Resources and Information Gaps:
Policies Affecting Academic Trajectories

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DEDICATION

I dedicate this dissertation to my daughter, Elizabeth Alexandra Barbatti. You make every day a joy, especially the long days spent writing this dissertation. Thank you.

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DISSERTATION OVERVIEW

While college enrollment has increased substantially over the past few decades, the total share of Americans with a college degree is essentially unchanged since 1980 and socioeconomic inequalities in college completion have widened over time (Aud et al, 2013; Bailey & Dynarski, 2012; Bound, Lovenheim, & Turner, 2010). These gaps in college success persist even after controlling for students' academic achievement (Belley & Lochner, 2007; Long & Mabel, 2012). One of the most commonly employed policy levers to address income disparities in college enrollment is the provision of financial aid to subsidize certain students' college costs. Rigorous evaluations of several types of financial aid policies suggest that strategy works (Bettinger, 2004; Castleman & Long, 2013; Goldrick-Rab et al, 2012; Scott-Clayton, 2011). Financial aid policies directly and successfully target the resource constraints low-income students and their families face when making decisions about their human capital investments and postsecondary options (Becker, 1964).

But low-income students are also resource-poor in non-monetary ways that affect their likelihood of benefiting from these federal policies. All students struggle with the complexity of various college enrollment and continuation tasks such as refiling the FAFSA or registering for classes that will apply to their major. However, low-income students are especially likely to struggle with these tasks given their lack of access professional assistance or parental familiarity with the college process (Castleman & Page, 2014; Lareau, 2003; Ross, White, Wright, & Knapp, 2013). In addition to gaps in access to informed advisors to navigate complex education investment decisions, low-income students and families often have a limited "mental bandwidth," or ability to focus on different tasks in the face of more pressing day-to-day concerns (Mullainathan & Shafir, 2013; Ross et al., 2013). Information asymmetries by socioeconomic status about the various requirements and steps needed to successfully advance one's education, lack of guidance in navigating those requirements, and the cognitive demands of poverty and daily life combat low-income students' goals of furthering their education.

This dissertation explores students' access to high-quality information and advisors to help them advance through the K-12 education system and into the postsecondary system. Financial aid policies are an example of one type of high-resource, high-intensity intervention to address income gaps in college-going. However, there are several other potential policy interventions that address the non-pecuniary resources students need to navigate the education system, and there are several leverage points earlier in students' education trajectory that merit intervention well before students make the decision about whether and where to apply to college.

Chapter 1 examines the effects of school counselors on students' outcomes, specifically examining the role counselors play in affecting students' discipline, to provide insights into students' access to high-quality advising in the crucial high school years where they make important decisions about how they wish to begin their young adult lives. Several descriptive studies have documented the positive effects of small group counseling and counselor interactions on student outcomes, including the frequency of experiencing discipline outcomes such as suspensions. However, to date, no research has examined whether policies that govern the number of counselors a high school employs affect suspension and expulsion rates, or effects on more serious infractions. In Oklahoma, law dictates that high schools employing more than 450 counselors must employ an additional school counselor, and schools are compliant with that mandate, and I examine the effects of adding a school counselor in response to the policy through a fuzzy regression discontinuity approach. Leveraging two years of data on Oklahoma high schools, I find differential effects by year. In 2013-14, the additional counselor results in reductions in suspension rates for students with disabilities and that students without disability experience increases in in school suspension rates. In 2015-16, I see no effect of counselors on student outcomes. I then explore Oklahoma Department of Education staff data to examine whether insights into counselor characteristics such as experience. Taken within the context of an increasing national focus on reducing disparate applications of school discipline while concurrently keeping schools safe, this suggests that additional counselors may focus on high policy priorities but also engage in low-effort but time-intensive strategies for the majority of students. This mirrors responses to increased staffing in other sectors and points to the need for targeted directives and clear understandings of the unanticipated consequences of school personnel policies.

Chapter 2 examines the common practice of colleges requiring students to submit a "commitment deposit" of $100-500 when they submit an intent-to-enroll form to a postsecondary institution that is then applied to the subsequent semester's tuition and fees. While there are no federal data sources that document the prevalence of these policies, they represent a financial obstacle for many college intending students who have been accepted to an institution. Partially in recognition of this barrier, many institutions provide a waiver for students with financial need. This is particularly important for low-income students who would otherwise have their fall tuition and fees covered by grant aid and scholarships – the outlay in fact represents a non-refundable fee rather than a deposit. I use a regression discontinuity approach to examine the deposit policy at the City University of New York (CUNY) where waiver eligibility is determined by students' expected family contribution (EFC) on the FAFSA to explore whether this waiver has an effect on students' enrollment decisions. I find no effect of waiver eligibility on students' enrollment outcomes; given this null effect, I examine several hypotheses for why the $100 subsidy did not affect enrollment. I find some evidence of policy noncompliance - that nearly a third of students eligible for a waiver still end up submitting their deposit, and several students with higher EFCs find a way not to pay the deposit. To the extent that there is noncompliance, that likely explains part of but not all of the null enrollment findings. I also find no evidence of an enrollment effect by the salience of the waiver requirements or ease of waiver application across institutions, and hypothesize that the timing of the subsidy may come at a juncture in the college-going process where students' demand for education is inelastic to small price shifts.

In addition to providing financial support, states and institutions also have opportunities to invest in targeted support programs and offer additional advising resources to students to mitigate the costs of college enrollment and increase the likelihood students will succeed in the classroom. Programs such as the Accelerated Study in Associate Programs (ASAP) at the City University of New York (CUNY) community colleges combine financial support with institution-level investments in intensive advising and structured pathways that have significant effects on students' persistence and degree attainment. While access to such high quality advising can lead to substantial improvements in students' postsecondary outcomes, many college advisors are overworked and unable to address all students' needs, and advising resources are often particularly limited at the broad access public institutions attended by most students. In Chapter 3 - co-authored with Benjamin Castleman, Zachary Sullivan, and staff at the University of Virginia financial aid and admissions offices - I explore how low-cost text message outreach can achieve some of the goals of providing students with timely information about financial aid deadlines and requirements in the face of limited staff capacity to address all students' needs.

Chapter 3 addresses a very specific financial aid process that students are likely to need assistance navigating. A growing body of research indicates that proactive outreach from high schools and college access organizations about college preparation tasks results in increased college enrollment. However, to date the majority of that research has focused on the Free Application for Federal Student Aid (FAFSA). Comparatively less attention has been paid to the role of colleges and universities in this outreach and the effects of outreach on other financial aid forms. The CSS PROFILE is a less frequently but still widely used financial aid form (particularly at private and highly selective public four-year institutions). Given the smaller number of colleges that require the CSS PROFILE, many students applying to UVA likely have not encountered guidance on when or how to complete the form. In this article, we investigated the effect of sending targeted, semi-personalized text messages to students during the college application process about important financial aid deadlines. The intervention increased timely

CSS PROFILE filing by 3.1-4.3 percentage points, where the estimates and their significance varied depending on the comparison group. Although unable to observe student's financial aid awards in this study, descriptive examinations of previous cohorts of students' aid packages suggest that filing the CSS PROFILE results in a large increase in grant aid offered. We did not observe impacts on student enrollment. These results suggest that colleges and universities have an important role to play in outreach to applicants relating to important financial aid tasks, though the effects on enrollment likely vary across different types of institutions.

Taken together, these chapters point to the range of potential policy interventions to provide students with assistance navigating the education investment decision - from additional school support staff to small financial subsidies to proactive information about important tasks and deadline. However, my results also point to the importance of a clear theory of action between a policy intervention and the outcomes an organization or school system hopes to affect, and that design matters. Chapter 3 has been published in the *Journal of Student and Financial Aid*, volume 47, issue 3. I envision expanding my analysis in Chapter 1 to a broader set of states and schools to improve statistical power and examine variation in counselor effectiveness on student outcomes by different state contexts. Chapter 1 also highlights an area for future research exploring counselor labor markets and how students' access to counselors with different experience levels and training varies. Chapter 2 highlights how little is known about the commitment deposit waiver process or other small financial and administrative hassle factors in the college-going process, and I envision expanding my research to understanding the prevalence of and variation in deposit and waiver policies as well as other college matriculation hurdles across institutions.

CHAPTER 1

The Effect of School Counselors on Student Discipline: Evidence from a Regression Discontinuity

Abstract

Despite the vital role school counselors play in students' development, many schools' counseling offices employ an inadequate number of counselors to serve their student body, with counseling staffing and quality distributed inequitably across communities. The American School Counselor Association (ASCA) recommends schools employ one counselor for every 250 students enrolled, yet the national average student/counselor ratio is about 490:1, ranging from 211:1 in Wyoming to 941:1 in Arizona (ASCA, n.d.). There are also large disparities in whether schools employ even a single counselor. While many policy advocates support increasing school counselor staffing, there is comparatively little empirical evidence that supports this as an effective policy lever to affect students' educational outcomes. Leveraging two years of data on Oklahoma high schools, I find differential effects by year. In 2013-14, the additional counselor results in reductions in suspension rates for students with disabilities and that students without disability experience increases in in school suspension rates. In 2015-16, I see no effect of counselors on student outcomes. I then explore Oklahoma Department of Education staff data to examine whether insights into counselor characteristics such as experience. Taken within the context of an increasing national focus on reducing disparate applications of school discipline while concurrently keeping schools safe, this suggests that additional counselors may focus on high policy priorities but also engage in low-effort but time-intensive strategies for the majority of students.

I. INTRODUCTION

In January 2014, the U.S. Departments of Education and Justice released a joint "Dear Colleague" letter to public elementary and secondary schools addressing the state of school discipline, particularly disparities in disciplinary incidents by students' race and disability status (OCR, 2014). The letter drew on a substantial body of research associating "exclusionary discipline" measures (e.g., suspensions, expulsions, and referrals to law enforcement) with negative student outcomes, reduced educational attainment, and increased likelihood of experiencing substance abuse or arrest. Disparities highlighted in the letter and departments' subsequent work include documenting that students with disabilities were twice as likely to experience an out of school suspension relative to students without disabilities, and between a fifth and a quarter of students of color with a disability experienced an out-of-school suspension (CRDC, 2014). Among the recommendations contained in this "Dear Colleague" letter was that schools "ensure that there are sufficient school-based counselors" to work with students not only to transition back to the classroom after an exclusionary disciplinary incident, but also to prevent future discipline concerns. Recent federal accountability policies, including the Every Student Succeeds Act (ESSA) have subsequently formalized the role of the school counselor in abating school violence and improving school climate.

Students, teachers, and principals often turn to their counselors to address students' myriad non-instructional needs, including enlisting their support to manage student behavior and discipline and to build a safe and welcoming school culture. A third of high school counselors in 2002 reported spending at least 20 percent of their time on attendance, discipline, and personal problems with another third spending between 10-20 percent of the time on those topics (NCES, 2002). Despite the important role that counselors play in student discipline, in many schools counselors manage substantial student caseloads that make it challenging to address all students' needs; counselors are moreover often tapped to engage in administrative tasks unrelated to their counseling mission.[1] The American School Counselor Association (ASCA) recommends one counselor for every 250 students, yet nationally the average student/counselor ratio is about 490:1 (ASCA, n.d.). The national average masks substantial state variability – with ratios ranging from 211:1 in Wyoming to 941:1 in Arizona (ASCA, n.d.). There are also large disparities in whether schools employ even a single counselor. A fifth of public schools do not employ a school counselor, and both black students and low-income students are more likely to attend a school that employs a law enforcement officer but not a school counselor (CLASP, 2015; USDOE, 2016).

One potential policy solution to increase counselors' capacity to invest in positive youth development, behavior management, and school discipline is to fund or mandate hiring additional school counselors. School counseling advocates point to a host of descriptive research and surveys suggesting that schools with more school counselors experience improved student outcomes (Lapan, Gysbers, Bragg, & Pierce, 2012; Lapan, Whitcomb, & Aleman, 2012). In particular, research has found that the more frequently secondary school students met with their counselor, the safer they felt in school and the more likely they were to say there was a trusted adult in their school they could talk to (Lapan, Wells, Petersen, & McCann, 2014). However, these studies often suffer from potential selection biases. For example, a school principal willing to invest resources in an additional school counselor might concurrently implement broader reforms aimed at promoting a stronger school culture and positive student behavior, making it difficult to disentangle the unique contribution of the added school counselor to student disciplinary outcomes.

These selection biases notwithstanding, additional counselors could improve student behavior and discipline outcomes through several channels. Despite the negative outcomes

[1] While the American School Counselor Association (ASCA) advocates for counselors to spend 80% of their time on direct service, many counselors report spending substantial time on administrative tasks such as lunch duty or substitute teaching (ASCA, n.d.; NCES, 2002; Lapan & Harrington, 2009; Lieberman, 2004; Perera-Diltz & Mason, 2008; Scarborough & Culbreth, 2008)

associated with students experiencing a suspension, it is a widely used disciplinary tool. Suspensions have increased in prevalence over the past few decades, with the overall suspension rate increasing from about 15 percent of students in 1993 to almost 20 percent of students in 2012 (Losen & Gillespie, 2012). In 2011-12 3.5 million students experienced an in school suspension (ISS) and 3.45 million students experienced an out of school suspension (OSS) (OCR, 2014). In recent years in an effort to stem suspension rates, schools have implemented restorative justice practices and whole-school behavioral interventions, with school counselors frequently tapped to oversee these programs (Kline, 2016). Whole-school programs often led by counselors, such as Positive Behavioral Interventions and Supports (PBIS), that focus on preventative behavioral modifications and school climate, result in higher levels of student concenration, prosocial behavior, and social-emotional functioning (Bradshaw, Waasdorp, & Leaf, 2012; Goodman-Scott, 2014). In addition to descriptive studies, a few experiments also demonstrate that small-group counselor-led programs can positively affect student behavior and school engagement (Midget et al., 2017; Sinclair et al., 1998; 2005).

If a school hires an additional counselor, the counseling office would conceivably have more capacity to put towards implementing these evidence-based strategies. On the other hand, an additional school counselor ends up translating into a very small increase in available annual counseling minutes per student in the school – for example, adding an additional counselor to a school with 450 students means that each student has access to an additional three hours of counseling over the course of the year.[2] Given the many roles counselors hold, it may be unreasonable to expect improvements on a wide range of student outcomes and instead more plausible that counselors focus their additional time on specific projects, policy goals, or groups of students. Simultaneously, an increase in counseling staff represents an increase in surveillance

[2] Assuming counselors work eight hours a day for 180 school days a year

in a school, and lessons from other contexts suggest that increased monitoring is not always associated with a reduction in undesired behavior.

I provide additional evidence on the impact of school counselors on discipline outcomes by examining school counselor staffing in high schools in the state of Oklahoma, using a "Maimonides Rule"-like regression discontinuity design to investigate the impact of schools having an extra counselor by virtue of schoolwide enrollment being just above an arbitrary threshold (Angrist and Lavy, 1999).[3] In 1991, the Oklahoma State Board of Education mandated in their public school Standards for Accreditation that high schools must maintain "at least one certified school counselor to each 450 students" (§210:35-7-43). According to this policy, perfectly compliant schools would have one counselor in schools with 450 or fewer students, and then add a second counselor right at the enrollment threshold. Oklahoma has high levels of compliance with the state counselor staffing policy, with schools just above the 450-student threshold employing an additional 0.68 FTE counselor, making it an optimal state for examining the causal relationship between counselor staffing and student outcomes.

Oklahoma also provides a unique setting to examine the effectiveness of additional counseling on school discipline outcomes, since several of its districts have high rates of exclusionary discipline.[4] Oklahoma City Public Schools have one of the top ten highest district high school suspension rates, with high school suspension rates increasing by 20.5 percentage points between 2009-10 and 2011-12 (Losen et al, 2015). Oklahoma City Public Schools also had the highest rate of black male suspensions and the highest expulsion rate in the nation in 2012 (KidsCount, 2016). Overall, the state of Oklahoma has the highest rate of expulsions for special

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[3] The authors of this study found that in Israel, there is a strong mandate from the country's education ministry to limit class size to 40, inspired by the interpretation of the Talmud by Maimonides, a Rabbinic scholar in the twelfth century. The researchers examine how school compliance with the rule (e.g., creating additional classrooms once enrollment exceeds a certain threshold) affects elementary students' academic performance.

[4] A little over 20 percent of Oklahoma youth reside in either Oklahoma City or Tulsa (Oklahoma Institute for Child Advocacy, 2016).

education students and 17 schools in Tulsa expelled more than half of their special education students in 2011-12 (Robson, 2015). In light of these high levels of exclusionary discipline and disparate application, the U.S. Office of the Civil Rights has placed Oklahoma City Public Schools under investigation.

Oklahoma has also experienced a decline in statewide counselor availability, as measured by the counselor-student ratio and an uptick in emergency certifications for counselors. Figure 1 plots the number of school counselors in the state available per 450 students (the state policy), and shows that the counselor/student ratio declined from about 1.17 counselors per 450 students in 2008-09 (slightly more than required by the state) to about one counselor per 450 students in 2016-17 (the most recent year available). Over the same time, there has also been a dramatic uptick in the issuance of emergency certifications for counselors. Oklahoma issued two emergency certificates to counselors in 2008-09, but by the 2013-14 academic year that number jumped to 13 counselors and increased to 58 emergency counseling certificates by 2016-17.[5] The counselor workforce in Oklahoma over the past decade has increasingly included fewer traditionally certified counselors, which may result in a change in counselor activities and the quality and types of support services students receive.

My analysis employs a two-stage least squares (2SLS) "fuzzy" regression discontinuity approach to examine counselor staffing in Oklahoma high schools above and below the state enrollment threshold (450 students) for a school needing to hire an additional counselor. I use schools' placement on either side of the 450-student enrollment level as an instrument for whether the school employs an additional counselor to estimate the causal effect of counseling on student outcomes. For the purposes of this survey, a counselor is defined as a school employee whose duties include "counseling with students and parents, consulting with other staff members

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[5] Note that while my analysis focuses on Oklahoma high schools, the Oklahoma state data on emergency certifications are not disaggregated by school level (e.g., elementary, high) and so figure 1 represents overall counselor statistics for the state.

on learning problems, evaluating student abilities, assisting students in making education and career choices, assisting students in personal and social development, providing referral assistance, and/or working with other staff members in planning and conducting guidance programs for students". This category does not include psychologists or social workers.

Two other quasi-experimental studies have used a similar identification to investigate the impact of counselor staffing levels various outcomes.[6] Reback (2010) used a seven-year panel of state data to examine the effect of Alabama elementary schools receiving funds intended for the hiring of an additional half-time school counselor. The analysis used a "fuzzy" regression discontinuity approach to compare schools just above and below student enrollment thresholds that determine eligibility to receive a state subsidy to support an additional 0.5 full time equivalent (FTE) school counselor. Reback (2010) finds that an increase in counselor funding subsidy reduced elementary school suspensions and weapon-related incidents, but did not affect attendance, standardized test scores, or expulsion rates. However, the treatment definition for this analysis was school's subsidy eligibility and the analysis did not have reliable data available on actual school counselor staffing by school to verify the "first stage" compliance with whether schools actually employed additional staffers. Additionally, due to the relative infrequency of elementary school discipline outcomes, Reback (2010) examines broad binary outcomes such as whether a school had any student suspended. Hurwitz and Howell (2014) used the national Schools and Staffing Survey (SASS) to examine multiple states' laws around counselor staffing and the effect of staffing on high school students' college enrollment rates. Using a similar "fuzzy" regression discontinuity approach, they found that adding an additional school counselor to a school increased the proportion of students enrolling in a four-year college after high school, but the authors do not investigate non-college enrollment outcomes.

[6] Another quasi-experimental study used fixed effects to examine variance in graduate student field placements at Florida elementary schools to identify the effects of a temporary increase in counselor staffing on student outcomes; the researchers found small, positive effects on student discipline reduction and male students' academic achievement (Carrell & Carrell, 2006; Carrell & Hoekstra, 2014).

I build on this prior work in several important ways. First, I extend Reback's (2010) analysis of school counselors' effects on student discipline incidents by examining this relationship in the high school context. While mitigating behavioral problems at an earlier age has important implications for students' long-term school engagement and success (Kupchick & Catlaw, 2013), disciplinary incidents are far more common in secondary schools than elementary schools – in the 2011-12 academic year, 2.6 percent of elementary school students experienced a suspension compared with 10 percent of secondary school students (Losen et al., 2015). In addition to studying counselor effects in high schools, I am able to extend Reback's (2010) work and document first-stage compliance with the Oklahoma policy. I use a rich dataset in my analysis, the 2013-14 and 2015-16 Civil Rights Data Collections (CRDC), which include full-time equivalency (FTE) counts of counselors to examine the effect of actual counselor staffing levels as opposed to a counselor subsidy. The CRDC also includes data for every public school in the state in the 20113-14 and 2015-16 administrations, and thus provides a sufficiently large within-state sample to examine heterogeneity in counselor effectiveness by school type. Finally, the CRDC disaggregates student disciplinary outcomes across student characteristics (e.g., disability, race, and gender), which enables a more nuanced understanding of not only whether school counselors affect disciplinary outcomes, but for whom.

To preview my results, I find that Oklahoma schools are on average highly compliant with the state policy in both the 2013-14 and 2015-16 academic years, employing an additional 0.68 FTE counselor upon crossing the enrollment threshold. This response is larger at schools with a higher proportion of minority students. I then examine the effects of increased counselor staffing on overall suspensions, and then separately for in school and out of school suspensions as well as examining those outcomes by students' disability status. I find no effect of school counselors on student discipline outcomes when pooling the 2013-14 and 2015-16 CRDC administrations together. Given year-to-year variations in state and federal policy attention and changes over time in school resources, I estimate the effect of an additional school counselor on student discipline outcomes separately by year. While there is not a significant effect of an additional school counselor on overall suspension measures in 2013-14, I do observe a significant increase in overall suspensions for students without a disability, driven by significant increases in in school suspensions (ISS). Turning to students with a disability, the point estimate for overall suspension rates is large and negative, though not statistically significant; examining by suspension type, I observe a significant decrease in out of school rates for students with a disability in schools with an additional counselor. However, results are inconsistent across years, with no significant effects in a positive or negative direction in the 2015-16 data. These varying effects year-to-year merit caution in interpretation.

