted and could choose to enroll based on GPA and test score) influence enrollment probabilities. For example, if students from different states (or regions of states) with equal scholarship aid enroll at different rates, then EM administrators could choose to target scholarship opportunities toward areas where there is low enrollment or simply use the information to better predict class yields and craft future cohorts. Having a better understanding of a student's possible choice set would enable EM administrators to be more efficient with aid expenditures (i.e., reduce/not award any scholarship aid) when it appears students with particular score sets are likely to enroll with little or no scholarship support. Additional research on the impact of geography and a student's choice set at could improve the information available to EM administrators to improve institutional EM strategies.

Chapter 6 Conclusion

This dissertation estimated separate causal impacts of need- and merit-based scholarship programs on graduate school enrollment. From fall 2014 to fall 2020, grant recipients in the need-based aid program received a scholarship named “[Field of Study] Grant,” which was changed to “[School Name] Scholarship” for the fall 2021 cohort. Using pre/post data, a DID was employed to estimate the causal impact of a more prestigious-sounding need-based scholarship name on enrollment. The merit-based scholarship program was also evaluated. Students qualified for a merit-based scholarship by either having a high test score and meeting or exceeding the minimum GPA eligibility threshold *or* by having a high GPA and meeting or exceeding the minimum test score threshold. Since compliance was imperfect around both thresholds, two separate FRDs were employed to estimate the causal impact of scholarship receipt at each threshold on enrollment.

Two central frameworks guided the research. Demand theory is the foundation upon which scholarships influence enrollment decision-making. As the net price that a student faces decreases as a result of scholarship aid, the quantity demanded (enrollment) increases (Heller, 1997; Leslie & Brinkman, 1987). HCT identifies price (net price) an important component of the direct costs students must consider when choosing to enroll (Becker, 1962; 1994; Mincer, 1958; Schultz, 1961). As students weigh the decision to enroll at this specific institution against available alternatives, such as enrollment at a peer institution or not enrolling/remaining employed, price (net price) is an important consideration that influences decision-making.

A third framework, gift-exchange theory, underpinned the scholarship name change analysis. Adapted from anthropology, gift-exchange theory frames a student's decision to enroll as a repayment for the gift (i.e., named scholarship) (Akerlof, 1982; DesJardins & McCall, 2010). Using this framework, the paper evaluated whether a more prestigious-sounding scholarship name (gift) induced enrollment (repayment).

The research answered four main research questions. 1) Merit-based aid has a significant influence on enrollment at both the GPA and test score eligibility margins. Due to data limitations, the impacts of other types of aid, such as need- and loan-based, are still questions for future research. 2) Differences in the amount tuition is discounted via financial aid do affect admitted students' enrollment probabilities. Comparing lower bound point estimates in the 95% confidence interval, students receiving medium merit scholarships at the test score threshold were 16.2 percentage points more likely to enroll than their below threshold peers, whereas students receiving small scholarships at the GPA threshold were only 6.1 percentage points more likely to enroll than their below threshold peers. 3) Changes in the naming conventions of financial aid awards does not affect graduate student enrollment chances. 4) There are heterogeneous treatment effects of merit-aid at each score threshold. Among high test score students at the GPA margin, scholarship receipt significantly increased the enrollment probability for White students relative to their below threshold peers. Among high GPA students at the test score margin, scholarship receipt above the threshold significantly increased the probability of enrollment for students who are female, male, White, Asian, of two or more races, or non-residents.

This dissertation provided the first causal evidence of the impact of scholarships on graduate school enrollment decisions at a single institution. The findings, while limited in external validity, provide a foundation against which EM professionals can compare future findings of their own. More importantly, it provides a blueprint for how the most relevant statistical analyses for pre/post policy changes (DID) and threshold-based scholarship programs (RD) can be utilized to estimate causal impacts. While the results of this paper pertain to a single, highly-selective graduate school at a public institution, the conditions for the analyses described above are often present at many different types of graduate schools and institution-types, and therefore applicable.