Despite the fact that the first stage policy binds in both the 2013-14 and 2015-16 CRDC administrations, I find varying effects of a school counselor across years. I cannot rule out the possibility that the 2013-14 observed effects occur by chance and are a result of noisy estimates rather than the true effect of a school counselor on student outcomes. To the extent that the effects reflect the true effect of an additional school counselor on student outcomes, the findings may suggest that the policy focus on student discipline in the 2013-14 academic year may have focused additional counselors' attention on the subject, whereas in 2015-16 their attention was diffused across several other outcomes. Another plausible explanation is that over a longer time, schools with fewer counselors were able to "catch up" with better-resourced schools and targeted discipline outcomes in a similar manner. Finally, there could be year-to-year differences in the availability of other school resources that explain different effects across years. I examine year-to-year variation in average incident rates and leverage staffing and budgetary data from the Oklahoma Department of Education to explore the extent how plausible these hypotheses are. This preliminary analysis suggests that above- and below-threshold schools shifted toward the mean in incident rates between 2013-14 and 2015-16, and I find little difference in counselor experience or counseling office composition between the two years that might explain the differential effects.

In section II, I provide a conceptual model of how we might expect school counselors to affect student disciplinary outcomes, and particularly how increased staffing might enable a school counseling office to address these outcomes. In section III, I provide details on the Oklahoma context and data used in this analysis. Section IV describes the empirical strategy and Section V discusses the validity of the regression discontinuity design. In section VI, I share results for the first stage and 2SLS analyses, discussing heterogeneity in counselor effects by student and school characteristics as well as various robustness checks to examine consistency across models. In Section VII, I explore student enrollment and counselor-staffing shifts observed between the two years in my analysis that provide context to interpreting the year-to-year variance in counselor effects, and in section VII, I discuss policy implications of these findings and directions for further research.

II. CONCEPTUAL FRAMEWORK

To understand how the Oklahoma policy of hiring additional secondary school counselors might affect students' discipline outcomes, I first present a review of the factors influencing school discipline outcomes and the existing literature on how school counselors contribute to student discipline. I then provide a discussion of counselor time use and a conceptual framework for how increasing the number of counselors might affect student discipline in a positive or negative direction.

School Discipline and School Counselors

Evidence suggests that exclusionary disciplinary actions such as suspensions or expulsions do not benefit either the individual students exhibiting behavioral problems or their classmates. While studies find that disruptive students have a negative effect on their peers' learning, behavior, and long-term earnings (Carrell, Hoekstra, & Kuka, 2016; Deming, 2011; Figlio, 2007), there is little evidence that suspensions mitigate these negative effects (Lacoe & Steinberg, 2018). Large-scale descriptive studies across different state contexts also suggest a negative correlation between a school's rate of suspensions and students' academic performance (Noltemeyer, Ward, & McLoughlin, 2015; Rausch & Skiba, 2005; Skiba, et al 2014). Descriptive and quasi-experimental studies moreover suggest a negative to null effect of suspension on academic outcomes for the student who has been disciplined (Cholewa, Hull, Babcock, & Smith, 2018; Chu & Ready, 2018; Lacoe & Steinberg, 2018). Because of this evidence, the American Psychological Association recommends less frequent applications of exclusionary discipline policies and increases in preventative counseling available at schools (APA, 2008).

Decades of research has moreover documented persistent disparities in the application of school discipline. Students with disabilities are more likely to experience an out of school suspension (CRDC, 2016). Students with disabilities are also disproportionally represented in the share of students experiencing more severe discipline – arrest, law referrals, or physical restraint (CRDC, 2016). These incidents resulting in removing students from the classroom are especially troubling given the protections under the Individuals with Disabilities Education Act (IDEA), which states students should not lose access to their school services for a significant period.[7] Black students are also more likely to receive exclusionary discipline for minor infractions and on average receive longer suspensions then white peers even for the same type of infraction; this trend occurs both within and across schools (Anderson & Ritter, 2017; Anderson & Ritter, 2018; Barrett, McEachin, Mills, & Valant, 2017; Skiba et al., 2014; Losen et al., 2015).

The Mechanisms through which Increased Staffing Improves Counselor Practice

While I am unable to observe counselor time use in the CRDC and therefore cannot uncover the mechanisms behind how counselor staffing affects student outcomes in this study, below, I outline potential mechanisms and lessons from how increased staffing in other contexts inform how we might hypothesize the addition of a school counselor could increase or decrease

[7] This is referred to as a "change in placement" and occurs when a student with a documented individual education plan (IEP) under the IDEA provisions loses access to her support services for more than 10 cumulative (but not necessarily consecutive) days.

student discipline. First, increasing counselor staffing likely results in each counselor managing a smaller caseload of students and being able to spend more time with each student. In the field of education, studies of instructional staffing have generally found that when teachers manage a smaller class, they can spend more time on task and their students experience improved academic and social-emotional outcomes.[8] To the extent that an additional counselor enables each counselor to spend more time on a student's case, the Oklahoma staffing policy might reduce bias in counselor recommendations and result in improved student outcomes (Auwarter & Arguete, 2008; Francis, Dimmitt, de Oliveira, 2018; Welsch & Winden, 2018).[9]

An increase in counselor staffing could also affect counselor performance through peer effects. Counselors could be more productive simply by having a colleague physically nearby "watching" their output – in the medical field, for example, healthcare workers are far more likely to engage in proper hygiene when being watched (Hagel et al., 2015).[10] Counselors could also learn from a colleague's different perspective and skill set; the teacher effectiveness literature suggests that the arrival of a higher-performing teacher to a new school improves other teachers' students' test score growth (Jackson & Bruegmann, 2009). Another hypothesis of increased staffing is that counselors could specialize in their duties according to their comparative advantage (Bastain & Fortner, 2018). For example, one counselor could focus on behavioral management and another could focus on college and career planning for all students. This specialization could result in counselors better performing the duties associated with their specialization (e.g., a counselor focused on just college and career planning may be better able to advise students through financial aid form submission), but potentially at the expense of

[8] See, among others, Angrist & Lavy, 1999; Hanushek, 1999; Krueger, 1999; Krueger & Whitmore, 2001; Krueger & Whitmore, 2001; Chetty, et al., 2011

[9] Bias in school professional interactions with students is certainly not unique to counselors – see Gershenson, Holt, & Papageorge, 2016; Lavy & Sand, 2015; Riegle-Crumb & Humphries, 2012; among others

[10] See Burnham & Hare, 2007; Chib, Adachi & O'Doherty, 2018; and Ernest-Jones, Nettle, & Bateson, 2010 for psychological experiments also documenting the effect of surveillance on productivity.

developing a personal relationship with each student when only focusing on one aspect of the student's needs as opposed to a holistic approach.

The extent to which counselors are adequately trained for their position likely affects their ability to execute their central job functions. While increasing school staffing is generally hypothesized to have a positive effect on students, the class size reduction efforts in California in the 1990s suggest that the qualifications of additional staff play an important role in the relationship between staffing and student outcomes. An analysis of the state's class size reduction policy found that as a result of the policy, average teacher qualifications dropped and there were more non-certified teachers in the workforce, with those teachers disproportionally teaching at schools with a high percent of low-income and English language learner students (Bohrnstedt & Stecher, 2002). Generally there does not appear to be a large difference in teacher effectiveness by certification, and that those differences tend to fade over time (Boyd, Grossman, Lankford, Loeb, & Wyckoff, 2006; Goldhaber & Brewer, 2000), but the California case study calls into question what the limit is on the effectiveness of non-certified school staff when the total pool of staff members is expanded. As noted in figure 1, Oklahoma has seen an increase in non-certified school counselors in their workforce in recent years, and the extent to which counselors with less experience and less training are the solution to the counselor staffing policy, students may not benefit fully from the increased counseling availability.

In the context of schools and counselors, a school counselor could operate as a second set of eyes and may observe more student misbehavior that warrants disciplinary actions, thus resulting in an increase in discipline rates that have more to do with observing more incidents as opposed to more incidents actually occurring. Within schools, the research on school resource officers (SROs) provide additional insights into a potential negative relationship between increased non-instructional staffing and student outcomes.[11] The most rigorous evaluations that leverage quasi-

[11] Counselors and SROs have very different job descriptions – counselors' stated purpose is focused on advising students on mental health, career and college options, and academic planning, while SROs have an

random placement of SROs in schools find that an increase in SROs results in increased school discipline rates particularly for Hispanic and Black students and a reduction in high school graduation and college enrollment rates (Weisburst, 2018).

Lessons from other professions also suggest that increased staffing and increased monitoring are not universally associated with improved outcomes for all parties. For instance, an increase in police officer deployment to certain high crime areas is associated with increases in investigative stops (MacDonald, Fagan, & Geller, 2016).[12] In another study, researchers found that the more police officers assigned to a neighborhood, the worse those students perform in school, suggesting a potential psychological spillover effect of increased surveillance (Legewie & Fagan, 2019). In the medical profession, increases in nursing home staff do not consistently translate to improved outcomes for patients, with some studies finding that an increase in nursing home staff is associated with an increase in more labor-intensive but generally discouraged practices, such as physically restraining patients (Bowblis & Lucas, 2012; Bowblis & Ghattas, 2016). Other studies find that when certain nursing home standards are increased, staff generally respond by substituting time to the targeted standards and away from other regulations (Bowblis & Lucas, 2012). These studies suggest that a school might direct their additional school counselors to focus on high-priority policy areas at the opportunity cost of other important—and potentially more beneficial—student services, and that certain labor-intensive disciplinary practices (such as in-school suspension which require in-person supervision) might be less costly and potentially more likely to be implemented with a larger school counseling staff.

III. BACKGROUND AND DATA

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explicit school safety mandate (Finn & McDevitt, 2005). However, similar to counselors, SROs have wide-ranging responsibilities that vary across districts; in one district, SROs reported spending 25 percent of their time teaching safety lessons in classrooms and spend 38 percent of their time on mentoring and advising, while in another SROs reported spending 60-65 percent of their time on law enforcement (Finn & McDevitt, 2005)

[12] Lessons from police staffing are generally mixed, with some studies finding an increase in SWAT deployments associated with increased perceptions of crime (Mummolo, 2018), and others finding larger police forces associated with crime reductions (Corman & Mocan, 2000; Levitt, 1997).

A. *Data and Sample*

This analysis uses data for Oklahoma high schools from the Civil Rights Data Collection (CRDC) from 2013-14 and 2015-16 academic years. The U.S. Department of Education Office of Civil Rights administers the CRDC every other school year; collected since 1968, the CRDC has been a census of public schools since the 2011-12 administration. The 2013-14 and 2015-16 administrations collected data from schools on student demographics, instructional and non-instructional staffing, and various student academic and disciplinary outcomes. I removed several alternative school types from the sample that are likely to have very small enrollments or be exempt from state counselor policies or from providing certain data elements to the CRDC (e.g., juvenile justice centers, special education schools, charter schools, and magnet schools). I then supplemented the CRDC with the Common Core of Data (CCD) to describe the sample's school locale, subsidized lunch participation rates, and Title I status. This resulted in a potential sample of 892 Oklahoma high schools – 442 schools in 2013-14 and 450 schools in 2015-16. The Oklahoma policy mandates that when a high school's enrollment exceeds multiples of 450 (e.g., 450, 900), the school should hire an additional school counselor. I focus my analysis on the margin for a school hiring its second school counselor – schools above and below the 450-student enrollment threshold in Oklahoma, but enrolling fewer than 900 students (the next relevant threshold for increasing staffing).[13]

In Table 1, I report average school characteristics for all schools retained after removing special school types and separately for my regression discontinuity sample focusing around the second counselor margin; this sample restriction includes about 90 percent of the schools in the overall sample. Mechanically, schools in the regression discontinuity sample have smaller

[13] This restriction avoids heterogeneity in treatment definition – going from one to two counselors represents a 100 percent increase in staffing, while the addition of a third counselor represents only a 50 percent increase in staffing. Analyses including additional thresholds available upon request. I also run my analyses examining the effects at higher counselor margins with margin fixed effects, although there are few schools around those additional margins (for example, across the two years there are 55 schools on either side of the margin for a third counselor and 28 on either side of the margin for a fourth counselor).

enrollments on average due to this restriction. A high proportion of Oklahoma high schools in the sample are eligible for Title I funds – about 90 percent in 2013-14 and around 82 percent in 2015-16. Subsidized lunch participation is high, between 60-70 percent across years in regression discontinuity sample. The majority of Oklahoma high schools are in a rural area, and as one might expect with the focus on smaller schools, there is a higher proportion of rural schools in the regression discontinuity sample than overall. Those smaller schools also have fewer teachers and counselors on average and are less likely to employ a security officer or, for the 2015-16 sample, a school psychologist or social worker.[14] Demographically, however, the schools in the regression discontinuity and full sample are similar. Notably, Oklahoma has a high proportion of American Indian/Native students enrolled than the U.S. as a whole; American Indian/Native students comprise about 22-23 percent of the school population in Oklahoma compared to around one percent nationally (NCES, 2019).

This analysis primarily focuses on the effects of additional school counselors on student discipline outcomes, but also explores a host of other student outcomes, including course and test taking (such as the percent of students taking the ACT/SAT), and school engagement (such as chronic absenteeism). In Table 2, I show average rates of these outcomes each year for schools in the regression discontinuity sample above and below the 450-student enrollment threshold for hiring an additional counselor. For example, in 2013-14, 24 percent of students took the SAT/ACT in schools below the counselor threshold and 26 percent of students took the SAT/ACT in schools just above the threshold. On the CRDC school form, schools report the count of students experiencing various disciplinary outcomes disaggregated by gender, disability, and duration/frequency of discipline – for example, the count of male students with a documented

[14] The staffing question for psychologists and social workers was not asked in the 2013-14 administration.

disability who experienced only one out of school suspension.[15][16] I combine categories of disciplinary action that are noted as mutually exclusive on the survey form (e.g., I combined "students…who received an expulsion with educational services" and "students…who received an expulsion without educational services" into a single measure of expulsions at that school), and create aggregate counts of disciplinary incidents across student groups. While I also report overall suspension rates (combining ISS and OSS) as an outcome in my main analysis, it is important to note that those constructed categories are not mutually exclusive. Table 2 also illustrates the distribution of schools by enrollment, and the fact that the majority of schools in the sample fall in the untreated group below the counselor-hiring threshold. Finally, Table 2 illustrates the difference rates of discipline by student disability – for example, the average ISS rate for students without a disability in schools below the threshold in 2013-14 is 9 percent compared with 11 percent of students with a disability. About 8 percent of students without a disability experience OSS in below-threshold schools in 2013-14 compared with 12 percent of students with a disability.

IV. EMPIRICAL STRATEGY

A. Fuzzy Regression Discontinuity Approach

My analysis focuses on the discontinuity in counselor staffing around Oklahoma's mandated enrollment thresholds and the effect of increased staffing on student outcomes. I employ a "fuzzy" regression discontinuity analysis, or two-stage least squares model, that accounts for non-perfect compliance with state policy. In Oklahoma, which mandates no more than 450 students per school counselor, perfect compliance would mean schools with enrollments

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[15] These discipline measures are the percent of students experiencing an incident; the CRDC does not include measures of discipline intensity (e.g., length of a suspension) or frequency within student (e.g., whether student was suspended once or multiple times throughout the school year).

[16] I examine additional CRDC supplemental data to explore types of disability represented; the most common disability designation is a learning disability, followed by "other," intellectual disability/developmental delay, and emotional disturbance.

less than or equal to 450 students would have one school counselor ($x_i < c_1$), while a school with 451 students would have two counselors ($c_1 \leq x_i < c_2$). The relationship between counselor count and enrollment in a school would have a slope of zero on either side of the threshold, and a discontinuity equal to one.

Since schools are not perfectly compliant, school enrollments instead affect the probability of increased staffing (*D_i*) based on a school's enrollment (x_i), as a function of g(x), where functions g_1 and g_0 can vary at cutpoint c_1 (illustrating here the simple case around a single cutpoint):

$$\Pr(D_i = 1 | \, x_i \,) = \begin{cases} g_1\,(x_i) & if\; x_i \geq c_1 \\ g_0(x_i) & if\; x_i \leq c_1 \end{cases}$$

The first stage estimation process examines the extent to which a given school, just above the enrollment cutpoint for hiring an additional school counselor in a certain year, in fact increases staffing. I examine the first stage compliance with the counselor staffing policies through the form:

$$Staffing_{it} = \pi_0 + \pi_1(Distance_{it}) + \pi_2(Above_{it}) + \pi_3(Above_{it} * Distance_i)t + \delta_t + \mu_{it} \qquad (1)$$

which regresses school counselor staffing (*$Staffing_{it}$*) on the distance between a given school's total enrollment and the enrollment threshold (calculated as $x_{it} – x_0$), an indicator of whether enrollment is above or below the threshold, and the interaction of the above indicator with enrollment distance to allow for g_1 and g_0 to vary on either side of the enrollment threshold. I include a year fixed effect, δ_t.

The reduced form model is similar to the first-stage, replacing a measure of staffing for student outcomes *Y_{it}*:

$$Y_{it} = \gamma_0 + \gamma_1(Distance_{it}) + \gamma_2(Above_{it}) + \gamma_3(Above_{it} * Distance_{it}) + \delta_t + v_{it} \qquad (2)$$

To capture the causal effect of counselor staffing on student outcomes, I run a 2SLS instrumental variable model, where counselor staffing is instrumented by a school having enrollment above the threshold as estimated in the first stage equation (1). The instrumental variables estimates are generated by the form:

$$Y_{it} = \beta_0 + \beta_1(Staffing_{it}) + \beta_2(Distance_{it}) + \beta_3(Above_{it} * Distance_{it}) + \delta_t + \varepsilon_{it} \qquad (3)$$

The coefficient of interest, B_1 represents the causal estimate of increasing school counselor staffing on the outcomes described above in a given year. As recommended by Lee and Card (2008), I cluster standard errors by enrollment distance from the threshold.[17]

B. Parametric vs. Non-Parametric Estimation

As with all empirical analyses, it is essential to use the correct functional form for estimating treatment effects in a regression discontinuity. Researchers typically use two strategies to determine the correct functional form in a regression discontinuity – a parametric approach, which uses all observations and includes higher-level polynomials that improve upon the linear model and best fit the data, and a nonparametric approach, which uses observations close to the cutpoint under the assumption of a linear relationship (Bloom, 2012). The trade-off between the parametric and non-parametric estimation strategies for model specification represents a fundamental trade-off between bias and precision (Bloom, 2012). Particularly given the small

[17] Standard errors are robust to clustering decisions. Results without clustering and with other clustering choices.

starting sample size in this study, a local linear approach significantly reduces the analytic power to detect effects, especially given how observation-greedy regression discontinuity designs are relative to other empirical methods (Deke & Dragoset, 2012). Ideally, the direction and magnitude of regression discontinuity estimates should be robust to the parametric or non-parametric specifications (and across various bandwidths in the non-parametric approach), even if the smaller sample sizes in a nonparametric local linear approach results in smaller standard errors. I run my models with a linear interaction, quadratic, and quadratic interaction functional form.

I also run my analysis using two forms of bandwidth restrictions. First, I employ a policy-motivated bandwidth. I examine results for schools with enrollments above 225 and below 675 (+/-225). This accounts for Oklahoma policies that provide for partial counselor staffing in schools with enrollments below 225 and for the possibility that schools with enrollments greater than 675 may hire another counselor as they approach the 900-student enrollment threshold for a third counselor. This bandwidth restriction results in a sample size of 259 schools, or about a third of the regression discontinuity sample summarized in Tables 1 and 2. I also use a data-driven approach to bandwidth selection, calculating mean-squared error (MSE) optimal bandwidths using the *rdrobust* package in Stata according to the recommendations of Calonico, Cattaneo, & Titiunik (2014). Appendix Table A1 illustrates for a set of outcomes how much variance there is in outcome-specific bandwidths and the subsequent sample sizes. Generally, outcome-specific bandwidths are similar, ranging between 94-172. For example, the optimal bandwidth for the percent of students with a disability experiencing OSS is +/-102 students resulting in a sample size of 97 schools while the optimal bandwidth for the percent of students without a disability experiencing OSS is +/-111 students, resulting in a sample size of 106 schools.[18] The most

[18] These examples report on bandwidths calculated using a triangular kernel, which weights observations closer to the cutoff more in the estimation; I also report in appendix table A1 bandwidths calculating using a uniform kernel/weighting procedure.

exhaustive method of data-driven bandwidth selection would be to use the individually calculated bandwidths for estimating each outcome (Cattaneao, Idrobo, and Titiunik, 2018); however, I use a common bandwidth of +/-130 (the average bandwidth across outcomes) for simplicity of reporting and constant sample sizes across bandwidth-restricted models, which results in a sample of 129 schools.

V. VALIDITY

If schools, knowing the cutoff for counselor eligibility, adjust their reported enrollments to land on one side of the cutoff, that would violate the LATE independence assumption and bias findings. I conduct a McCrary density test of enrollment counts around the threshold to detect bunching, and separately examine whether school-level covariates differ on either side of the cutoff to investigate whether there appears to be strategic manipulation around the RD threshold and in turn, whether the independence/continuity assumptions hold.

In figure A1, I present the McCrary density plot, in which I find no evidence of enrollment manipulation around the counselor-staffing threshold. In Appendix Table 2, I present results from a covariate balance test using the full bandwidth of schools, using each school characteristic as the outcome of interest in the linear reduced form equation (2) for the pooled sample and each year as well as running quadratic and quadratic interactions for the relationship. There is some imbalance on student race using the quadratic interaction model, but no evidence of discontinuity using a linear interaction approach in the pooled or by-year samples. The Akaike Information Criteria (AICc), adjusted for small sample sizes further suggests for both percent black and percent American Indian/Native students that the linear interaction model is the best fit of the data among the three functional forms tested (Akaike, 1973; Hurvich & Tsai, 1989).[19][20]

VI. RESULTS

A. First Stage

In Figure 2, I present visual evidence that compliance with the state counselor staffing policy is high. There are two ways to conceptualize the change in treatment at the threshold – one is the change in the number of counselors in the school while the other is the ratio of available counselors to a group of students; for this analysis, I compute a ratio of FTE counseling hours available per 450 students to reflect Oklahoma policy. A third option, most commonly used in statewide analysis, is to compute a student-counselor ratio, which divides the number of students by the number of available counselors in a school. In a school-by-school analysis, this approach mechanically drops observations with zero counselors employed, as a number divided by zero is undefined. I do not use this student-counselor treatment definition in my 2SLS models; however, I do compute a student-counselor measure to describe the first-stage discontinuity in counselor caseloads at the policy threshold for the subsample of schools that employ at least one counselor.[21] By either the counselor-count or the counselor-student ratio measure, there is a jump in counselor availability at the policy threshold. I illustrate in Figure 2 the relationship between school enrollment and each measure within the policy bandwidth ($|s_i|225$), with schools just above the policy threshold employing 0.55 additional counselors, or an additional 0.66 FTE counseling hours per 450 students.

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[19] The equation for calculating the AICc that corrects for small sample sizes is $AICc = 2K - 2\log(\mathcal{L}(\hat{\theta})|\gamma) + \frac{2K(K+1)}{n-K-1}$. As AICc converges to AIC with larger n, it is prudent to always employ AICc unless one has a sample size one is confident is sufficiently large (Burnham & Anderson, 2002).

[20] The AIC difference between the linear interaction and quadratic interaction is 10 for percent American Indian/Native and 8 for percent black; Burnham & Anderson (2004) suggest there is low support for AIC differences between 4-7 and "essentially no support" for models with a greater AIC difference.

[21] There are 20 schools in the overall regression discontinuity sample with zero counselors and one school within the policy bandwidth that employs zero counselors.

In Table 3, I run the first-stage estimation for the full set of observations using a linear and quadratic model to predict the effect of crossing the threshold on counselor count, counselor-student ratio, and student-counselor ratio. I also run linear estimations within the policy ($|s_i|225$) and MSE-optimal ($|s_i|130$) bandwidths and run all models for 2013-14 and 2015-16 pooled and separately. Across models, the main conclusion holds – in Oklahoma, high schools respond to the state policy mandate by hiring a school counselor when their enrollment exceeds 450 students. Using the counselor count measure and the full range of observations, Oklahoma schools just above the policy threshold hire an additional 0.60-0.75 FTE counselor across years; the magnitude of that estimate is smaller, but still positive and statistically significant for the quadratic model using the full sample and for the policy bandwidth. The first stage is not significant for the smaller bandwidth of $|s_i|130$ with a sample size of 129 schools in the pooled sample but is still positive. The local linear specifications for the counselor-student ratio produces very similar first stage estimates to the counselor count measure, showing a 0.58-0.71 increase in counseling available per 450 students within the policy bandwidth.[22] I prefer the counselor count measure since it speaks directly to the policy – schools must hire an additional FTE when enrollment crosses a threshold – and because I prefer to use a treatment variable provided in the data, not a measure open to subjective scaling decisions. I run also models using the counselor-student ratio measure as the treatment definition within the policy bandwidth and find consistent estimates.

Table 3 also includes the student-counselor ratio discontinuity estimates at the policy threshold, which provide insights into the practical effect of increased counselor staffing.

[22] The estimates from the full bandwidth using the counselor-student ratio measure illustrate the importance of functional form, especially for constructed measures such as a ratio. Using the full sample, the linear and quadratic estimate reveal large and opposite direction effects – that a school just above the threshold either has a higher counselor-student ratio by 3.3 FTE or a lower ration by 2.6 FTE. This constructed measure results in very large values for small schools and means that small counselor staffing differences will result in large shifts in the ratio measure. For example, a school with 100 students and one counselor has a counselor-student ratio scaled to 450 students of 4.5; a school with 100 students and 1.25 counselors has a scaled counselor-student ratio of 5.625.

Mechanically, as enrollment remains stable around the policy threshold, an increase in staffing leads to a reduction in each counselor's caseload. Among this subsample of 768 schools with at least one counselor (and therefore where I can compute a student-counselor ratio), I observe that each counselor at a school just above the policy threshold manages caseload of about 147 fewer students. This is qualitatively close to the approximate 225 reduction in caseload that perfect compliance on either side of the threshold would imply.[23]

B. Student Outcomes

I then turn to examine in Table 4 the effect of an additional school counselor on student discipline outcomes in the pooled sample and then in Table 5 separately by academic year (2013-2014 and 2014-2015). In each table, I show the reduced form estimate of a school crossing the threshold for hiring an additional counselor, using a linear interaction functional form and the full range of observation, followed the 2SLS estimates using linear interaction, quadratic, and quadratic interaction models, and finally a local linear estimate restricted to the policy bandwidth of $|s_i|$ 225. I examine first a measure of any suspension for any student in the school, followed by ISS and OSS rates for the full sample, then examine overall, ISS, and OSS rates by students' disability status

Pooling the two years of CRDC administration together in Table 4, there is no indication of a relationship between school counselor staffing and student discipline rates across a host of potential discipline incidents. I next examined how additional counselors might affect student outcomes in each year of the CRDC administration, given year-to-year variation in state and federal policy attention to discipline issues. . Looking at overall school incidents, the reduced form estimate in 2013-14 suggests a potential 5.6 percentage point increase in any suspension, likely driven by a very similar 5 percentage point reduced form point estimate for ISS rates.

[23] Perfect compliance assumes a school with 449 students would have one counselor and a student-counselor ratio of 449:1 while a school with 450 students would have two counselors and a student-counselor ratio of 225:1

However, these reduced form effects are only marginally significant and in the case of overall suspensions, that marginal significance disappears with the 2SLS estimates. While suggestive, these are not robustly estimated effects of school counselors on overall school discipline rates.

Turning to examine student outcomes by disability status, I observe that an additional school counselor results in an increase in suspensions for students without a disability, with a significant increase in ISS rates and little to no effect on OSS rates. The 2SLS estimate suggests a 9.5 percentage point increase in suspension rates for students without a disability, and a 7.8 percentage point increase in ISS rates. In contrast to the relationship between counselor staffing and discipline for students without a disability, for students with a disability an additional counselor results in fewer discipline incidents. The overall suspension effect for students with a disability is not statistically significant, though negative. However, the estimate on OSS rates for students with a disability implies that an additional counselor results in a 10.5 percentage point reduction in incidents. In Table 6, I examine the more severe disciplinary outcome of expulsions and find suggestive evidence that an in the 2013-14 data an additional school counselor results in an increase in the expulsion rate for students without a disability, with no consistent or statistically significant effect on students without a disability across models. However, the expulsion effects are small and not precisely estimated.[24] None of these effects exists in the 2015-16 data, and indeed the direction of the point estimate for each type of suspension and subgroup combination swings in the opposite direction. I plot in Figures 3 and 4 ISS and OSS rates for students without a disability (Figure 3) and with a disability (Figure 4), showing the relationship between enrollment and crossing the policy threshold on ISS and OSS rates for the pooled year sample and then separately for 2013-14 and 2015-16. This interesting year-to-year difference in the data suggests the need for further exploration into the policy context, staffing composition,

[24] For simplicity of table display, Table 6 only shows the two years of CRDC administration separately and omits the pooled analysis; there were no statistically significant differences in the pooled sample on expulsion measures.

and incidence rates across years, as well as an acknowledgement that the findings in 2013-14 may be spurious.

I also examined whether increased counselor staffing affected a variety of other student outcomes. Given the broad scope of the counselor's role, there is a plausible theory of change for how a counselor might affect a host of student behaviors, including student course taking, students' participation in college admissions tests, student attendance, or students' grade retention. However, given the primary focus of my analysis on the impact of school counselors on school discipline, I view these analyses as exploratory. As I show in Appendix Table 3, I do not find compelling evidence that schools with additional counselors in Oklahoma in 2013-14 or 2015-16 affected many non-discipline student outcomes.[25] I do find suggestive counselor effects in 2013-14 on increasing the share of students taking Algebra I as a 9th or 10th grade student and increasing the share of students who ever take Algebra II. The suggestive effects on Algebra I course-taking do not hold across models and bandwidth restriction (the point estimates are negative for some models). The Algebra II effects are similar in magnitude across models within 2013-14 and suggest that additional counselors may also have had an effect on students' engagement with higher-level mathematics courses. However, given the lack of an effect on other course-taking outcomes I examine, I caution against over-interpretation of this single outcome.

Finally, given the suggestive effect of school counselors on student discipline outcomes in 2013-14, I then examined for the full sample whether there were additional heterogeneous reduced form effects beyond the differential impacts by disability status. I ran models interacting whether a school was majority minority as well as whether a school's percent of students with a disability was above median (Appendix Table 4, median enrollment around 15 percent). There is some but certainly not exclusive overlap of those categories – about 19 percent of schools are

[25] To fit all results into a single table, I show in Appendix Table 3 the reduced form and main 2SLS estimate from the full sample and linear interaction model, though discuss in text the consistency of estimates across models and have full models available for review.

both majority-minority and enroll an above-median percent of students with disabilities, while 36 percent of schools are majority-white and enroll a below-median percent of students with disabilities. First, I examine whether there are significant first-stage differences across school types – majority-minority and majority-white schools have significantly different policy response rates, with majority-minority schools employing on average an additional 1.3 FTE counselor at the policy threshold compared with a marginally significant 0.26 FTE increase at majority-white schools. The only significant difference in estimated counselor effect by school type was on expulsions at majority-minority compared with majority-white schools. I estimate a 3.9-4.6 percentage point increase in expulsions overall and for students without a disability in majority-minority schools and a significant 1.1-1.3 percentage point reduction on those measures in majority-white schools. There are no significant differences in counselors' effects on student outcomes between schools with above- and below-median enrollment of students with a disability. These results provide suggestive evidence that school counselors have an effect on some suspension rate outcomes, but that counselor effects vary by student type and are not consistent across years. Given these inconsistencies, I cannot rule out that I detected spurious relationships, and view these results as preliminary evidence that motive the need for further exploration in the relationship between counselor staffing and student outcomes with a larger sample.

C. Robustness of Estimates

As observed in Table 4 and Table 5, I run my analyses using a linear interaction, quadratic, and quadratic interaction model as well as a local linear estimate using the smaller bandwidth of $|s_i|$ 225. In 2013-14, where I observe significant effects of counselors on discipline outcomes for students with and without disabilities, the point estimates are generally robust to different models, although are certainly not statistically significant across different models and bandwidth specifications, suggesting caution in interpreting these effects.

I also ran discipline outcomes using higher margins of additional counseling; including schools around the 900-student, 1,350-student, and 1,800-student thresholds for hiring additional counselors, presenting results in Appendix Table 5. I categorize schools as belonging to the nearest margin; therefore a school enrolling 670 students is above the threshold for hiring a second counselor while a school enrolling 680 students is below the threshold for hiring a third counselor; as a result I drop schools enrolling fewer than 225 students so that there are similar margin ranges across thresholds. Therefore, this analysis includes 371 schools; 259 schools around the first threshold, 58 schools around the second, 30 schools around the third, and 24 schools around the fourth. I do not observe any statistically significant effects of counselors when pooling margins together, although the direction of the estimates and the year-to-year variance in directionality persist.[26] I also run discipline outcome models using counselor-student ratio as the treatment in Appendix Table 6.[27] As noted above, due to the functional form of the counselor-student ratio, in Appendix Table 6 I produce the 2SLS estimates within the policy bandwidth. While the point estimates are not statistically significant, likely given the small sample size with that bandwidth restriction, they are very similar in magnitude to the equivalent estimates using counselor count as the treatment. For example, as shown in Table 5, the point estimate for 2013-14 within the policy bandwidth on an additional counselor's effect on ISS rates for students without a disability using counselor count as the treatment is 6.9 percentage points; in Appendix Table 6, the equivalent point estimate is 5.7 percentage points.

VII. YEAR-TO-YEAR CONTEXT AND COUNSELOR EFFECTIVENESS

[26] For example, around the first counselor margin I observe an increase in ISS for students without a disability above the threshold in 2013-14 and the point estimates using multiple margins is also positive. The coefficient for OSS rates for students with a disability is negative in 2013-14 in both the main analysis around the first counselor margin and in the multiple margin models.

[27] Note that the reduced form columns in Appendix Table 6 replicate the reduced form columns from tables 4 and 5 as computation of the reduced form does not rely on the first stage.

The availability of two administrations of CRDC provides the opportunity to examine year-to-year variance in counselor staffing and their effects on students. Although I do not observe robustly estimated counselor effects on many student outcomes, the evidence suggests that in 2013-14 schools with additional counselors experienced a decrease in out of school suspensions for students with a disability but increase in in school suspensions for students without a disability. These effects do not appear in the 2015-16 administration. In this section, I examine year-to-year differences in student enrollment, school resources, discipline incidence rates, and counselor characteristics to explore whether shifts over time on those measures might provide insights into why I observe counselor effects in one year but not the other.

I first examined the extent to which counselor staffing and student enrollment changed across the two years in my analysis. As observed in Table 1, the average school enrollment and student characteristics are not markedly different in 2013-14 and 2015-16. Among schools observed in both years, only six schools had enrollment shifts that resulted in being classified on the other side other policy threshold in 2015-16 than in 2013-14.[28] About 49 percent of schools observed in both CRDC administration experienced some shift in staffing across the two years, with 75 schools increasing staffing and 109 schools decreasing staffing. Among the 75 schools that employed more counselors in 2015-16 than in 2013-14, the individual school increase ranged from adding 0.2 FTEs to adding 1.38 FTEs. This range suggests that Oklahoma schools are able to adjust staffing in very precise ways, to the point where they can increase counseling by 0.2 FTEs, or adding an additional day of counseling each week. Among the 109 schools that cut counseling hours between 2013-14 and 2015-16, the decrease ranged from 0.04 FTEs (or a little under two hours a week) to three full FTE positions. Six schools dropped from some counseling availability in 2013-14 (ranging between 0.2-1.1 FTEs) to no available counselors in 2015-16; conversely eight schools had no counselors in 2013-14 and added counselors in 2015-16. While

[28] Three schools transitioned from above to below the threshold, three schools transitioned from below to above the threshold

interesting insights into Oklahoma school counseling and the precise nature schools can target staff allocations, these statistics do not suggest substantial or particularly uneven changes in counselor staffing from 2013-14 to 2015-16 that might explain variance in whether counselors affect student outcomes.

As table 1 suggests, overall Oklahoma school characteristics are fairly stable across the two years in my analysis. In 2013-14 and 2015-16, student demographics are similar – about 48 percent female each year, about 23 percent American Indian/Native each year. However, I also examined whether there were differential year-to-year shifts on various student demographics and school resources such as number of teachers employed for schools above and below the threshold for hiring an additional counselor, which I display in Appendix Table 7. I see no difference in 2013-14 compared with 2015-16 in terms of the number of teachers employed at a school or whether schools had a security officer for either above- or below-margin schools. Student demographics are also similar, though schools below the threshold have more students on subsidized lunch in 2015-16 than in 2013-14. I also see that there is a substantial decrease in total personnel salaries (the category including counselor salaries as well as other support staff and administrations) for below-threshold schools across the two years; in 2013-14, the schools below the threshold spent about $1.9 million on personnel compared with about $1.4 million in 2015-16. These differences are only marginally significant, though suggest that schools below the threshold for hiring an additional counselor had fewer school resources and served more disadvantaged students in 2015-16.

I also examined the extent to which there may be reporting concerns or wide year-to-year variance in student discipline incident rates with the CRDC. I examined the difference in reported incident rates and whether schools above and below the policy threshold reported significantly different rates across years. I did not find evidence that schools on either side of the policy threshold experienced significant differences in incident rates from year-to-year; in Appendix Table 8, I report the results from year-to-year t-tests run separately for schools that were above or below the policy threshold. Here I restrict my t-tests to schools within the policy bandwidth to capture mean incident rates close to the policy threshold. I see that among schools above the policy threshold, there is a marginally significant decrease in ISS and expulsion rates for students without a disability, suggesting that the lack of an observed school counselor effect on those metrics is potentially driven by schools with an additional counselor reducing the actual incidence or reporting of those infractions. While there was an observed discontinuity on OSS rates for students with a disability in 2013-14 but not 2015-16, there is no evidence that schools below or above the threshold significantly shifted their incidence rates across the two years. Instead, descriptively the average incidence rate for schools below the threshold decreased a little and incidents at schools above the threshold increased a little, enough that the 2015-16 difference is smaller than the 2013-14 difference. These descriptive insights into incident rates shed additional light onto the potentially mechanisms through which there was an observed counselor effect in 2013-14 but not in 2015-16, though are not conclusive.

Leveraging publicly available data from the Oklahoma Department of Education on school counselor characteristics and workload, I also examine the extent to which counselor characteristics might explain different effects across years. In Appendix Table 9, I run various counseling measures as an outcome for my reduced-form model, first examining counselor staffing measures (such as the number of counselors in a school) and then examining counselor characteristics (such as the percent of American Indian/Native counselors). Not all 788 schools from the main regression discontinuity analysis merge with the Oklahoma Department of Education files; there are 757 schools in both files. I examine first whether the first stage observed in Table 1.3 holds for this sub-sample of schools; while slightly smaller, there is still a statistically significant 0.43-0.53 FTE discontinuity at the policy threshold in counselor staffing across years. I then examine the total number of individuals providing counseling in a school; in addition to having more FTE hours, schools above the threshold also have more people providing counseling services, with about 0.6-0.79 additional employees allocating at least part of their hours toward counseling. I then examined whether there was a difference at the threshold in whether school counselors served a single school or split their time across multiple schools; I do not observe a statistically significant difference on this measure of staffing.[29]

I also examined what percent of each employees' time is dedicated to counseling. I observe that in schools above the policy threshold, counselors have fewer of their individual FTE hours dedicated to counseling.[30] Taken together with other measures, this analysis shows that schools just above the policy threshold have more individuals in the school performing counseling tasks, and as a result, students have access to more counseling FTE hours, but that each individual counselor spends a smaller share of their hours on counseling than counselors at schools below the threshold. While the point estimates for 2013-14 and 2015-16 are likely not significantly different from each other, the discontinuity is larger in 2015-16, suggesting that in that year, school counselors in schools above the threshold were spread a little thinner than their counterparts in 2013-14. Finally, I document that schools above the threshold spend more money on counseling, unsurprising since they employ more FTE counseling hours.[31]

I then turned to examine whether counselor characteristics varied at the threshold, examining the percent of American Indian/Native counselors, the percent of white counselors, the percent of female counselors, and the average years of experience of counselors on either side of the threshold. I find suggestive evidence in the pooled models that counselors at schools above the threshold are also more experienced; having worked in the profession an additional 3.5 years,

[29] To calculate this measure, I included Oklahoma Department of Education data on counselor staffing in non-high schools, ensuring that this measure properly captured the extent to which a counselor was assigned to a single high school or, for example, had a 0.5 FTE allocation at a high school and a 0.5 FTE allocation at a middle school.
[30] The most common "other jobs" that counselors hold are teacher (N=143), principal (N=27), and librarian/media consultant (N=14) in 2013-14
[31] Here I only include the percent of an individual's salary connected to their counseling assignment (and is not capturing, for example, the wage rate paid for the teaching portion of the employees FTE assignment). It is worth noting, however, that individuals who share their FTE between counseling and a higher-paying staff position likely have leverage to negotiate a higher salary connected to counseling hours than individuals who only perform counseling or individuals who share their FTE between counseling and a lower-paying staff position.

though that discontinuity is not statistically significant running each year individually. I also observe no statistically significant differences in counselor race or gender across the threshold.[32] While the staffing data does not include counselor certification data, Figure 1 illustrates that from 2013-14 to 2015-16 the number of counselors in Oklahoma working with emergency certifications increased dramatically – there were 13 counselors in the state with an emergency certification in 2013-14 and 46 with emergency certifications in 2015-16. As lessons from the California class size reduction policy suggest, non-certified school staff often are less effective, and to the extent that there was an increase in non-certified counselors in Oklahoma in 2015-16, that may explain the lack of a strong counselor effect on school discipline (Bohrnstedt & Stecher, 2002).

These three descriptive analyses – examining student enrollment shifts across years, examining differential discipline incident rates across years, and examining counseling office staffing and counselor characteristics across years – provide insights into the Oklahoma high school counseling context. I observe that there are not dramatic enrollment shifts across the two years or a substantial difference in the number of counselors observed in 2015-16 relative to 2013-14 that might explain why I do not observe the counselor effect on student discipline in the latter year that I observe in 2013-14. The analysis of discipline incident rates suggest that the lack of a counselor effect on ISS and expulsion rates for students without a disability may be driven by schools above the policy threshold reducing their issuance of those punishments, though year-to-year differences are only marginally significant. If anything, year-to-year differences on OSS rates for students with a disability suggest a regression to the mean over years. Leveraging a panel of Oklahoma school staffing data, I do observe that there are significant differences in counselor staffing on a host of staffing measures, not just the counselor FTE count reported in the CRDC. There is an interesting trend of counseling offices being spread more thinly in 2015-16,

[32] Nearly all counselors in the data have a master's degree, therefore there is little variation to explore at the threshold

with individual counselors dedicating less of their time to counseling activities relative to other duties, such as teaching, but these are only suggestive trends that would require additional examination. Overall, I do not observe markedly different discontinuities on these staffing measures across years, suggesting that differential counselor effects are likely not driven by the composition of the counseling workforce. As noted in the results section, in light of the lack of an effect of counselors on student outcomes in 2015-16, I also cannot rule out that the 2013-14 counselor effect is statistically significant by chance.

VIII. DISCUSSION

School discipline has been a hotly debated topic in U.S. public schools over the past decade, with research highlighting both the lack of evidence that exclusionary discipline benefits disobedient students or their classmates as well as the disparate application of disciplinary actions to certain student groups, including students with disabilities. Policy responses include the "Dear Colleague" letter distributed by the Departments of Education and Justice and the placement of several districts under investigation for their student discipline patterns.

School counselors play a central role in developing and implementing whole-school reform efforts that aim to change school culture and build strong relationships with students in hopes to preventing student misbehavior. School counselors are well suited to serve in this capacity, particularly given their broad mission and training in cross-cultural communications and data management (Betters-Bubon, Brunner, & Kansteiner, 2016). To date, however, the question has remained whether an increase in counselor staffing is an effective strategy to achieve goals of reducing discipline.

I demonstrate through my analyses that Oklahoma high schools strongly adhered to state policies mandating counselor-staffing increases at certain enrollment thresholds. This finding itself is perhaps surprising, given the numerous education funding cuts Oklahoma has experienced over the past decade (CBPP, 2017

). Schools generally maintained counselor-staffing levels at or around those suggested by state legislation in both 2013-14 and 2015-16. In my analysis of Oklahoma staff data, I observe that not only do schools above the threshold have more FTE counseling hours, they also tend to have more individuals performing counseling, and those individuals spend less of their total FTE hours on counseling (implying that above the threshold, counseling offices have more counseling hours available, but those come from people who have competing other jobs at the school). I do not observe that counselors above the threshold have different demographics, though there is suggestive evidence that counselors above the threshold have more years of experience.