The methods discussed in this paper have the potential to arm EM decision-makers with the information necessary to evaluate and optimize their EM strategies. They also provide EM administrators the ability to determine for themselves whether their recruitment efforts actually impact enrollment and to what extent they do. Furthermore, this paper provides the necessary framework for determining impacts across different subgroups of interest, which can aide any targeted enrollment efforts, and situating any effects against prior literature.

This paper establishes the general transferability of the prior literature regarding the impact of scholarships on undergraduate enrollment to graduate school enrollment. While the majority of the literature examined the impact of scholarships on undergraduate enrollments, this paper provides the first causal evidence that scholarships also induce enrollment in a graduate school setting. The overall findings were broadly consistent with the economic concepts used to frame the work and the extant literature, suggesting that graduate students respond to changes in net price as a result of discounts via scholarship aid. Still more data and research are needed to evaluate whether scholarship names matter or subgroup differences exist along the GPA threshold at this institution, but these analyses enable EM decision-makers at graduate schools to have confidence that financial aid via scholarships does positively impact enrollment and can be utilized as a tool to craft cohorts. Furthermore, despite the null impact of the scholarship name change on enrollment, this paper details how low-cost/no-cost scholarship interventions can be implemented and evaluated, which may be an attractive option to EM decision-makers looking to possibly impact enrollment with a limited budget. Adding these resources to the toolkit of an EM administrator enables more robust targeting of scholarship aid based on evaluation findings and helps to optimize student recruitment efforts.

Appendices

Appendix A. How to Calculate Tuition Elasticities for EM

It is helpful to view the effects of different magnitudes of tuition elasticities on projected changes in enrollment so that one can more readily comprehend findings in the literature. Below, examples of two different magnitudes are utilized to convey the corresponding effect on the projected change in quantity of enrollment demanded. Using the presentation format provided by DesJardins and Bell (2006) as a guide, a demonstration is provided of the projected effects of tuition elasticities of -0.15 and -0.90 on the change in quantity demanded of enrollment using a price increase of 10% in both instances. First, using an elasticity of -0.15:

$$-0.15 = \frac{\%\Delta enrollment}{10\%}$$

As DesJardins and Bell (2006) note, it is often easiest to rearrange the terms to solve for the percentage change in enrollment:

$$\%\Delta enrollment = E_p * \%\Delta price \tag{A1}$$

Substituting known terms:

$$\%\Delta enrollment = -0.15 * 10\%$$

$$\%\Delta enrollment = -1.5\%$$

The above illustration shows that, despite a large tuition increase of 10%, the institution can expect a reduction in the quantity demanded (enrollment) of only 1.5% for this particular group of students. To illustrate the effects of the same price increase on a group of students who have greater elasticity (in absolute value), the projected effect on quantity demanded of enrollment using a tuition elasticity of demand of -0.90 is as follows:

$$\%\Delta enrollment = -0.90 * 10\%$$

$$\%\Delta enrollment = -9.0\%$$

Applying the same equation to a group of students with a -0.90 tuition elasticity, the institution can expect a nine percent reduction in enrollments if tuition is increased by 10%. As the above equations illustrate, students who have a greater (in absolute value) elasticity are considered to be more tuition elastic and sensitive to changes in price than their lower, more inelastic (closer to zero) peers. This also highlights why knowing whether there are different elasticities among subgroups, because they might be differentially responsive to the same change in price for the same good (college enrollment). Knowing and understanding how tuition elasticities of enrollment operate enables accurate accounting of enrollment projections for future classes, as well as comprehending the effects of various efforts to provide differential net prices for strategic enrollment purposes.

Appendix B. Balance Checks

Table B.1 *RD Balance Checks for Test Score Running Variable*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Sex		Race/Ethnicity							Residency	
	Female	Male	White	Asian	Hispanic	Black	2 or More	Hawaiian	Not Indic.	Resident	Non-Res.
RD Estimate	-0.038	0.031	0.054	0.005	-0.049	0.016	-0.024	0.005*	-0.004	0.009	-0.009
	(0.061)	(0.061)	(0.060)	(0.040)	(0.038)	(0.020)	(0.023)	(0.003)	(0.029)	(0.040)	(0.040)
Observations	3,812	3,812	3,812	3,812	3,812	3,812	3,812	3,812	3,812	3,812	3,812
Bandwidth	3.752	3.770	3.712	3.887	3.765	3.985	4.973	5.646	4.020	3.546	3.546
Effective Obs.	2056	2056	2056	2056	2056	2056	2511	2860	2511	2056	2056

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Balance check for discontinuities of pretreatment characteristics using test score as the RV. Fall 2014-2021.

Table B.2 *RD Balance Checks for GPA Running Variable*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Sex		Race/Ethnicity				
	Female	Male	White	Asian	Hispanic	Black	2 or More
RD Estimate	0.114	-0.081	0.301*	-0.102	-0.058	-0.176	-0.063
	(0.153)	(0.150)	(0.174)	(0.160)	(0.131)	(0.118)	(0.047)
Observations	2,973	2,973	2,973	2,973	2,973	2,973	2,973
Bandwidth	0.0958	0.102	0.135	0.117	0.106	0.117	0.115
Effective Obs.	151	170	234	191	170	191	191

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Balance check for discontinuities of pretreatment characteristics using GPA score as the RV. Fall 2014-2021.

Note: Categories for Hawaiian, Not Indicated, Resident, and Non-Resident had too few observations to perform RD estimation.

Appendix C. **Falsification and Robustness Checks for High GPA Group**

Table C.1. *Falsification Tests with Alternative Cutpoints Using RDRobust, High GPA Group*

	(1) Cut -2	(2) Cut -1	(3) Cut +1	(4) Cut +2
Treatment Effect	0.046	0.487	0.194	0.246
	(0.414)	(0.443)	(0.312)	(0.219)
Observations	3,812	3,812	3,812	3,812
Effective Obs.	1424	1754	2642	2690
Bandwidth	3.091	3.860	4.979	4.887
Order polynomial	1	1	1	1

Standard errors in parentheses are heteroskedastic-robust
*** p<0.01, ** p<0.05, * p<0.1
Sample: Cohorts from Fall 2014-2021. Standard errors use Eicker-Huber-White HC3 adjustment. Optimal bandwidths are used. Falsification check at alternative test score bandwidths. Columns correspond to false cutoff scores relative to centered test score.

Table C.2. *Alternative Bandwidth Checks Using RDRobust, High GPA Group RD*

	(1) BW 4	(2) BW 5	(3) BW 6	(4) BW 7	(5) BW 8	(6) BW 9	(7) BW 10
First Stage	0.221***	0.413***	0.420***	0.401***	0.372***	0.362***	0.364***
	(0.001)	(0.083)	(0.059)	(0.049)	(0.044)	(0.036)	(0.030)
Treat. Effect	0.150***	0.160***	0.181***	0.224***	0.260***	0.272***	0.292***
	(0.003)	(0.010)	(0.015)	(0.032)	(0.036)	(0.035)	(0.045)
Observations	3,812	3,812	3,812	3,812	3,812	3,812	3,812
Effective Obs.	2056	2511	2860	3137	3349	3483	3582
Bandwidth	4	5	6	7	8	9	10
Order polynom.	1	1	1	1	1	1	1

Robust and clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Sample: Cohorts from Fall 2014-2021. Standard errors are clustered on the running variable. Each column represents a test score bandwidth.