In Oklahoma high schools in the year 2013-14, additional school counselors may have had an effect on reducing out of school suspensions for students with a disability, widely accepted in research to be an exclusionary and harmful disciplinary option for children with disabilities.[33] One might expect this to occur due to an increase in available counselor time to implement some of the preventative whole-school behavioral programs and restorative justice models that target reducing discipline outcomes. The particularly large effects for students with a disability, however, point to counselors potentially targeting their time at specific outcomes and student groups. This could work through counselors developing closer relationships with a few students most at risk for OSS and being able to deliver individual counseling and outreach to those students. These whole school models of behavioral management often have a tiered structure of identifying student risk and identifying students in greater need of support who would benefit from customized outreach.

For students without a disability, who comprise the majority of students in most schools, having an additional counselor in the school in 2013-14 appears to have resulted in an increase in in school suspensions, and had no effect on out of school suspensions. Although initially counterintuitive, lessons from other fields suggest that an increase in staff levels is sometimes

[33] Although the point estimates on in school suspension rates for students with a disability trended negative, they are not precisely estimated.

associated with increased monitoring and disciplinary action. Research from nursing home staffing, for instance, suggests that when there are more staff working on a given shift, facilities tend to focus on more time-intensive practices such as patient restraint that do not necessarily lead to better outcomes. In the Oklahoma context, an additional counselor could represent an additional set of eyes and ears around the school who observes behavior likely to result in an ISS. The Oklahoma City Public Schools code of conduct, for example, specifies conduct violations likely to result in a short-term suspension including using profanity, using wireless devices (e.g., cell phones), or generally "behaving in a manner that disrupts or interfered with educational activities" (OCPS, n.d.). National surveys suggest that schools dole out suspensions for a wide range of incidents, including many actions tangential to behavioral issues or classroom disruptions, such as dress code violations (Lacoe & Steinberg, 2018). An increase in counselors—and additional set of eyes observing and punishing low-level infractions—could explain the increase in in-school suspensions for students without disabilities. These opposite observed effects for different types of students suggest that increased staffing of school support staff may not have a universally positive effect on student outcomes, to the extent that decision-makers view reduced disciplinary rates as a positive policy goal.

While I observe these counselor effects on student discipline in 2013-14, I do not observe similar effects in 2015-16, and caution over interpretation of these preliminary findings around the relationship between counselor staffing and student outcomes. Given the many outcomes I test, I cannot rule out that I observe the significant effects of counselor staffing on discipline outcomes in 2013-14 by chance. To investigate further this year-to-year variation, I explore changes in counseling between years and changes in student outcomes year-to-year. I find that a large share of schools shift counseling staffing over the course of two years, and that some schools experience an elimination of counseling staff over that period. I do not find evidence that staffing on the policy threshold or student outcomes varied dramatically across years, suggesting that there was not a substantial change in student outcomes in 2015-16 explaining the lack of a counseling effect. I posit that perhaps schools with lower staffing levels in 2013-14 were able to "catch up" to their counterparts with more counselors, perhaps engaging in similar activities and time use that they needed additional time to implement due to lower staffing.

Finally, as Figure 1 illustrates, the period between 2013-14 and 2015-16 saw a dramatic increase in emergency certifications for school counselors, and one explanation for year-to-year variance might be the unobserved composition of counselors' training and certification backgrounds. Research on teacher effectiveness suggests that while there may be increases in student achievement associated with smaller class sizes, the effects of having a higher quality teacher can be greater than class size reduction (Rivkin, Hanushek, & Kain, 2005). An interesting question remains what the differential effect of improving counselor training and quality would be relative to changes in counselor staff and caseload reductions. This analysis also sheds light on the dearth of research exploring counselor labor markets, and knowledge gaps in how schools provide students with counseling services, who performs those services, and what the implications of counselor labor market trends are on student outcomes.

The role of school counseling in student discipline remains an important issue in Oklahoma and other states. The Oklahoma Advisory Committee to the U.S. Commission on Civil Rights cautioned in their 2016 "school-to-prison pipeline report" that many of the state disparities in disciplinary incidents may be exacerbated by anticipated budget cuts that they suggest could lead to "larger class sizes, fewer…school counselors, and fewer opportunities for…restorative justice models" (USCCR, 2016). As my analyses show, changes in school staffing levels can have differential effects on student discipline. These impacts should be factored in alongside other considerations as states and districts contemplate future changes to school staffing.

Table 1.1: Descriptive Statistics, School and Student Characteristics

	2013-14		2015-16	
	All Schools	RD Sample	All Schools	RD Sample
A. School Characteristics				
Total enrollment	390	231	402	228
Average number of school counselors	1.48	1.09	1.45	0.98
Average number of teachers	27.47	19.10	26.83	18.02
Locale: Urban	0.06	0.03	0.06	0.03
Locale: Rural	0.70	0.78	0.71	0.79
Title I eligible	0.88	0.90	0.81	0.82
Whether school employs security	0.29	0.21	0.26	0.19
Whether school employs psychologist or social worker	N/A	N/A	0.12	0.08
B. Student Demographics				
Female	0.48	0.48	0.48	0.48
American Indian/Native	0.22	0.23	0.22	0.23
Black	0.06	0.05	0.05	0.04
White	0.57	0.58	0.56	0.56
Subsidized lunch eligible	0.66	0.69	0.59	0.60
Documented disability plan	0.15	0.15	0.15	0.16
Number of Schools	442	392	450	396

Notes: Does not include juvenile justice centers, special education schools, alternative schools, charter schools, magnet schools, or schools without a full data match in the Civil Rights Data Collection. "N/A" means the variable was not available for that year. The RD sample includes schools that had a student enrollment less than 900 in a given year.

Table 1.2: Descriptive Statistics, Select Outcomes

	2013-14		2015-16	
	Below Threshold	Above Threshold	Below Threshold	Above Threshold
A. Course and Test Taking				
Share students taking AP	0.05	0.13	0.04	0.14
Share students taking Algebra I early	0.25	0.22	0.24	0.20
Share students taking Algebra II	0.20	0.19	0.20	0.19
Share students taking Calculus	0.01	0.02	0.01	0.02
Share students taking ACT/SAT	0.24	0.26	0.32	0.32
B. Discipline				
Any suspension, all students	0.18	0.21	0.19	0.19
In School Suspension, all students	0.10	0.13	0.11	0.12
Out of School Suspension, all students	0.08	0.07	0.09	0.07
Any suspension, students without disability	0.17	0.20	0.18	0.17
In School Suspension, students without disability	0.09	0.13	0.10	0.11
Out of School Suspension, students without disability	0.08	0.07	0.08	0.06
Any suspension, students with disability	0.23	0.29	0.25	0.32
In School Suspension, students with disability	0.11	0.16	0.12	0.19
Out of School Suspension, students with disability	0.12	0.13	0.12	0.13
Expulsion, all students	0.01	0.02	0.01	0.01
Expulsion, students without disability	0.01	0.02	0.01	0.01
Expulsion, students with disability	0.02	0.01	0.02	0.01
C. School Engagement				
Chronic absenteeism	0.11	0.18	0.12	0.17
Chronic absenteeism, students with disability	0.12	0.16	0.14	0.18
Retained in 10th grade	0.00	0.01	0.00	0.01
Retained in 10th grade, students with disability	0.01	0.01	0.00	0.01
Number of Schools	337	55	340	56

Notes: Does not include juvenile justice centers, special education schools, alternative schools, charter schools, or schools without a full data match in the Civil Rights Data Collection. Restricted to schools within the regression discontinuity sample for the main analysis.

Table 1.3: First Stage Estimates, Compliance with School Counselor Staffing Policy

		Counselor Count			Counselor-Student Ratio			Student-Counselor Ratio		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		n	Linear	Quadratic	n	Linear	Quadratic	n	Linear	Quadratic
Pooled	Full	788	0.676 ***	0.453 **	788	3.261 ***	-2.581 *	768	-146.59 ***	-73.68 *
			(0.14)	(0.17)		(0.58)	(1.03)		(21.80)	(33.93)
	AIC		1091	1085		4670	4596		9478	9467
	|si|≤225	259	0.551 ***		259	0.655 ***		258	-99.78 ***	
			(0.16)			(0.15)			(27.14)	
	|si|≤ 130	132	0.300		132	0.323		131	-102.92 *	
			(0.27)			(0.24)			(45.92)	
2013-14	Full	392	0.752 **	0.542 *	392	4.099 ***	-3.875 *	381	-138.98 ***	-102.43 ~
			(0.24)	(0.26)		(1.05)	(1.93)		(33.49)	(55.18)
	AIC		651	649		2546	2508		4652	4651
	|si|≤225	136	0.574 *		136	0.706 **		135	-102.76 *	
			(0.26)			(0.24)			(43.53)	
	|si|≤ 130	69	0.125		69	0.196		68	-111.52	
			(0.52)			(0.45)			(74.36)	
2015-16	Full	396	0.598 ***	0.362	396	2.379 ***	-1.221 *	387	-155.56 ***	-47.68
			(0.16)	(0.22)		(0.38)	(0.54)		(27.91)	(40.05)
	AIC		410	409		1788	1730		4825	4818
	|si|≤225	123	0.500 **		123	0.582 **		123	-96.44 **	
			(0.19)			(0.18)			(33.92)	
	|si|≤ 130	63	0.419		63	0.407		63	-98.41 ~	
			(0.26)			(0.25)			(57.53) ***	

Notes: Robust standard errors clustered by running variable in parentheses. Models include year fixed effects. Counselor count is the number of FTE counselor positions in a school; counselor-student ratio is the number of counselors divided by school enrollment multiplied by 450; student-counselor ratio is school enrollment divided by the number of counselors and omits schools with zero counselors since enrollment divided by zero is undefined.

~p<0.10, *p<0.05, **p<0.01, ***p<0.001

Table 1.4: Reduced form and 2SLS estimates, discipline

	Reduced form	2SLS			
Any suspension, all students	0.025	0.036	0.010	-0.010	0.009
	(0.022)	(0.032)	(0.028)	(0.067)	(0.048)
In School Suspension, all students	0.025	0.036	0.016	-0.022	0.015
	(0.018)	(0.025)	(0.023)	(0.055)	(0.038)
Out of School Suspension, all students	-0.000	-0.000	-0.006	0.011	-0.006
	(0.011)	(0.015)	(0.013)	(0.030)	(0.023)
Any suspension, students without disability	0.025	0.036	0.008	-0.008	0.013
	(0.022)	(0.031)	(0.029)	(0.064)	(0.047)
In School Suspension, students without disability	0.022	0.032	0.011	-0.028	0.012
	(0.018)	(0.025)	(0.024)	(0.053)	(0.038)
Out of School Suspension, students without disability	0.003	0.004	-0.003	0.020	0.001
	(0.011)	(0.015)	(0.013)	(0.030)	(0.023)
Any suspension, students with disability	-0.007	-0.010	-0.004	-0.021	-0.061
	(0.050)	(0.068)	(0.063)	(0.145)	(0.101)
In School Suspension, students with disability	0.021	0.030	0.025	-0.011	-0.007
	(0.033)	(0.047)	(0.043)	(0.105)	(0.070)
Out of School Suspension, students with disability	-0.033	-0.047	-0.029	0.002	-0.054
	(0.025)	(0.033)	(0.031)	(0.066)	(0.051)
Observations	788	788	788	788	259
Functional form	Linear interaction	Linear interaction	Quadratic	Quadratic Interaction	Linear interaction
Bandwidth	Full	Full	Full	Full	|si|225

Notes: Robust standard errors clustered by running variable in parentheses. Pooled year estimates include year fixed effects. All models include measures of percent American Indian/Native students, percent black students, percent white students, percent female students, percent students with documented disability, the total number of teachers employed, whether the school employs a security guard, whether school is Title I eligible, and school locale.
~p<0.10, *p<0.05, **p<0.01, ***p<0.001

Table 1.5: Reduced form and 2SLS estimates, discipline by year

	2013-14					**2015-16**				
	Reduced form	2SLS				Reduced Form	2SLS			
Any suspension, all students	0.056~	0.071	0.049	0.058	0.060	-0.007	-0.012	-0.044	-0.103	-0.052
	(0.032)	(0.045)	(0.038)	(0.087)	(0.068)	(0.030)	(0.048)	(0.046)	(0.134)	(0.084)
In School Suspension, all students	0.050~	0.064~	0.054~	0.050	0.055	-0.002	-0.004	-0.037	-0.117	-0.036
	(0.026)	(0.034)	(0.030)	(0.072)	(0.054)	(0.024)	(0.038)	(0.037)	(0.120)	(0.066)
Out of School Suspension, all students	0.006	0.007	-0.005	0.008	0.005	-0.005	-0.008	-0.007	0.013	-0.016
	(0.016)	(0.021)	(0.018)	(0.041)	(0.033)	(0.014)	(0.024)	(0.021)	(0.049)	(0.036)
Any suspension, students without disability	0.074*	0.095*	0.070~	0.076	0.089	-0.023	-0.038	-0.072	-0.120	-0.078
	(0.032)	(0.042)	(0.036)	(0.080)	(0.068)	(0.029)	(0.048)	(0.046)	(0.132)	(0.081)
In School Suspension, students without disability	0.061*	0.078*	0.065*	0.056	0.069	-0.018	-0.029	-0.062	-0.141	-0.061
	(0.027)	(0.032)	(0.029)	(0.066)	(0.053)	(0.023)	(0.037)	(0.038)	(0.121)	(0.066)
Out of School Suspension, students without disability	0.013	0.017	0.005	0.020	0.020	-0.006	-0.009	-0.010	0.021	-0.016
	(0.016)	(0.021)	(0.017)	(0.040)	(0.034)	(0.015)	(0.024)	(0.022)	(0.053)	(0.036)
Any suspension, students with disability	-0.116	-0.149~	-0.114	-0.031	-0.148	0.095	0.157	0.127	-0.025	0.061
	(0.071)	(0.083)	(0.076)	(0.185)	(0.134)	(0.063)	(0.104)	(0.092)	(0.238)	(0.148)
In School Suspension, students with disability	-0.034	-0.044	-0.036	-0.020	-0.073	0.074~	0.122~	0.097	0.002	0.083
	(0.048)	(0.060)	(0.054)	(0.129)	(0.094)	(0.044)	(0.072)	(0.066)	(0.182)	(0.109)
Out of School Suspension, students with disability	-0.082*	-0.105*	-0.079*	-0.014	-0.075	0.012	0.019	0.030	0.007	-0.022
	(0.039)	(0.042)	(0.040)	(0.094)	(0.071)	(0.028)	(0.046)	(0.041)	(0.095)	(0.065)
Observations	392	392	392	392	136	396	396	396	396	123
Functional form	Linear interaction	Linear interaction	Quadratic	Quadratic Interaction	Linear interaction	Linear interaction	Linear interaction	Quadratic	Quadratic Interaction	Linear interaction
Bandwidth	Full	Full	Full	Full	|si|225	Full	Full	Full	Full	|si|225

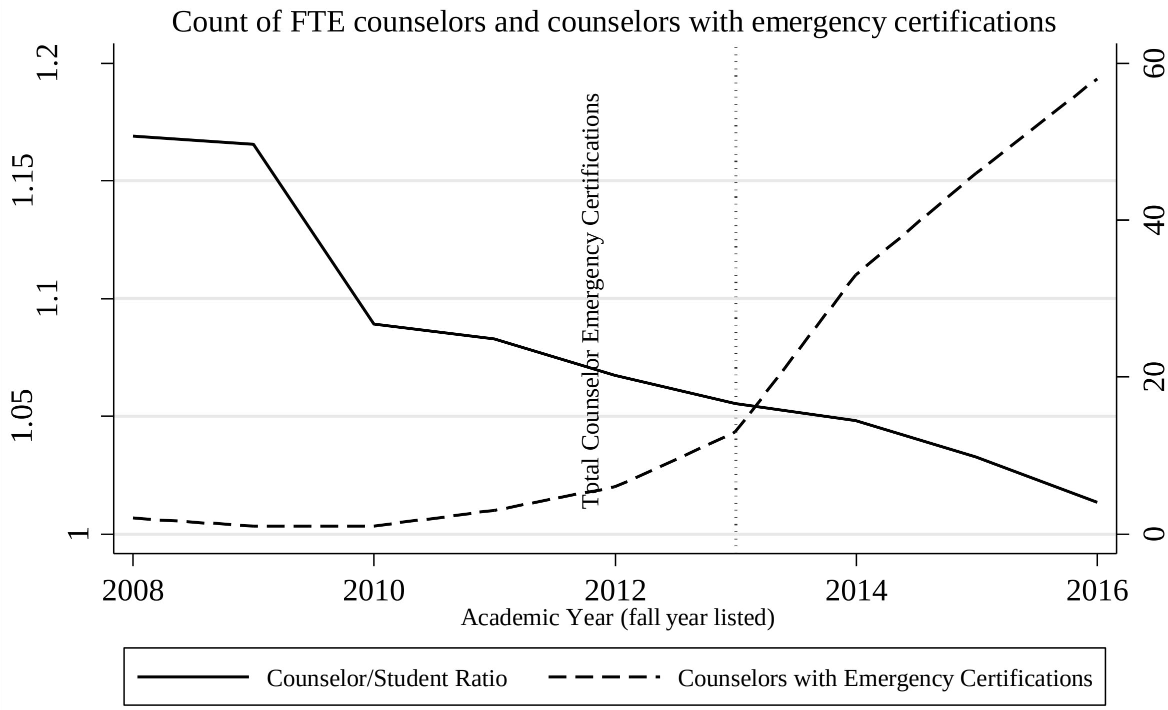
Notes: Robust standard errors clustered by running variable in parentheses. All models include measures of percent American Indian/Native students, percent black students, percent white students, percent female students, percent students with documented disability, the total number of teachers employed, whether the school employs a security guard, whether school is Title I eligible, and school locale.
~p<0.10, *p<0.05, **p<0.01, ***p<0.001

Table 1.6: Reduced form and 2SLS estimates, expulsions

	2013-14					2015-16				
	Reduced form	2SLS				Reduced Form	2SLS			
Expulsion, all students	0.019	0.024~	0.020	0.045	0.026	-0.007	-0.011	-0.011	-0.011	-0.004
	(0.013)	(0.014)	(0.014)	(0.032)	(0.021)	(0.005)	(0.009)	(0.008)	(0.015)	(0.014)
Expulsion, students without disability	0.026~	0.033*	0.029~	0.045	0.032	-0.006	-0.011	-0.009	-0.004	0.001
	(0.015)	(0.016)	(0.015)	(0.032)	(0.024)	(0.005)	(0.008)	(0.007)	(0.013)	(0.012)
Expulsion, students with disability	-0.011	-0.015	-0.017	0.063	0.012	0.000	0.001	-0.003	-0.023	-0.027
	(0.017)	(0.021)	(0.020)	(0.061)	(0.030)	(0.013)	(0.022)	(0.020)	(0.038)	(0.031)
Observations	392	392	392	392	136	396	396	396	396	123
Functional form	Linear interaction	Linear interaction	Quadratic	Quadratic Interaction	Linear interaction	Linear interaction	Linear interaction	Quadratic	Quadratic Interaction	Linear interaction
Bandwidth	Full	Full	Full	Full	|si|225	Full	Full	Full	Full	|si|225

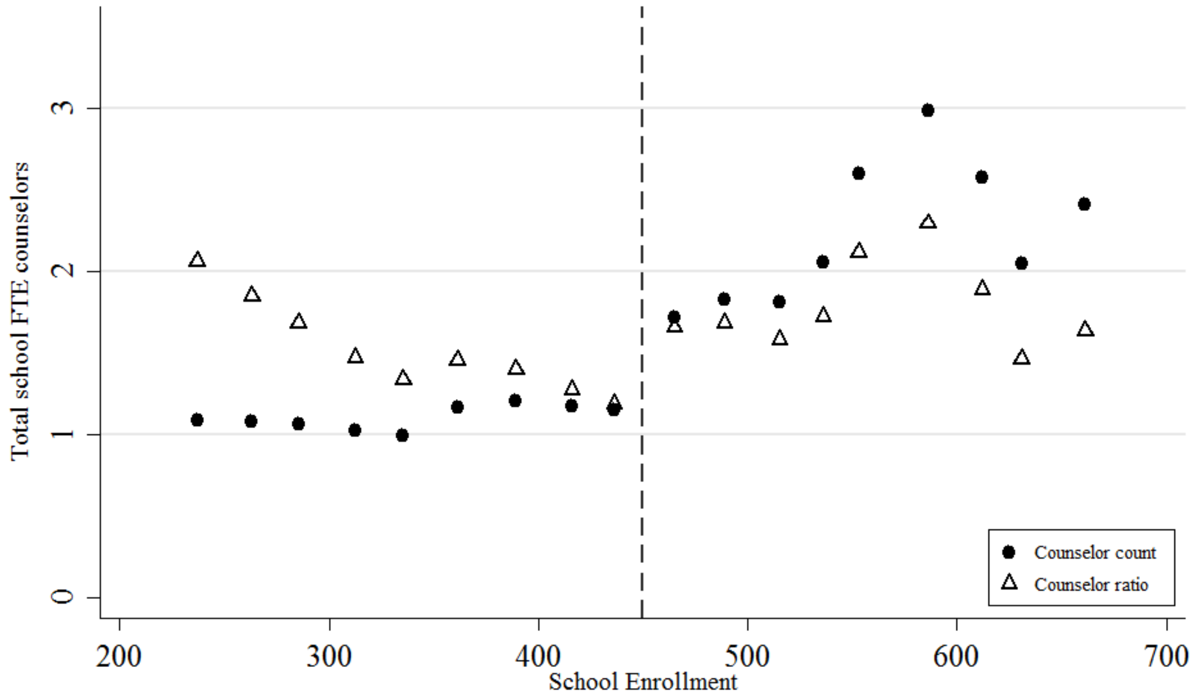
Notes: Robust standard errors clustered by running variable in parentheses. All models include measures of percent American Indian/Native students, percent black students, percent white students, percent female students, percent students with documented disability, the total number of teachers employed, whether the school employs a security guard, whether school is Title I eligible, and school locale.
~p<0.10, *p<0.05, **p<0.01, ***p<0.001

Figure 1.1: Oklahoma Counselor Staffing, 2007-08 through 2016-17



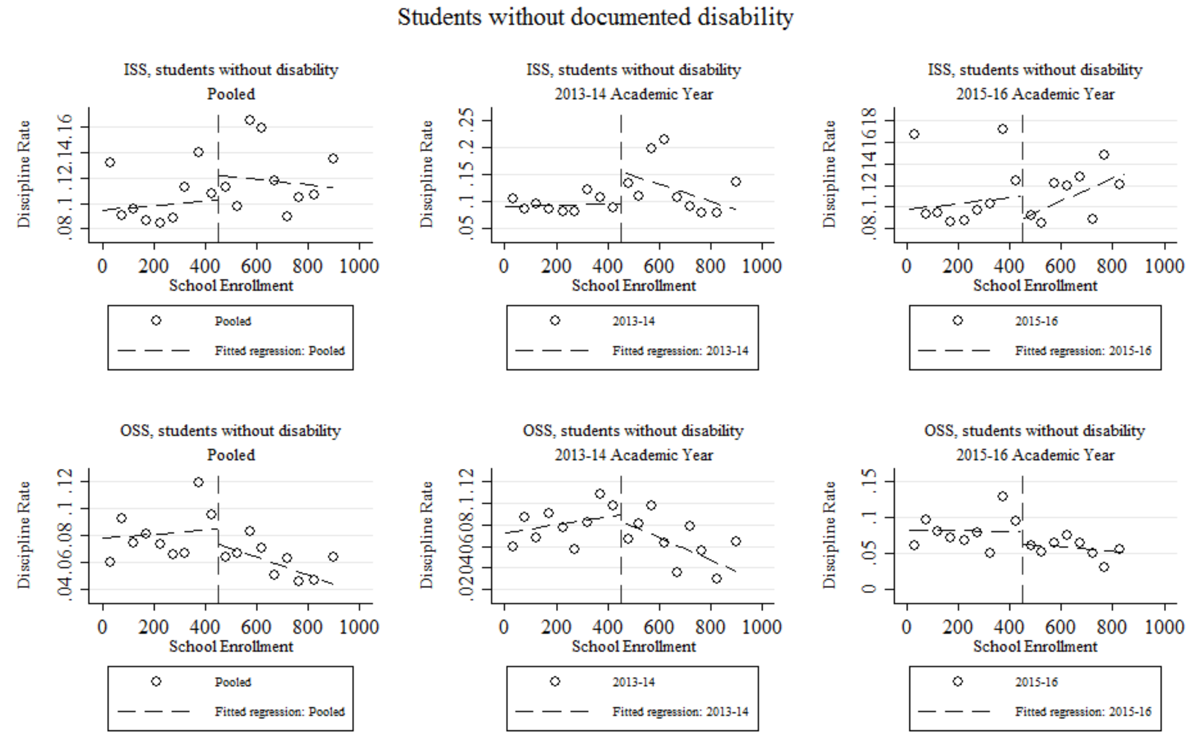
Notes: Counselor ratio represents the number of FTE counselors per thousand students enrolled in Oklahoma for a given year. Uses public data compiled from Oklahoma Department of Education state certificate counts, and school employee and school headcounts aggregated to the state level. Vertical line represents 2013-14 academic year.