Table C.3. *Alternative RD Specifications Using RDRobust: High GPA Group*

	(1) Heteroskedasticity Adjustment	(2) Optimal Coverage Error Rate
First Stage	0.380***	0.395***
	(0.066)	(0.050)
Treatment Effect	0.259*	0.231***
	(0.145)	(0.036)
Observations	3,812	3,812
Effective Obs.	2511	2056
Bandwidth	4.778	3.620
Order polynomial	1	1

Robust and clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Sample is from Fall 2014-2021. Test score has been re-centered at 0. In Column 1, standard errors are adjusted using Eicker-Huber-White HC3 adjustment. In Column 2, standard errors are clustered on the running variable but an adjustment in the bandwidth selection is made for optimal coverage error rate. Robustness checks for Table 4.5.

Table C.4. *Main Results, High GPA Group using RDRobust*

	(1) Enrolled
First Stage	0.390***
	(0.050)
Treatment Effect	0.246***
	(0.037)
Observations	3,812
Effective Obs.	2511
Bandwidth	4.277
Order polynomial	1

Robust and clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sample is from Fall 2014-2021. Test score has been re-centered at 0. Standard errors have been clustered on the running variable. Results show enrollment among high GPA students at the test score threshold. Robustness check for Table 4.5.

Table C.5. *Main Results, High GPA Group Including Competing Aid Recipients*

	(1) Main Results
First Stage	0.231***
	(0.024)
Treatment Effect	0.070*
	(0.042)
Centered Test Score	-0.043***
	(0.006)
Constant	0.205***
	(0.036)
Mean Enrollment	0.25
Bandwidth	4
Observations	2,943
R-squared	0.053

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Sample is from Fall 2014-2021. Test score has been re-centered at 0. Standard errors are clustered by the running variable. Sample includes all students who participated in the competing aid process. Robustness check for Table 4.5

Table C.6. *Main Results, High GPA Group Using RDRobust: By Sex*

	(1) Male	(2) Female
First Stage	0.417***	0.388***
	(0.078)	(0.033)
Treatment Effect	0.199***	0.317***
	(0.056)	(0.066)
Observations	1,695	2,093
Effective Obs.	886	1422
Bandwidth	3.902	4.433
Order polynomial	1	1

Robust and clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sample is from Fall 2014-2021. Test score was re-centered at 0. Standard errors have been clustered on the running variable. Results show enrollment among high GPA male and female students at the test score threshold. Robustness check for Table 4.6.

Table C.7. *Main Results High GPA Group Using RDRobust: By Race/Ethnicity*

	(1) White	(2) Asian	(3) Hispanic	(4) Black
First Stage	0.298***	0.736***	0.569***	0.192
	(0.050)	(0.103)	(0.051)	(0.419)
Treatment Effect	0.099	0.796*	0.046	0.133
	(0.083)	(0.457)	(0.086)	(0.377)
Observations	2,555	461	293	160
Effective Obs.	1693	190	151	78
Bandwidth	4.363	2.883	3.727	5.289
Order polynomial	1	1	1	1

Robust and clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sample is from Fall 2014-2021. Test score was re-centered at 0. Standard errors clustered on the running variable. Results show enrollment by race among high GPA students at the test score threshold. Robustness check for Table 4.7.

Table C.8. *Main Results High GPA Group Using RDRobust: By Residency*

	(1) Resident	(2) Non-resident
First Stage	0.319***	0.380***
	(0.018)	(0.045)
Treatment Effect	1.086*	0.229***
	(0.609)	(0.074)
Observations	326	3,486
Effective Obs.	179	2291
Bandwidth	3.064	4.463
Order polynomial	1	1

Robust and clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
Sample is from Fall 2014-2021. Test score has been re-centered at 0. Standard errors have been clustered on the running variable. Results show enrollment by residency among high GPA students at the test score threshold. Robustness check for Table 4.8.

Appendix D. Robustness Checks for High Test Score Group

Table D.1. *Main Results, High Test Score Group Using RDRobust with Optimal Bandwidths*

	(1) Standard	(2) Coverage Error Rate Optimal	(3) Heteroskedasticity-Adjusted
First Stage	0.271**	0.236	0.270**
	(0.137)	(0.147)	(0.136)
Treatment Effect	0.733	0.936	0.733
	(0.468)	(0.682)	(0.456)
Observations	2,973	2,973	2,973
Effective Obs.	530	304	530
Bandwidth	0.250	0.168	0.248
Order polynomial	1	1	1

Robust and heteroskedastic standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Sample is from Fall 2014-2021. GPA is the running variable and has been re-centered at 0. Column 1 is robust standard errors. Column 2 uses optimal coverage error rate option. Column 3 used Eicker-Huber-White standard errors. Results show enrollment among high test score students at the GPA threshold. Robustness check for Table 4.9.

Table D.2. *Main Results, High Test Score Group including Competing Aid Recipients*

	(1)	(2)	(3)	(4)
	0.15	0.20	0.25	0.30
First Stage	0.191**	0.181**	0.247***	0.259***
	(0.092)	(0.084)	(0.076)	(0.071)
Treatment Effect	1.119	1.032	0.727*	0.488
	(0.730)	(0.645)	(0.383)	(0.318)
Centered GPA	-3.405	-3.509**	-2.441***	-1.791***
	(2.386)	(1.775)	(0.855)	(0.620)
Constant	-0.634	-0.578	-0.310	-0.105
	(0.653)	(0.570)	(0.337)	(0.278)
Mean Enrollment	0.32	0.33	0.32	0.31
Bandwidth	0.15	0.20	0.25	0.30
Observations	317	439	638	780

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Sample is from Fall 2014-2021. GPA has been re-centered at 0. Centered GPA is the running variable. Sample includes all students who participated in the competing aid process. Robustness check for Table 4.9.

Table D.3. *Main Results, High Test Score Group using RDRobust: By Sex*

	(1)	(2)
	Female	Male
First Stage	0.311*	0.383***
	(0.186)	(0.130)
Treatment Effect	0.853	0.497
	(0.618)	(0.352)
Constant	-0.469	-0.079
	(0.558)	(0.299)
Observations	227	328

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Sample is from Fall 2014-2021. GPA has been re-centered at 0. Results show enrollment among high test score students by sex at the GPA threshold. Robustness check for Table 4.10.

Appendix E. Descriptive Enrollment Rates Above and Below Test Score Threshold

Table E.1. *Enrollment Rates of High GPA Students by Test Score Bandwidth, 2014-2021*

	Overall	-4	-3	-2	-1	0	1	2	3	4
Student Characteristics										
Sex										
Female	19%	31%	27%	22%	20%	28%	22%	17%	12%	13%
Male	20%	36%	35%	28%	26%	26%	24%	23%	15%	20%
Race/Ethnicity										
White	21%	44%	38%	28%	30%	31%	26%	21%	16%	18%
Asian	13%	33%	14%	33%	4%	25%	16%	20%	8%	8%
Hispanic	11%	5%	7%	6%	9%	18%	15%	8%	0%	17%
Black	5%	0%	0%	0%	0%	9%	0%	17%	0%	0%
Two or More	21%	40%	20%	18%	18%	44%	15%	10%	13%	13%
Not Indicated	15%	36%	25%	25%	17%	0%	31%	18%	15%	7%
In-State Resident	73%	74%	76%	91%	70%	72%	82%	70%	56%	67%
Non-Resident	14%	22%	21%	14%	16%	22%	17%	16%	12%	13%
First Generation Student	27%	27%	60%	31%	22%	39%	27%	30%	8%	20%
Socioeconomic Disadvantage	20%	59%	28%	23%	11%	38%	22%	24%	3%	9%

***Source*:** Author's analyses of institutional graduate school datasets

Notes: Please note, first generation status and socioeconomic disadvantage began to be recorded in 2018 and 2015, respectively. Socioeconomic disadvantage is indicated by administrators following a holistic assessment of their application materials. Percentages reflect enrollment rate for each subgroup. Test score is re-centered at 0.

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