Figure 1.2: First Stage Compliance



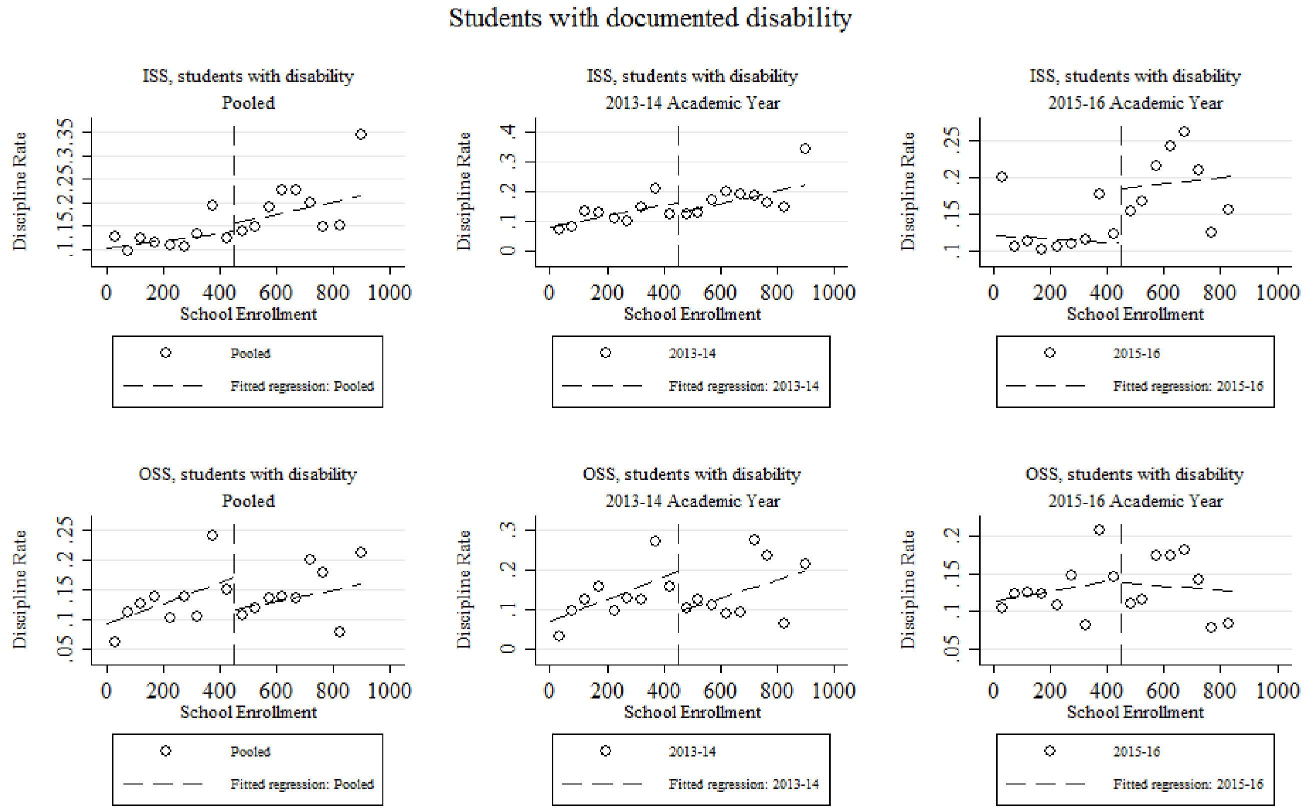
Notes: The above figure shows school counselor staffing levels in Oklahoma public schools in the 2013-14 and 2015-16 academic years.
Binsize = 50, bandwidth $|si| \leq 225$
First-stage linear discontinuity, counselor count: 0.551 (0.159)***, N= 259
First-stage linear discontinuity, counselor ratio: 0.655 (0.149)***, N= 259

Figure 1.3: Select Discipline Outcomes, Students without documented disability



Notes: The above figure shows suspension outcomes for Oklahoma school students in public high schools without a documented disability in the 2013-14 and 2015-16 academic years. Binsize = 50, no bandwidth restriction

Figure 1.4: Select Discipline Outcomes, Students with documented disability



Notes: The above figure shows suspension outcomes for Oklahoma school students in public high schools with a documented disability in the 2013-14 and 2015-16 academic years. Binsize = 50, no bandwidth restriction

CHAPTER 2

Do Small Price Changes Affect College Enrollment? Evidence from the City University of New York (CUNY) College System

Abstract

While overall college enrollment in the United States has increased in recent decades, gaps in enrollment by family income have widened (Bailey & Dynarski, 2011). These gaps persist even accounting for students' academic background (Belley & Lockner, 2007). Research in behavioral science has found that hassle factors – the prevalence of multiple, seemingly low-effort tasks – often stall individuals' progress toward a goal, and conversely the removal of these hassles can advance progress (Bettinger et al., 2012; Madrian & Shea, 2001; Dynarski & Scott-Clayton, 2006). One relatively understudied potential hassle that college-intending students face prior to college enrollment is the "commitment deposit" – commonly a $100-500 outlay that students pay to the college when they sign their intent-to-enroll forms the spring prior to starting classes. These deposits are typically due on May 1 for the fall term. Using data from the City University of New York (CUNY) and the National Student Clearinghouse (NSC), I examine whether enrollment outcomes for students admitted to CUNY vary by whether they are just above or below an arbitrary cut-off determining whether they are eligible for a commitment deposit waiver. I find no effect of waiver eligibility on students' enrollment outcomes; given this null effect, I examine several hypotheses for why the $100 subsidy did not affect enrollment. I find some evidence of policy noncompliance - that nearly a third of students eligible for a waiver still end up submitting their deposit, and several students with higher EFCs find a way not to pay the deposit. To the extent that there is noncompliance, that likely explains part of but not all of the null enrollment findings. I also find no evidence of an enrollment effect by the salience of the waiver requirements or ease of waiver application across institutions, and hypothesize that the timing of the subsidy may come at a juncture in the college-going process where students' demand for education is inelastic to small price shifts

I. INTRODUCTION

While overall college enrollment in the United States has increased in recent decades, gaps in enrollment by family income have widened (Bailey & Dynarski, 2011). These gaps persist even accounting for students' academic background (Belley & Lockner, 2007). There is growing evidence that in addition to financial constraints and academic readiness, various administrative hurdles or "hassle factors" may pose a significant barrier to students enrolling in college. Research in behavioral science has found that hassle factors – the prevalence of multiple, seemingly low-effort tasks – often stall individuals' progress toward a goal, and conversely the removal of these hassles can advance progress (Bettinger et al., 2012; Dynarski & Scott-Clayton, 2006; Madrian & Shea, 2001). Several policies and interventions have successfully targeted reducing the hassle factors associated with taking important college assessment exams (Goodman, 2016; Hurwitz, Smith, Niu, & Howell, 2015; Hyman, 2017; Klasik, 2013), sharing those exam results with colleges (Pallais, 2015), or completing financial aid applications (Bettinger et al., 2012), with positive effects on college enrollment, particularly at well-resourced institutions.

In the context of college matriculation, students must navigate several tasks between gaining admission to a college and arriving on campus the following semester: sending in their acceptance letter, attending an orientation, finalizing financial aid documents, registering for college, and setting up housing, among other tasks. Research shows that a substantial number of low-income students accepted to college fail to show up anywhere in the fall in part due to lack of advising over the summer months on how to navigate the myriad forms and processes required to successfully matriculate (Arnold, Fleming, Deanda, Castleman, Wartman & Price, 2009; Castleman & Page, 2013). Successful interventions to increase college enrollment among college-intending students have included additional advising support (Castleman, Arnold, & Wartman, 2012; Castleman, Page, & Schooley, 2014) and technology-based outreach and reminders about important tasks (Castleman & Page, 2014; 2015). These interventions work not by eliminating administrative tasks, but by providing students with reminders and advice necessary to navigate these hassle factors. Some hassles are necessary for the operation of an organization such as a college, but the extent to which there is inequality in students' access to the resources needed to navigate those hassles certainly contributes to inequalities in college going.

One relatively understudied potential hassle that college-intending students face prior to college enrollment is the "commitment deposit" – commonly a $100-500 outlay that students pay to the college when they sign their intent-to-enroll forms the spring prior to starting classes. These deposits are typically due on May 1 for the fall term. The college then usually applies the deposit to the student's fall tuition and fee charges.[34] One goal of the financial outlay component of the decision announcement process is to create a barrier to students accepting spots at more than one university – "double depositing" or "double committing" – which a student might do to give themselves additional time to decide between institutions.[35] While deposits deter students from committing to multiple institutions, colleges also recognize that for many low-income students, this initial financial outlay may prove too large a burden and waive the deposit for students.[36]

There are many theoretical reasons why a small price change might affect students' college investment decisions, as well as empirical analyses that document this phenomenon. Demand theory in general argues that an increase in price results in a reduction in demand, holding all else constant; and conversely a decrease in price is associated with an increase in demand for a good or service. Research shows that college-going is not inelastic in response to price changes, as evidenced by the effectiveness of various financial aid policies at increasing student enrollment (Bettinger, 2004; Castleman & Long, 2013;

[34] Colleges ask accepted students to send in their intent-to-enroll and a commitment deposit for several reasons. First, colleges need an accurate sense of how many students will arrive on campus for the next semester well ahead of the start of term so they can provide appropriate resources aligned to the size of the cohort (such as housing). To this goal, colleges ask students to submit their intent-to-enroll by May 1

[35] If double committing were an accepted practice, low-income students in particular would likely have a stronger incentive to keep multiple institution options open while they negotiated financial aid packages (College Board, n. d.). Since students double committing would add error into colleges' calculations of the incoming class, institutions add barriers and sanctions in the form of a financial deposit and the threat of rescinding the admissions offer if a student is found to have double deposited (College Board, n. d.).

[36] This is a particularly important waiver for students who would otherwise have their full fall tuition covered by grant aid, but do not yet have access to that aid to pay for the deposit. For students who would have grant aid pay for their full tuition, the commitment payment is not a deposit, but rather a fee

Goldrick-Rab et al, 2012; Scott-Clayton, 2011). After matriculation, students' demand for different types of education remains elastic to differential tuition, with one study finding a negative elasticity to price changes of 0.3-0.5 for differential tuition for business and engineering (Stange, 2014). However, the empirical research on financial aid and tuition policies has generally focused on large price changes, while this analysis examines a smaller $100 cost that students encounter. Further, the fundamental relationship between price and demand is based on all other factors remaining equal, and to the extent that monetary price is substituted with the cost of time and effort to complete administrative steps to access the discount may affect how much of a demand shift results in a small price change. The empirical question remains how elastic students' demand for college enrollment is to small price shifts and how the characteristics of those price shifts, such as when costs are imposed, influence the effect a price change has on demand.

By easing potential short-term financial constraints of admitted students, deposit waivers could affect both inframarginal and marginal enrollment outcomes. For example, inframarginal students who plan to attend college no matter what might be more likely to enroll at an individual institution that offers a waiver if the deposit poses a financial barrier and other institutions in their choice set do not offer a similar waiver option. To the extent that students are on the margin of college enrollment and their decision whether to matriculate is sensitive to small price changes, a waiver may make that student more likely to enroll in college at all.

My paper adds to a robust literature examining how students make decisions about investing in education (Becker, 1964). While these is a large body of evidence around how large-scale policies, such as financial aid awards, affect students' investment, there is also increasing attention to the smaller-scale costs and barriers students face as they consider their postsecondary options. However, studies of smaller price changes have focused on price shifts earlier in the college search process (e.g., the effects on price changes on the application margin). To the best of my knowledge, there have not been prior empirical studies investigating whether the price of a commitment deposit, which occurs further along in students' journey to and through college, affects whether and where low-income students pursue postsecondary education.

Using data from the City University of New York (CUNY) and the National Student Clearinghouse (NSC), I examine whether enrollment outcomes for students admitted to CUNY vary by whether they are just above or below an arbitrary cut-off determining whether they are eligible for a commitment deposit waiver. I focus on immediate enrollment in CUNY and, if students do not enroll at CUNY, whether, where, and when they enroll in college. I then investigate whether students' responsiveness to the waiver offer varies by how transparent a given CUNY institution is about waiver availability.[37]

Overall, I do not detect a relationship between waiver eligibility on students' enrollment decisions, across various specifications and subgroups, though the confidence intervals around my estimates do not rule out meaningful enrollment effects in a positive or negative direction. I do find that many students eligible for the waiver appear to access it, inferred from a lack of a deposit transaction record with the CUNY bursar's office. However, I also find that about a third of admitted students eligible for the waiver end up paying a $100 deposit anyway, suggesting noncompliance below the threshold. I also observe many students above the waiver threshold without a deposit record, suggesting some other means of avoiding a payment and further contributing to noncompliance that would theoretically depress an enrollment effect of the waiver program. Nevertheless, there are substantial discontinuities in deposit rates around the threshold, suggesting that the waiver benefits a large proportion of CUNY admitted students and there is likely not a noncompliance rate that would completely wash away any true enrollment effects that might exist. One explanation for the imprecisely estimated overall effect is that the policy waiver is more effective at certain colleges – such as schools with easier to navigate waiver applications – or for certain students – students' whose demand for college is more

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[37] I categorize CUNY colleges according to how easy each institution makes it to obtain a waiver – for example, whether students need to check a box on their acceptance form, or if they need to submit additional documents to a separate office – and examine whether enrollment effects vary by the salience and level of hassle factors associate with obtaining the waiver

elastic to price changes. I do not find significant enrollment effects across a variety of institutional groupings, running analyses separately for schools with highly publicized waiver programs, schools with significant discontinuities in deposit payments, and community versus senior colleges. While I do find some suggestive evidence that certain subgroups of students (older students and first-generation students) may have differential enrollment responses to waiver eligibility, those effects are small and imprecisely estimated.

In section II, I provide an overview of the classical economic perspectives on how costs and subsidies might affect students' enrollment decisions, as well as behavioral science research that informs how students might access the waivers. In section III, I provide additional information on the CUNY context as well as an overview of the available data. In section IV, I outline my empirical analysis and in section V I present evidence around identification. Section VI includes the overall enrollment results, the deposit payment effects, and explores whether there is differential responsiveness to waiver eligibility across institutions and student subgroups motivated by prior literature on students' engagement with the college enrollment process. I conclude in section VIII with a discussion of the findings and policy implications.

II. BACKGROUND AND LITERATURE REVIEW

Researchers have long documented students' responsiveness to changes in college costs; generally, a $1000 change in cost is associated with a 3-5 percentage point shift in enrollment (Deming & Dynarski, 2009; Dynarski, 2003; Leslie & Brinkman, 1988). Notably not all studies of financial aid find an effect on student enrollment; one prevailing explanation for differences in aid efficacy is how complex and transparent a given grant program is. For example, there is little evidence that one of the largest federal financial aid programs – the Pell Grant – affects enrollment for marginal recipients. Some hypothesize that students are unaware of the program and don't receive information about their eligibility until too late in the college decision-making process for the program to affect the extensive enrollment margin (Carruthers & Welch, 2019). The fact that the more highly publicized programs, such as the

Georgia HOPE scholarship, have larger effects on college enrollment, lends support to the hypothesis that students' awareness of a financial aid program is an important condition for that aid to affect students' enrollment decisions (Dynarski, 2000).

Increasingly, researchers have found that even small price differences matter in students' educational investment decision-making. In an analysis of college application fees, researchers found that a 10 percent increase in an institution's application fee was associated with a 0.76 percent decrease in applications (Smith, Hurwitz, & Howell, 2014). The salience of costs often influences how effective a price change is at influencing behavior. In one experiment, researchers found that individuals substantially altered their shopping behavior when the sales tax of an item was advertised next to it on the shelf as opposed to when they encountered the final transaction cost at the cash register (Chetty, Looney, & Kroft, 2009). In both daily consumption and college investments, the timing and salience of a cost or subsidy strongly affects the elasticity of demand. The effects of a college commitment deposit or waiver on student enrollment may depend on when the deposit cost is most salient or the timing of when eligible students learn about waiver eligibility. In many ways, the commitment deposit represents more of a "cash register" price change – something students pay once they have progressed down the path of "purchasing" college. Individuals at the cash register have already committed to bringing home their selected item and are unlikely to return an item to the shelf due to the addition of expected taxes (even if the actual tax amount did not enter into their head while shopping). Similarly, by May of their senior year students may be committed enough to attending a given college to not be deterred by a commitment deposit, especially since it is not a surprise price. On the other hand, the Smith, Hurwitz, and Howell (2014) analysis of application fees represents an "aisle price" – students being deterred from beginning a transaction, much as individuals were deterred from adding an item to their cart by the higher advertised price of an item.

Two studies of small price changes in the college search process found that increasing the number of free score reports a student could send of their college standardized test results increased postsecondary enrollment (Hurwitz, Mbekeani, Nipson, & Page, 2017; Pallais, 2015). However, small price changes often come as part of a bundle of policies, and in the case of score sends, the subsidy also operated to change the default recommended behavior – in this case, the implied recommendation in the number of colleges to which a student should apply. Pallais (2015) examined a chance in ACT Corporation policy that allowed students to send four free score reports to colleges instead of three. This was effectively a $6 change in price, and as a result, the majority of students now submitted four score reports – prior to the change, 82 percent of ACT-takers submitted exactly three reports and after the policy change, 74 percent of ACT-takers submitted exactly four reports (Pallais, 2015). This shift in modal application count argues that the policy worked by changing the default behavior – by offering four free score reports, the ACT Corporation implicitly endorsed that four schools was the "right" number of colleges to apply to, or at least to send scores to. In a similar analysis, researchers examined the effect of a College Board subsidy that provided low-income students (those who had received an exam fee waiver) with four additional SAT score reports (Hurwitz et al, 2017). They found similar effects on score sending, a two percentage point increase in on-time college enrollment, and a 1.7 percentage point increase in bachelor's degree completion rates (Hurwitz et al, 2017). These two studies suggest that the default option is an important factor that influences student behaviors in the college search and application process.

While costs matter in students' college application and enrollment decisions, often a policy's effectiveness at improving postsecondary enrollment rates relies more on the change in framing than the change in price. Several studies have found that defaults and the status quo bias exert a powerful influence on individuals' behaviors – for example, when a company default is to automatically enroll employees in a 401(k) program, enrollment rates are substantially higher than when individuals must take action to enroll in the program (Choi, Laibson, Madrian, & Metrick, 2001; Madrian & Shea, 2001). Defaults are particularly effective because they shift the types of hassle factors individuals encounter. Individuals struggle to attend to these tasks in the face of numerous competing priorities or desires, even if an onerous investment of time in the present would result in substantially better outcomes in the longer-term (Beshears et al, 2012; Mullainathan & Shafir, 2012). The lower-income students that the CUNY policy targets are likely particularly susceptible to behavioral biases and being deterred by hassle factors. Many contextual factors affect the cognitive bandwidth people are able to dedicate to decision-making, and several studies have documented that the stress of poverty especially impairs decision-making and overcoming hassle factors to address important tasks (Gennetian & Shafir, 2015; Mullainathan & Shafir, 2013; Schilbach, Schofield, & Mullainathan, 2016).

Finally, the salience of an intervention affects its success. In the postsecondary sector, there have been several well-intending policies targeted at improving students' college search and application process that have had little to no effect (Meyer & Rosinger, 2019). One tool targeting college applications rolled out by the Obama administration was a College Scorecard, providing comparative information to students about colleges to aid their decision-making. However, researchers have not yet detected dramatic shifts in college search or application behaviors associated with the Scorecard's introduction. On the search margin, Huntington-Klein (2016) measured Google Search Trends for 3,595 two- and four-year institutions in the Scorecard dataset before and after Scorecard introduction. He found small increases in searches for colleges with lower tuition, higher graduation rates, and higher earnings. At the application margin, researchers used SAT "score sends" as proxies for applications and found similarly small effects, with 2.4 percent more SAT test scores sent to schools with higher earnings but no effect on score send behavior in response to an institutions' cost or graduation rate information (Hurwitz & Smith, 2018). They found effects concentrated among schools with a low proportion of underrepresented minority students or a low proportion of students eligible for free- or reduced-price lunch and among Asian and White students and students with higher SAT scores. They did not find significant effects on the college enrollment margin.

These papers suggest that while the Scorecard had a small effect on college search and application behaviors, those effects were concentrated among relatively high-achieving and economically advantaged students, and may contribute to widening socioeconomic gaps in college matriculation. As the authors note, the relatively unimpressive effects of the Scorecard contrasted with other successful proactive college outreach campaigns may reflect differences in how students access information. While active, more involved informational interventions have typically pushed outreach directly to students via mailings, text messages, and sometimes by interactions with college advisors (e.g., Castleman & Page, 2015; Hoxby & Turner, 2013), the Scorecard is an example of a policy that relies on students to seek out information on their own. Students of all income levels may benefit from well-organized information and policies to help facilitate the college investment decision-making process, but delivery systems matter, and low-income students are less likely to realize benefits from passive policies than targeted outreach.

In the CUNY context, the prior literature in students' college search processes suggest different possible effects of the commitment deposit waivers on student enrollment. First, research suggests a small price change can have larger effects on student behavior than one would expect. A $100 deposit waiver subsidy is small relative to financial aid scholarships, but larger than the price changes in other studies that yield college enrollment effects. Second, that research suggests the reason why small price changes matter is because they coincide with other policies, such as shifting the recommended actions students should take in their college search and applications, and that a simple price change absent a context change may not have as large an effect on student outcomes. Lessons from behavioral science suggest it is difficult for individuals to complete complex tasks, due to various administrative hassle factors. Finally, students cannot take advantage of a program that they are not aware of. While the CUNY waiver option is advertised on the central system website and mentioned various places on individual college websites, the salience of the program varies by college. In CUNY, the various hassle factors associated with waiver access or simple lack of knowledge about the program may hamper students' accessing the waiver, resulting in no enrollment effect.

III. CONTEXT AND DATA

A. City University of New York (CUNY) College System[38]

I use data from the City University of New York (CUNY) college system. The system includes 11 senior colleges that offer baccalaureate programs and 7 community colleges that award associates degrees and certificates. The 11 senior colleges have admission rates ranging from 30-99 percent and

[38] Statistics shared in this section come from author tabulations using the most recent (as of October 2018) College Scorecard data.

median SAT scores between 430-595 in verbal and 455-645, while the community colleges are open enrollment. Together, the CUNY colleges serve about 221,000 undergraduate certificate/degree-seeking students. Individual college demographics vary – 87 percent of students at Medgar Evers College identify as black while 73 percent of students at Hostos Community College identify as Hispanic. Between 11-38 percent of students enroll part-time across the 18 colleges, between 41-71 percent of students receive the federal Pell grant, and at four colleges, adult students comprise at least a third of the undergraduate student body.

On-time graduation rates (within six years) at the senior colleges range between 13-66 percent and on-time graduation rates (within three years) at the community colleges range between 16-44 percent. Four CUNY colleges place in the top ten colleges for socioeconomic mobility in the United States (Chetty, et al., 2017). The CUNY system has also invested substantial resources in intensive advising for associates degree students at nine colleges called the Accelerated Study in Associate Programs (ASAP), which provide students with financial resources, structured academic pathways, and enhanced student advising. The ASAP program has been rigorously evaluated with randomized controlled trials that find participating students are twice as likely to graduate with their associates within three years and are 10 percentage points more likely to earn their associates degree after six years of initial enrollment (MDRC, 2017). The CUNY system provides a range of college options for students in the New York City area and beyond, most of which help students achieve positive postsecondary outcomes.

B. CUNY Commitment Deposits and Waivers

All 18 CUNY system colleges require students to submit a $100 commitment deposit upon accepting an offer of admission. The CUNY webpage for admitted students makes it clear the amount of the deposit, the deadline, and that "students may have the Commitment Tuition Deposit waived…with an expected family contribution (EFC) of $3,000 or less."[39] However, each college varies in the steps needed

[39] The stated consequences of not paying a deposit or securing a waiver include that students "will not be allowed to participate in any early registration, and…may miss the opportunity to join us for the upcoming term" (Medgar

for students to receive a waiver. For example, at the College of Staten Island and Guttman Community College, students simply check a box on their acceptance letter and send in a copy of their student aid report (SAR) to verify their EFC with their intent-to-enroll. Other colleges do not post information on their waiver policy on the public admitted students site (though might have instructions once students log into their credentialed online account).

I categorize CUNY institutions into three groups according to how salient commitment deposit waivers are on individual websites and the hassle factors associated with receiving a waiver. In the “low effort” group, I include colleges where students can request a waiver by submitting their SAR at the same time as submitting their acceptance of a spot. In the “medium effort” group, I include colleges that highlight the availability of the waiver but require students to take additional steps beyond the submission of the acceptance form – for example, emailing their SAR to the bursar’s office with an official request. In the “high effort” group, I include colleges where I was unable to find public-facing reference to the waiver on the college website; while that information may be present on those schools’ acceptance letters, they are not easily accessible to students navigating the admissions and financial aid websites of those colleges. Table 1 displays the colleges that fit into each category along with sample language around deposit waivers. There is some variation even within category, with the notable variance of John Jay College appearing to provide a waiver automatically to students they deem eligible. The John Jay admitted students’ website states, “If you qualify for a waiver, you will not see the Pay Commitment Deposit button. Waivers are given to veterans and their dependents; SEEK students and students who have filed financial aid and have an Estimated Family Contribution (EFC) of $3,000 or less.” I hypothesize that to the extent the waiver is in fact automatically applied to John Jay admitted students,

Evers College commitment deposit form). At Queens College, the website highlights that timely deposit or waiver submission enables students to “schedule your orientation, placement tests, and registration.”

any effects of a waiver on student outcomes may be strongest there since all hassle factors associated with the waiver application appear to have been removed from the process.[40]

C. *Student Data and Outcomes*

I use data on all CUNY applicants provided by the CUNY office of policy research. This data includes application information (e.g., race, sex, and prior academic achievement), financial aid information (e.g., EFC and family structure), deposit submission information (whether a student paid a deposit), and National Student Clearinghouse (NSC) information on students' matriculation to non-CUNY postsecondary institutions. I limit my sample to one observation per student per application cycle (allowing for multiple observations per student if an applicant applied multiple years to CUNY). I use students' last recorded EFC prior to the commitment deposit deadline as the forcing variable for the majority of my analyses.[41]

For outcomes, I observe whether students enrolled at CUNY the semester immediately following application and students' enrollment at other postsecondary institutions. The most obvious effect of waiver eligibility around the CUNY threshold on enrollment behavior would be whether an accepted student enrolled at CUNY. An overall enrollment effect would potentially exist if students' postsecondary choice set is limited to attending a CUNY institution if they receive a deposit and no college if they do not (e.g. all other colleges to which they were accepted are too expensive).[42]

D. *Analytic Sample*

[40] While individual colleges' effects are interesting, it is important to note limited statistical power to run school-specific analyses given how observation "hungry" regression discontinuity designs are (Deke & Dragoset, 2012).
[41] I plan to run analyses using students' first EFC and final EFC as the forcing variable as well to document the robustness of my findings to different specifications.
[42] While outcomes are currently limited to enrollment, I am pursuing the option to obtain records of whether students accepted a spot at CUNY to recover the most proximal outcome that waiver eligibility would affect. Conversations to date with the CUNY office of policy research have indicated limited staff capacity to uncover these records; however, one approach may be to identify a small set of colleges and focus on obtaining acceptance data from those schools.

I use data on CUNY applicants for the fall 2016, 2017, and 2018 terms. I limit my analysis to admitted students who are not automatically exempt from paying the deposit – namely, excluding students accepted to special CUNY support programs such as ASAP or students over the age of 60.[43] I also limit my available sample to students who have filed the FAFSA and therefore have the forcing variable – students' EFC – on record, and those who filed before the deposit receipt/postmarked deadline of May 1.[44][45] This gives a potential sample of 89,295 individuals across three cohorts – Table 2 provides summary statistics. The average age for applicants in the full sample is 19; the sample skews slightly female at about 58 percent, with a quarter of the sample identifying as white and about a fifth each identifying as black or Asian. Applicants have low EFCs on average, typically between $1,000 and $1,300 across cohorts and FAFSA filing dates. I primarily run analyses within a limited bandwidth of students with an EFC +/- $1,100 from the $3,000 cutoff as calculated using the *rdrobust* package in Stata according to the recommended mean-squared error (MSE) optimal calculations of Calonico, Cattaneo, & Titiunik (CCT) and rounded to the nearest ten, resulting in 7,614 student observations across the three cohorts (Calonico, Cattaneo, & Titiunik, 2014).[46] The analytic sample within the CCT bandwidth includes more black and Hispanic students and fewer Asian and White students relative to the full sample, which also mechanically includes a wider range of family incomes and assets.

IV. METHODOLOGY

A. Sharp Regression Discontinuity Effect of Waiver Eligibility on Student Enrollment

[43] Veteran applicants are also automatically exempt from paying the deposit, but veteran status is not currently available for the cohorts I analyze in this paper. I hope to obtain veteran status data from the CUNY office of policy research; however, less than 0.25 percent in the fall 2011 applicant cohort were veterans and it is unlikely this additional sample restriction will affect overall findings.

[44] I also run models restricting the sample to students who filed prior to June 1 to allow for late FAFSA filing and late deposit submissions; this adds about 2,000 additional observations each year.

[45] My final restriction drops individuals with an EFC greater than $10,000 since such students would not fit into any potential bandwidth restriction.

[46] I calculate a separate bandwidth for analyses using a larger sample that includes FAFSA filers in June; that bandwidth rounds to +/- $1,200. In future iterations of this project, I will use outcome-specific bandwidths (for example, the bandwidth of $1,100 for the May FAFSA filer sample is for the CUNY enrollment outcome; however, the bandwidth for that sample for the outcome of enrolling in college anywhere is $940 and for enrolling at a two-year is about $950).

I leverage the CUNY policy that students just below an EFC threshold have the option to use a waiver for their commitment deposit while students with an EFC just above the threshold are not eligible for the waiver and theoretically should have to pay $100 to secure a spot in the incoming class. This sharp regression discontinuity strategy examining the effect of waiver eligibility argues that the arbitrary nature of the EFC cutoff means that waiver eligibility is determined in a way that is uncorrelated with how students will respond to treatment (Imbens & Lemieux, 2008; Lee & Lemieux, 2009; Thistlewaite & Campbell, 1969). My analysis examines the intent-to-treat effect of waiver eligibility on student outcomes. My sharp regression discontinuity model is as follows:

$$Y_{its} = \beta_0 + \beta_1(Distance_{its}) + \beta_2(Below_{its}) + \beta_3(Below_{its} * Distance_{its}) + X_{its} + \lambda_t + \delta_s + \varepsilon_{its}$$

where Y_{its} represents the college enrollment outcomes for student *i* in year *t* who was admitted to CUNY college s. $Below$ is an indicator for having an EFC below $3,000 and thus being eligible for a waiver (taking on values of 0 or 1), $Distance$ is the student's EFC centered at the waiver cutoff, and $Below$*$Distance$ is an interaction between the two allowing for different slopes on either side of the cutoff.[47] I also include an application cohort fixed effect, λ_t and an admitted school fixed effect, δ_s, as well as a vector of student-level covariates X_{its}, including age, race, sex, parental education, students' dependent status, and high school preparation.[48] I run models with standard errors clustered at the running variable as recommended by Lee and Card (2008), though results are robust to various clustering choices.

V. IDENTIFICATION

The validity of findings from this reduced form analysis rests on the extent to which students on either side of the EFC cutoff are assumed to be "equal in expectation" – or dissimilar only in their eligibility for a waiver (Lee & Lemiuex, 2008; Urquiola & Verhoogen, 2009). The concern if students are

[47] As noted above, I run my main models on students who completed the FAFSA by May 1, and accordingly use their last observed EFC prior to May 1 for calculating $Distance$. For example, if a student has a FAFSA recorded on February 1 and March 15, I use their EFC as of the March 15 filing to calculate $Distance$.
[48] The inclusion of covariates in a regression discontinuity model helps improve precision of the estimates (Imbens & Lemieux, 2008)

not equal in expectation is that there may be unobserved factors that resulted in a different composition of students in the treated and untreated groups on either side of the cutoff and the core assumption of regression discontinuity – that only the forcing variable in relation to the cutoff determines treatment – do not hold (Cattaneao, Idrobo, and Titiunik, 2018). To evaluate the extent to which the equality in expectation assumption holds, researchers examine whether the density of observations varies on either side of the cutpoint. The second test looks for evidence of manipulation into the treated or untreated conditions – in this case, students adjusting their FAFSA data to obtain an EFC just below the threshold to receive a deposit waiver. First, I present in figure 2 a test of the density of observations around the cutpoint (McCrary, 2008). Density appears smooth around the cutoff, suggesting that endogenous sorting is not a concern.

I also report in table 3 the continuity of student characteristics across the threshold; the presence of a discontinuity on a pre-treatment student characteristic at the cutoff suggests that the treated and untreated students are not equal in expectation. As recommended by Cattaneao, Idrobo, and Titiunik (2018), I run my regression discontinuity model with each student characteristic as the outcome in question, calculating a variable-specific MSE-optimal bandwidth for each estimation, rounded to the nearest ten. Accordingly, the sample size used to evaluate continuity of the covariates at the threshold varies, as documented in the final column of table 3. For example, the MSE-optimal bandwidth for evaluating the continuity of female around the threshold is +/-1,140 resulting in a sample size of 7,901 observations across the three cohorts, while the optimal bandwidth for evaluating first-generation is +/-800 resulting in a sample size of 5,492 observations across the three cohorts. Of the ten covariates I evaluate, there is a statistically significant discontinuity in the percent of Asian students and marginally significant discontinuity in the percent of first generation students. I also run these tests for each application year, and calculate a variable- and cohort-specific MSE-optimal bandwidth, shown in appendix table 1.[49] In the year-specific tests, there are no significant discontinuities in the 2017 cohort,

[49] There is not enough variability in American Indian to calculate optimal bandwidths by year, so that variable is dropped from the appendix table.

and the only significant discontinuity in 2018 is the percent of dependent students; the discontinuities in percent Asian and percent first generation persist in 2016 but do not appear in other cohorts. I have explored whether there was something unique about the 2016 cohort by examining differential merge rate across CUNY files or FAFSA filing rates for each cohort, finding no difference that might explain the differences on student covariates in 2016. I also run the McCrary density test by cohort (Appendix Figure 1); I see no significant density discontinuities across cohorts, although the direction of the density discontinuity point estimate is negative in 2016 and positive in 2017 and 2018. I run my analyses with and without the 2016 cohort to examine the extent to which the slight imbalance on student covariates in 2016 might affect my results.

VI. RESULTS

A. College Enrollment Outcomes

I first graph in figure 2 students' likelihood of enrolling at CUNY (the commitment deposit deadline), plotting average enrollment rates by EFC (measured as of May 1 each application year) bins of $50 within the CCT bandwidth. There are two main takeaways from this graph; first, there is no visually clear discontinuity in enrollment rates at the waiver threshold. Second, a linear functional form appears to be appropriate within the limited bandwidth, and indeed, there appears to be little to no overall relationship between EFC and likelihood of enrolling at CUNY.[50]

In table 4, I report the regression coefficients on the effect of waiver eligibility on students' enrollment outcomes, starting with CUNY enrollment but also examining through the National Student Clearinghouse data whether waiver eligibility affected whether students enrolled anywhere, if they enrolled at four-year institution, whether they enrolled in a public institution, and whether they enrolled in

[50] As appendix figure 2 shows, using the larger sample of individuals with an EFC less than $10,000 also supports a linear relationship and does not display a visual discontinuity in enrollment at the threshold (N=89,295 observations).

New York State.[51] There is no statistically significant effect on any outcomes of being waiver eligible versus non-eligible, although the confidence intervals around those small point estimates suggest that I also cannot rule out what would be considerably large enrollment effects in either direction. For example, the point estimate for the effect of waiver eligibility on CUNY enrollment is zero, but the confidence intervals range from negative 4.5 to positive 4.4 percentage points, which would represent substantial effects on enrollment.

B. Heterogeneity by Institution

Since waiver eligibility affects students likelihood of paying a deposit, one hypothesis could be that the imprecise enrollment estimates mask substantial variability across CUNY institutions. One potential source of college variance in waiver effects could be how salient the waiver is for admitted students or how easy it is for eligible students to access a waiver. As noted in table 1, I categorized schools by how easy it was to find information about the waiver on individual CUNY college websites and the steps students needed to take in order to obtain a waiver – whether students could check a box and submit documentation along with their intent-to-enroll, or if students needed to conduct separate transactions with the financial aid or bursar's office to verify eligibility for the waiver. In table 5, I explore the outcomes from table 4 separately by colleges I sort into each of those categories. There are no significant enrollment effects for any of the waiver effort subgroups, and the standard errors on each of the estimates are similarly large to the overall effects.

I also examined whether there were differential enrollment effects by sector, comparing the community and senior colleges. There are two hypotheses around why there might be differential effects by sector. First, research suggests that the enrollment plans of students intended to enroll at a community college are more susceptible to derailment than students intending to enroll in a four-year college (Castleman & Page, 2013). Second, students intending to enroll in a CUNY community college face a

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[51] To examine the extent to which any discontinuity in covariates at the threshold in 2016 might affect these results, I share results for just the 2017 and 2018 cohorts in Appendix Table 2, where the lack of a significant waiver-eligibility effect persists across outcomes.

lower tuition ($4,800 compared with $6,730 per year at the senior colleges), and the deposit represents a larger percent increase in total expenditures for those students (CUNY, n.d.). As with the overall sample and subsamples by waiver salience and institutional presence of deposit payment discontinuities, there are no significant discontinuities on enrollment outcomes at either senior or community colleges, as illustrated in Table 6. Perhaps counterintuitively, the point estimates for the community college sample trend negative, suggesting that waiver eligibility is associated with students being less likely to enroll at CUNY; however, these point estimates are similarly imprecise to other estimates reported here and I caution over interpretation of that trend.

C. Heterogeneity by Student Characteristics

I next turned to examine whether there were certain types of students who might be more affected by the waiver. I run models with interactions between waiver eligibility and three student characteristics – age, high school preparation, and first-generation status. These three variables may be correlated students' likelihood of being deterred from enrolling by a $100 deposit barrier. I hypothesize that older students may have access to fewer counseling supports to help them navigate the college application process than younger students still enrolled in high school who can turn to school or community agencies for assistance with the waiver application. I further hypothesize that students who have completed more high school credits are more engaged with the education process and may be more committed to college enrollment in the face of financial barriers.[52] Finally, I examine whether first generation students might have differential enrollment effects for similar reasons to the age hypothesis; namely that those students' parents may not have information about the college matriculation process to provide support navigating the deposit requirement and waiver option (Lareau, 2003; Pascarella, Pierson, Wolniak, & Terenzini, 2004). In table 7, I report on heterogeneous treatment effects on CUNY enrollment and overall college enrollment by student characteristics (bolding the coefficient of interest). There is a small and marginally

[52] More frequently-used variables in the education policy literature for evaluating differential effects by students' academic achievement are SAT or ACT scores; about half the CUNY analytic sample do not have recorded SAT scores, likely because they are not required for admission at the community colleges.

significant overall enrollment effect (1.1 percentage points) for waiver-eligible older students and a marginally significant effect (4.2 percentage points) on CUNY enrollment for waiver-eligible first-generation students. Given the multiple outcomes and subgroups tested in this paper, I caution over interpretation of these student sub-group findings. I run the heterogeneous treatment models for students excluding the 2016 cohort; the marginal significance found when including the full sample does not persist with the smaller sample, though the point estimates are similar.

D. Deposit Submission Rates

I do not find overall impacts of waiver eligibility, nor that the effect of waiver eligibility varied by the between-college hassle of obtaining a waiver or across student sub-groups. I now turn to exploring hypotheses to explain the null effect. First, I examine commitment deposit records to examine whether there appears to be a discontinuity in deposit payment rates around the threshold. In my data, I only observe students' eligibility for the waiver – whether they fall on either side of the EFC threshold. I do not observe actual waiver use for any students, though I do have data on students' deposit payments. Therefore, I assume that if an enrolled student does not have a deposit record, they used some type of waiver or negotiation process with the CUNY financial aid offices to receive an exemption from payment, and I measure whether there is a discontinuity in deposit payments across the $3,000 threshold for eligibility for a waiver.

An alternative explanation for a lack of a deposit record is poor record keeping and data retention in the CUNY system. To examine the extent to which this may be a concern, I group schools by whether there was a high or low merge rate between the admitted students file and the deposit records file. I categorized a school with a high merge rate if average deposit rate was at least half of the proportion of students in the sample above the waiver who would theoretically have to pay the deposit in a world with perfect compliance. For example, at Queens College, about 38 percent of the full sample had an EFC above the threshold, and the average deposit rate was 72 percent of the sample (suggesting low levels of waiver take-up, but likely not a case of not maintaining deposit records). However, about 58 percent of admitted students at Queensborough Community College had an EFC above the threshold, but the deposit rate was only 28 percent. For this group of low merge colleges, it is still not possible to distinguish with certainty between unobserved college policies that make depositing less likely and data issues. I run analyses examining differential deposit rates for the “high” and “low” merge rate schools, to examine whether those estimates varied by my confidence in the record keeping of the institution.

One reason for there to be no difference in enrollment effects is if no one eligible for the waiver ultimately takes advantage of the program or if just as many students above the waiver threshold end up finding a way to avoid the waiver through petitions to the financial aid offices. Across the three application cohorts, within the CCT bandwidth around the waiver threshold, 3,825 students enrolled at a CUNY college, and 1,992 of those enrollees, or about 52 percent, have a deposit payment on record; another 88 students paid a deposit but did not enroll in CUNY, suggesting a small amount of summer melt in this sample.[53] In figure 3, I plot commitment deposit rates against students’ EFC for the overall sample, as well as for the sub sample of students who enroll at CUNY to illustrate deposit rates among students who should (unless they received a waiver) have a deposit record on file.[54] There is a clear discontinuity in deposit payments at the threshold whether looking at the overall or enrolled sample, though larger for the enrolled sample. However, there is also noncompliance on both sides of the cutoff. For the overall sample, the deposit payment rate for the EFC bin right below (and including) the threshold is about 31 percent compared to 40 percent for the EFC bin just about the threshold. Not all students eligible for the waiver take advantage of the program and a substantial share of students with higher EFCs do not have deposit records. This suggests that we would expect a smaller effect of waiver eligibility on student enrollment outcomes given this substantial non-compliance with the policy. It also suggests that nearly a third of students eligible for a waiver ended up paying an extra $100 to attend CUNY, and that to

[53] There were 41 students who paid a deposit but did not enroll in 2016, 28 students who did so in 2017, and 19 who did so in 2018.

[54] Obviously, students who do not ever intend to enroll at CUNY would not pay a deposit, resulting in the overall deposit rates being lower in the overall sample than the conditional-on-enrollment sample.

the extent the institutions wish to enhance the welfare of their admitted students, they should consider increasing visibility of the program to increase take-up.

I then run my models with deposit payments as an outcome to capture the regression discontinuity estimate for the effect of waiver eligibility on deposit rates, first with the overall admitted sample, and then by college characteristics. I report the results in Table 8. I observe that students who are eligible for a waiver are 15.2 percentage points less likely to submit a deposit than students ineligible for the waiver are. The differences are similar at the community and senior colleges – 14.1 and 15.4 percentage points respectively. The discontinuities at “high” and “low” merge colleges are different, though their confidence intervals overlap. “High” merge colleges have a marginally significant 8.8 percentage point discontinuity in deposit payment rates at the threshold, while “low” merge colleges have a larger 16.9 percentage point discontinuity at the threshold. The hypothesis behind examining the “high” and “low” merge colleges is that we have better confidence in the overall data storage at “high” merge schools, and would attribute a discontinuity to true policy response rather than the possibility that there is a false discontinuity driven by poor data retention. The fact that the deposit discontinuity is smaller at high merge schools suggests perhaps that the overall deposit discontinuity overstates students’ actual policy response and provides suggestive evidence that noncompliance could be driving the lack of a waiver eligibility effect.

E. Robustness checks

While I find no statistically significant effects on enrollment outcomes, I also run several tests and versions of the analysis to estimate whether that estimate is sensitive to different sample criteria or model specifications.

First, I expand the sample to include students who submitted a FAFSA by June 1 to account for some students determining their EFC later than the deposit deadline who may have received an extension on their intent-to-enroll deadline. This increases the CCT bandwidth sample from 7,614 students to 9,150 students. In Appendix Table 4 I use this expanded sample and their EFC as of June 1 as the forcing variable to examine the effect of waiver eligibility on the enrollment outcomes in Table 5 as well as whether students paid a deposit. As with analyses using the May 1 EFC and sample, there are no precisely estimated enrollment effects, although there is a small but statistically significant 2.9 percentage point increase in deposit filing rates for students below the threshold. It is likely that students added into the FAFSA-filing sample between May and June are those who are less prepared to navigate the college application process and have fewer supports available as their 12th grade school year winds down to help them navigate the waiver process.

I use the CCT bandwidth calculated for the enrollment at CUNY outcome for my models to assess whether any findings are a function of the CCT window and whether the general point estimate trends hold for a narrower sample of individuals more directly affected by the policy (Angrist & Lavy 1999; Murnane & Willett 2010). Returning to the main sample of students who completed their FAFSA by May 1, I present in Appendix Table 5 the enrollment effect estimates for smaller bandwidths of +/- 1,100 (the main bandwidth employed in the main analysis), 1,000, 800, and 700. Across the increasingly smaller bandwidths and sample sizes, the only marginally significant coefficient on waiver eligibility is that students were more likely to enroll in a public college; given the lack of significance on this outcome in any other specification, I attribute that particular estimate to probabilistic chance.

VII. DISCUSSION

This study explores whether a small price increase – paying an enrollment or commitment deposit when indicating an intent-to-enroll the semester prior to matriculation – affects students' likelihood of college enrollment. To the extent that this small payment proves to be a psychological barrier or hassle factor that deters matriculation, colleges might pursue policies such as a waiver for lower-income students to alleviate the barrier the deposit faces.

In the CUNY context, I find that there is a substantial discontinuity in deposit payment rates at the waiver threshold – accepted students eligible for the deposit are about 15 percentage points less likely to have paid the $100 deposit than students with an EFC above the waiver eligibility cutoff do. This suggests that some students take advantage of their waiver eligibility and avoid the $100 payment the spring prior to enrollment. This varies by institution – there is a statistically significant discontinuity in deposit payments at eight of the 18 CUNY institutions; those discontinuities range from about 15 percentage points at Medgar Evers College to 31 percentage points at City College. There are some colleges with higher “merge” rates between the applicant and deposit data – where a higher share of enrolled students above the threshold where they would need to pay a deposit have higher rates of deposit payments. The discontinuities in deposit rates are smaller at these colleges, suggesting that record keeping may explain some but not all of the deposit discontinuity, and suggesting potentially larger shares of policy non-compliance that would theoretically depress the effect of waivers on student enrollment.

Despite the fact that a nontrivial percent of CUNY admitted students take advantage of their waiver eligibility, I do not detect an effect of the policy on student enrollment outcomes. Across several models, examining outcomes separately by sector, the salience of the waiver application on college websites, and whether there was a significant deposit rate discontinuity at the institution, I fail to reject the null hypothesis that waiver eligibility affects CUNY and overall college enrollment. While I fail to reject the null hypothesis, due to the large standard errors associated with these models, I also cannot rule out either a substantial negative or positive effect of waiver eligibility. At the upper end of the confidence intervals around my point estimates, the biggest potential effect of waiver eligibility would be a 4.4 percentage point increase in CUNY enrollment or a 2.4 percentage point increase in overall enrollment.

While I cannot conclusively assert a null effect of the waiver on student outcomes, there are several theoretical reasons to explain why this small price change might not affect enrollment decisions. College students face many barriers in their path to and through college, and many students do not succeed in completing necessary tasks that are necessary to matriculate. However, many explanations around the “summer melt” observed when college-intending students at the end of high school fail to show up on a college campus in the fall is attributed to a lack of guidance and support from mentors. The commitment deposit payment process occurs in May, while most K-12 schools are in session, and students can work with a school counselor or other individuals to access resources needed to either navigate a waiver or access funds to pay the deposit. In recent years, especially, schools have highlighted May 1 as "College Signing Day," with former First Lady Michelle Obama and nonprofits associated with her work organizing events throughout the country to raise excitement about the college-going process and the concrete step of sending in an intent-to-enroll form. Given this attention, there may be even more support systems in place to help students navigate the deposit process.

Research on other small price shifts in education have certainly found that policies that include small price shifts affect student enrollment, but those policies are often a package deal. Pallais' (2015) analysis of a \$6 subsidy to increase students' score sends of ACT scores to colleges examined a policy that included a subsidy, but also included a change in the default number of colleges that a student should send scores to. Students also encountered the ACT waiver earlier on in the college search process, and the act of sending an additional score changed the quality of institutions to which students applied, having ripple effects on their enrollment and persistence. Related to timing, the elasticity of students' demand for a given college is smaller once enrolled, with one study documenting students' hypothetical elasticity (in response to a survey, and therefore a likely upper bound of elasticity) around -0.12 for a \$500 fee increase, suggesting that as students progress through the college investment process, price has a smaller effect on demand, potentially reflecting a sunk cost bias (Bryan & Whipple, 1995). Perhaps students' demand for CUNY enrollment at the time they encounter the waiver is inelastic to price changes, while a waiver on a different college margin earlier on in the application may have a greater effect.

The CUNY deposit amount, waiver option, and waiver eligibility cutoffs are unique to this context. Although there is not a centralized source of information on different colleges' commitment deposit policies or waiver options, even within New York state colleges vary substantially. The CUNY deposit requirement is on the lower end - \$100, compared to \$500 at Columbia University (Columbia University, n.d.). Even compared to the State University of New York (SUNY) public colleges, CUNY deposits are lower than SUNY-Albany (\$275) and SUNY Buffalo (\$150) (SUNY-Albany, n.d.; SUNY-Buffalo, n.d.). While I observe no precisely estimated enrollment effect at CUNY, it remains possible that higher deposit requirements prove a greater barrier to enrollment at other colleges and that the implementation of a waiver policy could have a significant effect reducing that financial barrier. Similarly, while the CUNY system has a clear, well-advertised waiver eligibility policy, students must still exert some effort to obtain the waiver. If a college were to automatically exempt students from paying a deposit if they met certain criteria as a default policy, that waiver could have a greater effect on enrollment than a program that requires opting in (Johnson & Goldstein, 2003; Madrian & Shea, 2001).

While this study does not find evidence that waiver eligibility significantly affects college enrollment, the waiver program could still have a positive effect on other components of students' lives unobserved in the data. Students from a family of four with an adjusted gross income between $50,000-$60,000 with standard deductions and no assets would end up with an EFC around $3,000. While not an individual living at or near the federal poverty line, that income is still a little below the median household income in the United States (U.S. Census Bureau, 2018). These individuals likely benefit in the moment from the reduced financial outlay, and as noted above the existence of a waiver policy may signal to accepted students CUNY's commitment to easing students' financial burdens, endearing students to the institution and reducing the psychological stress associated with the college matriculation process (Stephens, Hamedani, & Destin, 2014; Walton & Cohen, 2011).

My findings also highlight the substantial share of students who are eligible for the waiver but do not take advantage of the program. About a third of students with an EFC just below the threshold who should have access to a waiver ended up paying the $100 deposit; at the lower tail of the CCT bandwidth, about 22 percent of students with an EFC around $1,900 paid the deposit even though they were eligible for a waiver. To the extent that CUNY believes that the waiver program offers a benefit to eligible students, the system or individual college admissions and financial aid counselors might work to promote the program more to admitted students.

Overall, my results show that the CUNY policy of offering students a waiver exempting them from paying the commitment deposit of $100 when accepting an admissions offer does result in a substantial share, though not all, of eligible students avoiding the fee. While I do not detect an effect of waiver eligibility on enrollment, I cannot rule out a positive or negative enrollment effect given the imprecision of my estimates, and even in the case where there is no true effect, there may be unobserved outcomes in the data that waiver eligibility affects, such as student affinity for the CUNY system and sense of support. As CUNY and other institutions consider the continuation or implementation of waiver programs, they should attend to the fact that many eligible students will not access the waiver in the face of various hassle factors such as verifying their EFC and requesting the waiver from the financial aid offices. If supporting more students with the waiver is an institution goal, institutions will likely have to engage in more proactive communication about the program or consider implementing default exemptions from deposit payments.

Table 2.1: Commitment Deposit Waiver Salience

Category	Institutions	Sample Language
Low Effort	Community Colleges: Guttman Community College; Kingsborough Community College Senior Colleges: College of Staten Island; John Jay College; New York City College of Technology	If you meet one of the following criteria, your deposit may be waived, however you must still complete and return the commitment form indicating the waiver request on it: Students who have filed a FAFSA and have an EFC of 3000 or less (please submit a copy of EFC page of the FAFSA with your commitment form) [Kingsborough Community College]
Medium Effort	Community Colleges: Borough of Manhattan Community College; LaGuardia Community College Senior Colleges: Hunter College; Medgar Evers College; York College	Request a commitment deposit waiver by contacting [Name redacted], director of admissions, at waiver@lagcc.cuny.edu. [LaGuardia Community College]
High Effort	Community Colleges: Bronx Community College; Hostos Community College; Queensborough Community College Senior Colleges: Baruch College; Brooklyn College; City College; Lehman College; Queens College	How Do I Pay and Submit My Deposit? By Mail: Mail this form along with your payment (check or money order only, NO CASH) to: Queensborough Community College, Office of Admissions, Administration Building, Room 210222-05 56thAve, Bayside, New York 11364 [Queensborough Community College]

Table 2.2: Sample Summary Statistics

	Fall 2016	Fall 2017	Fall 2018	Full Sample	Analytic Sample
Female	0.587	0.575	0.581	0.581	0.580
American Indian	0.004	0.004	0.004	0.004	0.004
Asian	0.222	0.210	0.207	0.213	0.152
Black	0.193	0.208	0.200	0.200	0.242
Hispanic	0.104	0.095	0.093	0.097	0.143
White	0.258	0.247	0.243	0.249	0.203
First Generation	0.482	0.481	0.485	0.483	0.463
Dependent student	0.938	0.939	0.943	0.940	0.975
Age	19	19	19	19	18
High school credits	17	17	17	17	17
EFC as reported by May 1	1021	999	1053	1025	2900
EFC as reported by June 1	1031	1004	1058	1031	2915
First EFC reported	1341	1214	1373	1308	3665
Final EFC reported	1167	1102	1135	1133	3246
Observations	27165	30756	31374	89295	7614

Notes: Average characteristics by the fall term students admitted for. EFC = expected family contribution. The EFC as reported by May 1 is the EFC on the last FAFSA a student submitted prior to May 1 of their application year; the EFC as reported by June 1 is the EFC from the last FAFSA a student submitted prior to June 1 of their application year; the First EFC reports is the EFC from the first FAFSA a student submitted during the FAFSA cycle associated with their application year and the Final EFC reported is the EFC from the last FAFSA a student submitted during the FAFSA cycle associated with their application year. High school credits measures the number of total courses across English, social studies, foreign language, mathematics, and science a student completed in high school; CUNY institutions vary in how many high school credits in each subject they require for admission.

Table 2.3: Continuity of Non-Outcome Variables

Variable	Sample	MSE-Optimal Bandwidth	RD Estimator	p-value	Number of Observations
Female	Pooled	1140	-0.008	0.710	7901
American Indian	Pooled	450	-0.002	0.568	3091
Asian	Pooled	1050	0.039	0.015	7266
Black	Pooled	800	-0.020	0.382	5492
Hispanic	Pooled	950	0.016	0.352	6556
White	Pooled	1030	-0.003	0.867	7115
First Generation	Pooled	800	0.046	0.091	5492
Dependent student	Pooled	870	0.012	0.127	5994
Age	Pooled	890	-0.103	0.332	6153
High school credits	Pooled	970	0.018	0.913	6705

Notes: Robust standard errors in parentheses. Includes application year and admitted college fixed effects. RD estimator column represents the difference at the $3,000 EFC threshold between waiver eligible and ineligible students on various characteristics.

Table 2.4: Effect of Waiver eligibility on students' enrollment

	Enrolled at CUNY	Enrolled in College	Enrolled Four-Year	Enrolled Public College	Enrolled in New York State
Waiver Eligible	-0.000	-0.007	-0.010	0.016	0.015
	(0.023)	(0.016)	(0.017)	(0.022)	(0.019)
Distance from Cutoff	0.000	0.000	-0.000	0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligibility*Distance	-0.000	-0.000	0.000	-0.000**	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	7614	7614	7614	7614	7614
R^2	0.024	0.088	0.330	0.026	0.054

Notes: Robust standard errors clustered on the running variable in parentheses. Models include application year and admitted college fixed effects. Enrolled at CUNY is a measure whether students in a given application year immediately enrolled at a CUNY institution the fall after application. Enrolled in college indicates whether students either enrolled at CUNY or at another institution reporting to the National Student Clearinghouse; enrollment at a four-year, public, or New York State institution indicates the type of institution at which a student enrolled. Models include application year and admitted college fixed effects as well as a vector of student covariates including age, race, sex, parental education, financial aid dependency, and high school academic preparation. Sample limited to students who completed the Free Application for Financial Aid (FAFSA) with a non-missing expected family contribution (EFC) value reported prior to May 1, the commitment deposit deadline.

~p<0.1, *p<0.05, **p<0.01, ***p<0.001

Table 2.5: Effect of Waiver eligibility on students' enrollment by waiver salience

	Enrolled at CUNY	Enrolled in College	Enrolled Four-Year	Enrolled Public College	Enrolled in New York State
Low Effort Waivers					
Waiver Eligible	-0.011	0.018	0.019	0.013	0.034
	(0.040)	(0.027)	(0.033)	(0.038)	(0.032)
Distance from Cutoff	-0.000	0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligibility*Distance	0.000	0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	2406	2406	2406	2406	2406
R^2	0.032	0.080	0.209	0.028	0.057
Medium Effort Waivers					
Waiver Eligible	0.025	-0.019	-0.031	0.043	0.001
	(0.040)	(0.030)	(0.031)	(0.039)	(0.034)
Distance from Cutoff	0.000	0.000	0.000	0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligibility*Distance	-0.000*	-0.000	-0.000	-0.000*	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	2530	2530	2530	2530	2530
R^2	0.017	0.084	0.360	0.019	0.055
High Effort Waivers					
Waiver Eligible	-0.013	-0.021	-0.013	-0.011	0.006
	(0.039)	(0.026)	(0.026)	(0.038)	(0.032)
Distance from Cutoff	-0.000	-0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligibility*Distance	-0.000	-0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	2678	2678	2678	2678	2678
R^2	0.029	0.120	0.398	0.038	0.067

Notes: Robust standard errors clustered on the running variable in parentheses. Models include application year and admitted college fixed effects. Enrolled at CUNY is a measure whether students in a given application year immediately enrolled at a CUNY institution the fall after application. Enrolled in college indicates whether students either enrolled at CUNY or at another institution reporting to the National Student Clearinghouse; enrollment at a four-year, public, or New York State institution indicates the type of institution at which a student enrolled. Models include application year and admitted college fixed effects as well as a vector of student covariates including age, race, sex, parental education, financial aid dependency, and high school academic preparation. Sample limited to students who completed the Free Application for Financial Aid (FAFSA) with a non-missing expected family contribution (EFC) value reported prior to May 1, the commitment deposit deadline.
~p<0.1, *p<0.05, **p<0.01, ***p<0.001

Table 2.6: Effect of Waiver eligibility on students' enrollment by sector

	Enrolled at CUNY	Enrolled in College	Enrolled Four-Year	Enrolled Public College	Enrolled in New York State
Admitted to Senior College					
Waiver Eligible	0.008	-0.004	-0.004	0.015	0.020
	(0.026)	(0.016)	(0.019)	(0.025)	(0.020)
Distance from Cutoff	0.000	-0.000	-0.000	0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligibility*Distance	-0.000	0.000	0.000	-0.000**	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	5818	5818	5818	5818	5818
R^2	0.023	0.052	0.105	0.020	0.041
Admitted to Community College					
Waiver Eligible	-0.031	-0.028	-0.038	0.014	-0.009
	(0.048)	(0.044)	(0.041)	(0.048)	(0.047)
Distance from Cutoff	-0.000	0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligibility*Distance	-0.000	-0.000	-0.000	-0.000	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1796	1796	1796	1796	1796
R^2	0.036	0.053	0.081	0.029	0.042

Notes: Robust standard errors clustered on the running variable in parentheses. Models include application year and admitted college fixed effects. Enrolled at CUNY is a measure whether students in a given application year immediately enrolled at a CUNY institution the fall after application. Enrolled in college indicates whether students either enrolled at CUNY or at another institution reporting to the National Student Clearinghouse; enrollment at a four-year, public, or New York State institution indicates the type of institution at which a student enrolled. Models include application year and admitted college fixed effects as well as a vector of student covariates including age, race, sex, parental education, financial aid dependency, and high school academic preparation. Sample limited to students who completed the Free Application for Financial Aid (FAFSA) with a non-missing expected family contribution (EFC) value reported prior to May 1, the commitment deposit deadline.
~p<0.1, *p<0.05, **p<0.01, ***p<0.001

Table 2.7: Heterogeneous Enrollment Effects by Student Characteristics

	Enroll CUNY	Enroll Any	Enroll CUNY	Enroll Any	Enroll CUNY	Enroll Any
Waiver Eligible	-0.049	-0.202~	0.019	0.002	-0.020	-0.005
	(0.113)	(0.110)	(0.057)	(0.045)	(0.025)	(0.018)
Distance from Cutoff	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligibility*Distance	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligibility*Age	0.003	0.011~				
	(0.006)	(0.006)				
Age	0.006	-0.013*				
	(0.006)	(0.005)				
Eligibility*High School Credits			-0.001	-0.001		
			(0.003)	(0.002)		
High School Credits			0.012***	0.014***		
			(0.002)	(0.002)		
Eligibility*First Generation					0.042~	-0.005
					(0.023)	(0.016)
First Generation					-0.013	-0.010
					(0.017)	(0.012)
Observations	7614	7614	7614	7614	7614	7614
R^2	0.025	0.089	0.024	0.088	0.025	0.088

Notes: Robust standard errors clustered on the running variable in parentheses. Models include application year and admitted college fixed effects. Enrolled at CUNY is a measure whether students in a given application year immediately enrolled at a CUNY institution the fall after application. Enrolled in college indicates whether students either enrolled at CUNY or at another institution reporting to the National Student Clearinghouse. Models include application year and admitted college fixed effects as well as a vector of student covariates including age, race, sex, parental education, financial aid dependency, and high school academic preparation. Sample limited to students who completed the Free Application for Financial Aid (FAFSA) with a non-missing expected family contribution (EFC) value reported prior to May 1, the commitment deposit deadline.
~p<0.1, *p<0.05, **p<0.01, ***p<0.001

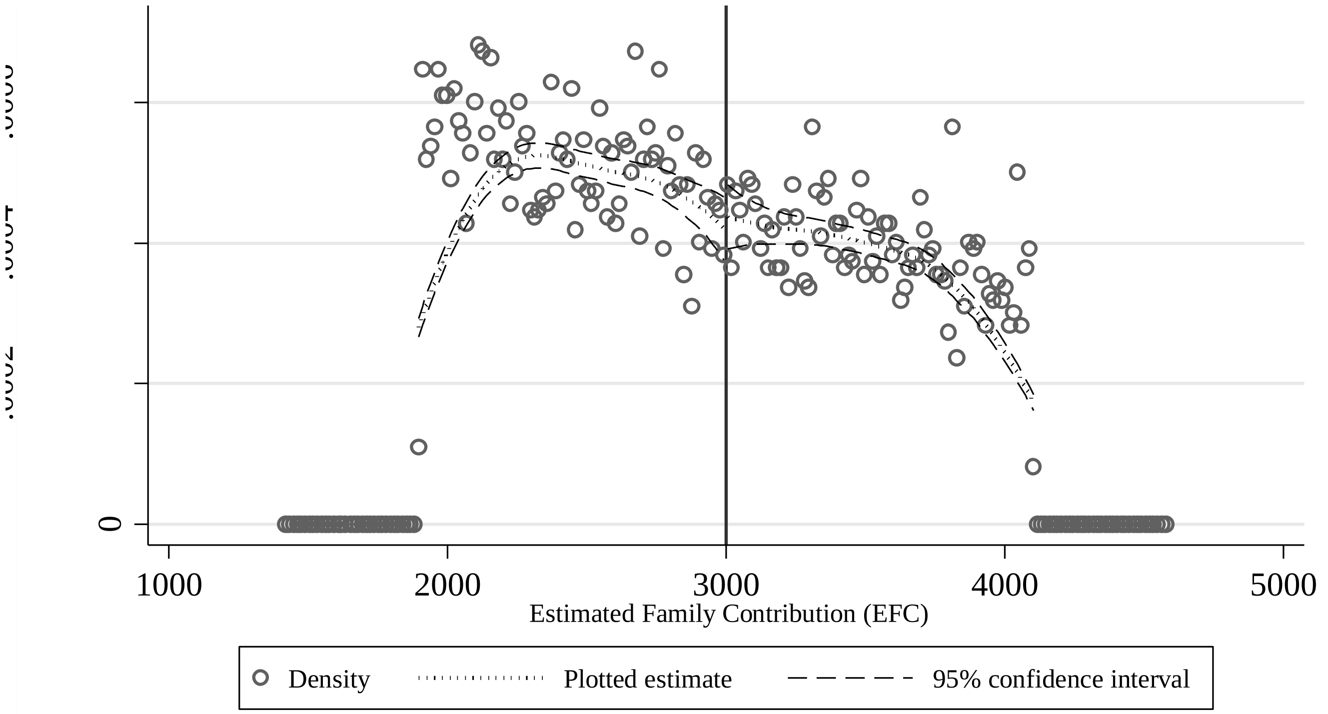
Table 2.8: Deposit Payment Rates

	Overall	Community Colleges	Senior Colleges	High Merge Colleges	Low Merge Colleges
Waiver Eligible	-0.152***	-0.141*	-0.154***	-0.088~	-0.169***
	(0.020)	(0.058)	(0.021)	(0.050)	(0.022)
Distance from Cutoff	0.000	0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligibility*Distance	-0.000	-0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	7614	968	6646	1498	6116
R^2	0.095	0.178	0.107	0.068	0.081

Notes: Robust standard errors clustered on the running variable in parentheses. Models include application year and admitted college fixed effects as well as a vector of student covariates including age, race, sex, parental education, financial aid dependency, and high school academic preparation. Sample limited to students who completed the Free Application for Financial Aid (FAFSA) with a non-missing expected family contribution (EFC) value reported prior to May 1, the commitment deposit deadline, and who enrolled at CUNY.

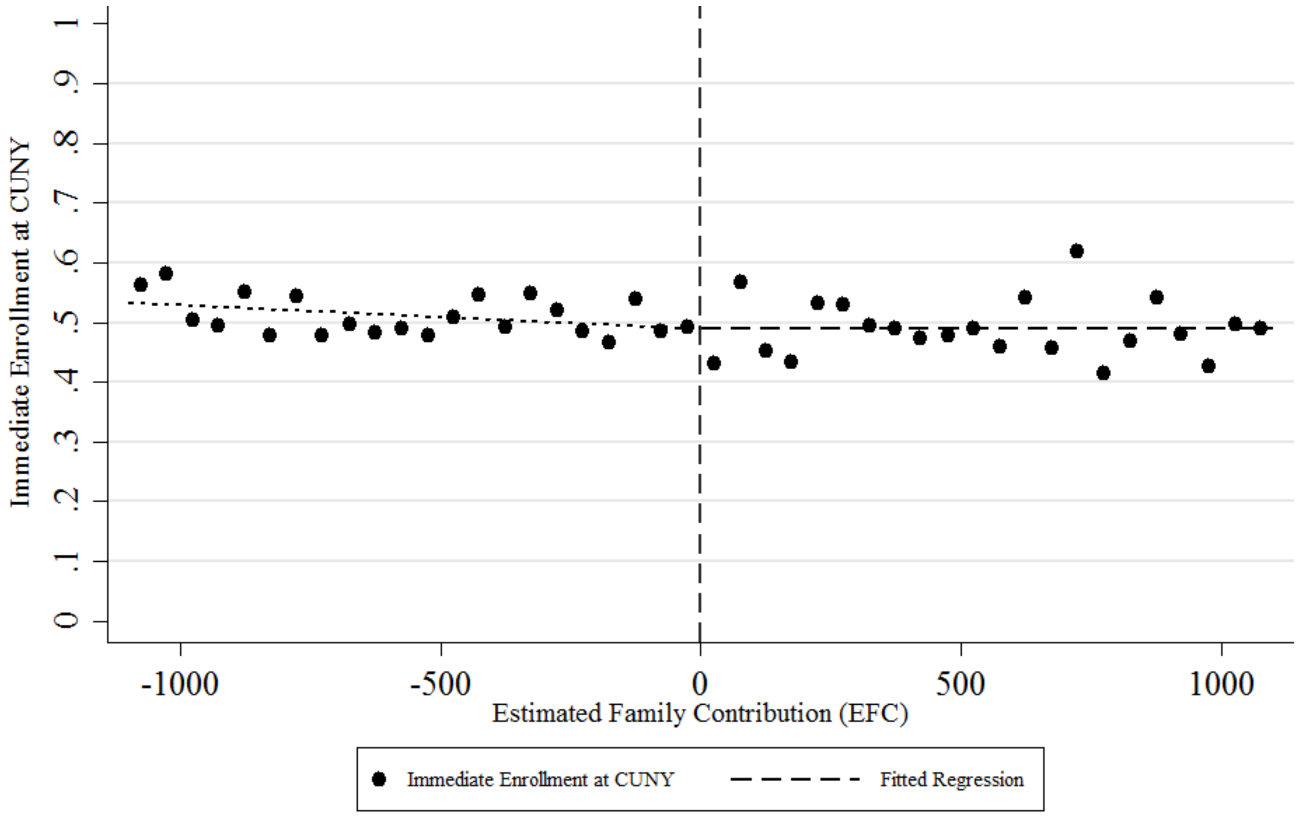
~p<0.1, *p<0.05, **p<0.01, ***p<0.001

Figure 2.1: McCrary Density Test



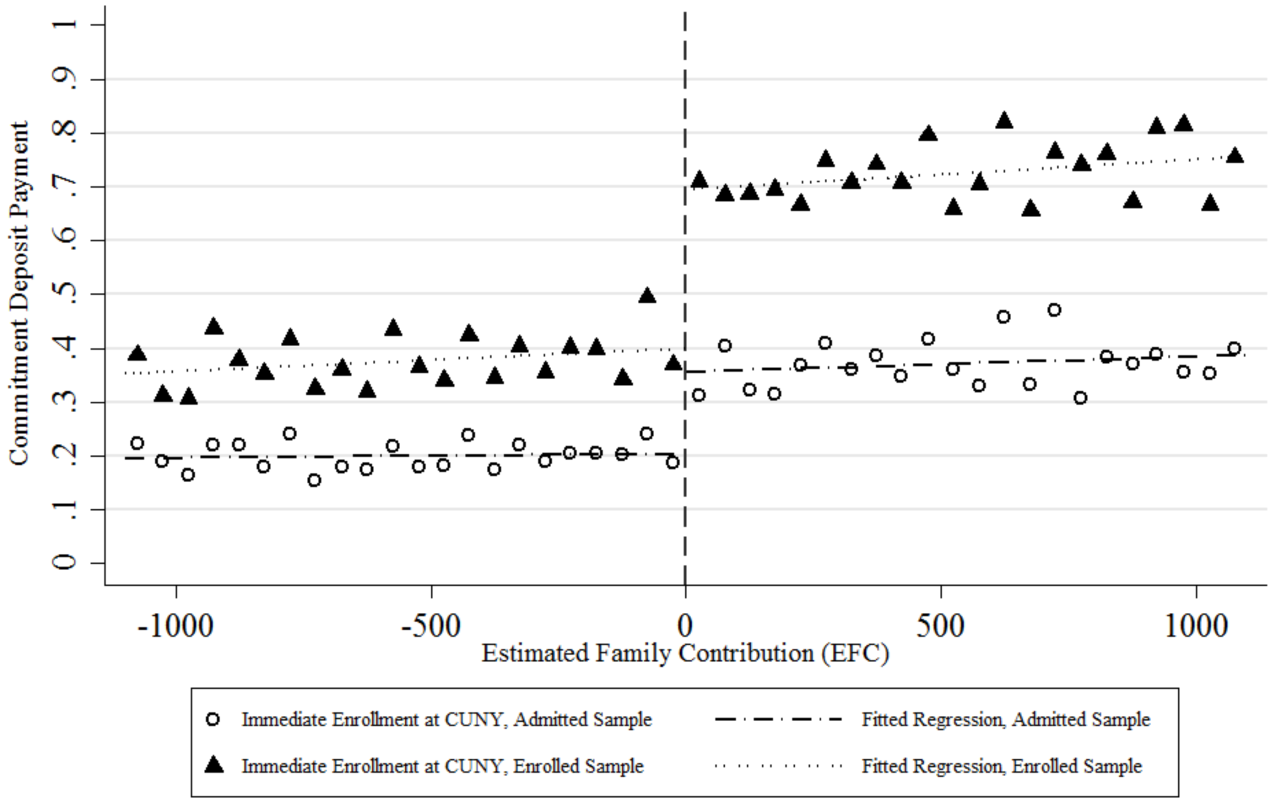
Note: Bin size = 14.43
Discontinuity estimate 0.048 (SE: 0.079)

Figure 2.2: Relationship between EFC and CUNY Enrollment



Notes: EFC is centered at the cutoff ($3,000). Each dot represents the average CUNY enrollment rate for EFC bins of $50. Limited to the CCT bandwidth of +/-$1,100 (N= 7614 observations)

Figure 2.3: Relationship between EFC and Deposit Payments



Notes: EFC is centered at the cutoff ($3,000). Each marker represents the average commitment deposit rate for EFC bins of $50. Limited to the CCT bandwidth of +/-$1,100 (N= 7614 observations in admitted sample; N= 3825 in enrolled sample)

CHAPTER 3

Nudging Students Beyond the FAFSA: The Impact of University Outreach on Financial Aid Behaviors and Outcomes

(with Benjamin Castleman, Zachary Sullivan, William D. Hartog, and Scott Miller)

Abstract

A growing body of research indicates that proactive outreach from high schools and college access organizations about college preparation tasks, and specifically focusing on completing the Free Application for Federal Student Aid (FAFSA), results in increased college enrollment. Comparatively less attention has been paid to the role of colleges and universities in this outreach and outreach relating to additional financial aid barriers that students face while applying to college, such as the CSS PROFILE form. In this article, we investigated, through an inter-university collaboration, the effect of sending targeted, semi-personalized text messages to students during the college application process about important financial aid deadlines, making salient the specific forms required and prompting students to plan specific times to complete these tasks. The intervention increased CSS PROFILE filing by 3.1-4.3 percentage points, where the estimates and their significance varied depending on the comparison group. Impacts on student enrollment did not accompany these filing impacts. Results from our collaboration support the idea that colleges and universities have an important role to play in outreach to applicants relating to important financial aid tasks. The paper includes a discussion of the promises and challenges of this outreach with recommendations for practitioners.

I. INTRODUCTION

For over a decade, researchers have demonstrated that the complexity of the Free Application for Federal Student Aid (FAFSA) can deter otherwise college-ready students from enrolling or succeeding in higher education. Approximately one in 10 college students who would be eligible for need-based federal financial aid fails to file the FAFSA. Even among college freshmen who received a Pell Grant and who are in good academic standing, nearly one in six fails to successfully refile the FAFSA for their second year in college (King, 2004; Bird & Castleman, 2016). A growing body of research demonstrates that the financial challenges and anxieties associated with poverty limit the cognitive bandwidth that families can devote to complex tasks like completing the FAFSA (Castleman, 2015; Dynarski & Scott-Clayton, 2006; Mullainathan & Shafir, 2013; Ross, White, Wright, & Knapp, 2013). Barriers associated with the FAFSA, and the financial aid application process more broadly, may contribute to long-running socioeconomic inequalities in college access and success—disparities that persist even after controlling for students' academic achievement (Bailey & Dynarski, 2012; Belley & Lochner, 2007; Long & Mabel, 2012).

In recent years, there has been substantial policy investment to provide lower-income students and families with additional information and assistance throughout the financial aid process. These initiatives include both governmental efforts like the U.S. Department of Education FAFSA Completion Project, which provides school districts with real-time information about which students have completed the FAFSA, and privately-funded efforts like College Goal Sunday, which provides students in most states with free FAFSA completion assistance.[55]

Researchers have demonstrated, through randomized, controlled trials, that low-cost strategies to support students and families with financial aid filing can also generate substantial improvements in college entry and persistence. In the seminal study, Bettinger, Long, Oreopoulos, and Sanbonmatsu (2012) integrated FAFSA completion assistance into the income tax preparation process at H&R Block. Helping

[55] For more information on these programs, see http://www.ed.gov/blog/2012/05/ed-announces-fafsa-completion-project-expansion/ and http://www.collegegoalsundayusa.org/pages/about.aspx

students fill out the FAFSA added less than 10 minutes to the income tax preparation time for families, but this assistance increased the share of treated students who completed at least two years of college by almost 30%. Castleman and Page (2015) demonstrated that sending personalized text message reminders about the key financial aid and procedural tasks students must complete during the summer after high school can increase the share of college-intending high school graduates who successfully matriculate in college. Working with the Common Application organization, researchers also found that sending financial aid planning prompt nudges at scale to over 450,000 high school seniors increased college enrollment for all students with a larger effect for first-generation college students (Bird, Castleman, Goodman, & Lamberton, 2017).

These financial aid filing interventions draw on insights from behavioral science research to develop outreach that overcomes the common behavior barriers students and families face during the college search and funding process. Many individuals, when faced with complex decisions and processes, tend to avoid these hassles and delay action, which may result in failing to complete important tasks, such as completing a financial aid form (Iyengar & Lepper, 2000; Madrian & Shea, 2001; Dynarski & Scott-Clayton, 2006). Given limited attention and a tendency to focus on the present, individuals may struggle to plan ahead or understand the importance of completing various financial aid forms on their long-term financial well-being (Karlan, McConnell, Mullainathan & Zinman, 2010; *Milkman, Beshears, Choi, Laibson, & Madrian* , 2012; Rogers, Milkman, John, & Norton, 2015; Bird, Castleman, Goodman, & Lamberton, ,, 2017).

Low-income students and families often lack access to professional advisors and mentors who have experience with the complex college and financial aid application processes and who can help navigate forms and timelines (Castleman & Page, 2014; Lareau, 2003; Ross et al., 2013). Recognizing gaps in access to “college knowledge” between low-income students and their more advantaged peers and the tendency for all individuals to, in the face of complexity, engage in some of the behavioral responses detailed above, interventions to date have focused on proven behaviorally informed strategies to increase financial aid filing. These strategies include prompting action through timely reminders, simplifying complex concepts and processes by changing the presentation of information, reducing hassles by making it easier for students and families to connect with experts, and personalizing information to make it more salient.

Much of the existing intervention work has focused on initial FAFSA completion, yet a growing body of work demonstrates that lesser-known aspects of financial aid policy can also pose barriers to low-income students receiving financial aid. For instance, most states have priority filing deadlines for allocating state-based financial aid to students. These priority deadlines are often not actively communicated to students and families, and frequently change over time. Bird (2015) shows that moving priority deadlines earlier in the year results in a more regressive distribution of aid, with lower-income students less likely to receive aid dollars that are targeted for financially needy students.

Another under-studied potential barrier in the financial aid process is the CSS PROFILE, a supplementary financial aid application administered by the College Board that almost 300 institutions require in addition to the FAFSA. The CSS PROFILE has not received nearly the public attention that the FAFSA has, yet at some institutions students are required to submit both the FAFSA and the CSS PROFILE in advance of priority filing deadlines to maximize the amount of financial aid they receive. Failure to submit both forms before the deadline can result in students foregoing thousands or even tens of thousands of dollars in grant aid. Unlike the FAFSA, there is a fee to complete the CSS PROFILE, and as a result, the College Board does not recommend students complete the form unless their college requires it. While the College Board provides a fee waiver to eligible students to cover submission at up to nine institutions, students must apply for the waiver, creating another obstacle to financial aid submission at certain institutions. Therefore, students face uncertainty about whether and when to complete the form, with the added barrier of paying a fee in order to process their paperwork fully.

Furthermore, while most colleges and universities include information about financial aid in their application materials, in acceptance packets, and on their websites, there is little rigorous research that investigates the efficacy of this communication at increasing the share of students who successfully apply for financial aid. The literature also lacks studies that evaluate more innovative approaches colleges have pursued to encourage students to complete the FAFSA and/or CSS PROFILE applications.

One intervention at Arizona State University (ideas42, 2015) found that sending emails to students and parents that emphasized FAFSA priority deadlines and encouraged students to set aside time to complete the FAFSA resulted in substantially higher FAFSA filing rates. In their study, half of treated students refiled the FAFSA compared to 29% of students receiving standard emails and no parent emails. We know of no other rigorously evaluated FAFSA completion interventions designed and implemented by individual colleges and universities. This is reflective of a broader trend in which most college access initiatives are pursued by the high schools, community-based organizations, and states in which students completed their secondary education, rather than by the higher education sector to which the students are aspiring. This disparity in effort to improve college access and success has prompted increasing calls to colleges and universities to play a more active role in supporting low-income students to and through college, such as President Obama's 2014 White House College Opportunity Summit. In this paper, we report on a novel initiative by the University of Virginia (UVA) to support applicants from Virginia to complete the FAFSA and CSS PROFILE in advance of UVA's March 1 priority filing deadline. This deadline has important implications for students' eventual aid awards: students who complete both forms in advance of March 1 are eligible to receive additional institutional grant aid compared with students who file after March 1. During the winter and early spring of 2016, the UVA admissions office sent more than 3,400 early action admitted students and regular decision applicants in the state a series of four text messages encouraging them to send in their financial aid forms before the deadline. The texts were semi-personalized to the student and emphasized the financial benefit to filing their forms before March 1.

Due to our inability to randomize receipt of the text campaign, we use a difference-in-differences estimation approach to evaluate the impact of this program. Specifically, we exploit variation between the treatment and control group in exposure to the text campaign, and compare changes over time in financial aid behaviors between students who were eligible and ineligible for the campaign. While UVA only texted students applying in 2016, we identified students applying in 2015 who would have received the texts had the campaign been enacted.

Our paper makes two primary contributions to the existing literature. First, we focus on an under-studied aspect of the financial aid process, the CSS PROFILE, and find suggestive evidence that universities can support students to complete these processes through a low-cost, highly scalable outreach campaign. Second, we highlight a role for higher education institutions to increase access to college by making a more proactive effort in reaching out to students about financial aid. Particularly given their access to real-time information about the status of students' financial aid applications, colleges and universities are well positioned to provide students with salient, timely nudges as they navigate what remains a highly complex financial aid application process.

To preview our results, we find that the short texting campaign increased the share of in-state admitted students who successfully completed the CSS PROFILE by the March 1 deadline by 3.1 to 4.3 percentage points, where the estimates and their significance varies depending on the comparison group used. While imprecise, we find that effects were larger for early action applicants, who were notified of their acceptance to UVA prior to the campaign. The difference could reflect the increased salience of the benefit to applying for aid when students know it will result in a financial aid offer. The campaign did not, however, increase the share of students matriculating to UVA or a similarly selective institution. We are unable to examine impacts on the generosity of financial aid packages, which could help explain the null enrollment finding.

The remainder of our paper is structured as follows. First, we provide additional background about UVA's financial aid initiatives and the design of the text messaging campaign. Next, we describe the data we use in our analysis before describing our empirical strategy. We then present our results, and finally conclude with a discussion of the importance of our findings and direction for future research and policy.

II. BACKGROUND AND INTERVENTION DESIGN

In 2004, the University of Virginia launched its flagship financial aid program, AccessUVa, to ensure that any student admitted to the university could afford to attend.[56] To be eligible for AccessUVa, a student must submit two financial aid applications, the FAFSA and CSS PROFILE, before the March 1 priority deadline.[57] Under AccessUVa, students receive a combination of grants, need-based loans, and work-study to meet their financial need.[58] Students who only submit the FAFSA, or who miss the priority deadline, are only considered for federal need-based student aid, which for the lowest-income students results in as much as a $20,000 reduction in annual grant aid offered.[59] In the year prior to our study, among the 20% of admitted FAFSA filers who failed to file the CSS PROFILE, 20% would have received at least $10,000 more in grant aid by filing the CSS PROFILE.

During the 2013-14 academic year, UVA President Teresa Sullivan convened a presidential task force to examine the university's existing policies and communication on access and affordability for socioeconomically disadvantaged students. This task force also sought to identify opportunities for more proactive and comprehensive efforts to communicate with lower-income prospective students about the financial aid resources available to them at the university.

One of the commitments that emerged from the task force was to use a broader range of communications strategies to reach students, recognizing that traditional means of communication (e.g., email or postal mail) might not be having the desired reach to economically-disadvantaged communities. Opportunities to integrate a personalized text messaging campaign into its outreach portfolio particularly interested the UVA admissions office, given a growing body of evidence that sending students and families text messages with simplified information, encouragement, and access to professional assistance led to improved outcomes on various educational measures. These interventions have proven effective at

[56] For more details visit http://www.virginia.edu/accessuva/learn.html.
[57] The CSS PROFILE is run by the College Board, and is required for more than240 colleges, universities, and scholarships. Unlike the FAFSA, the PROFILE can contain questions specific to a school, requires a minimum student contribution, and uses a different methodology to determine financial need.
[58] Demonstrated need is equal to the cost of attendance minus EFC. Loan offers are capped at $3,500/year for the lowest-income students and $7,000/year for all other students.
[59] Author's calculation based on a student with zero EFC and income less than 200% of the federal poverty line.

improving many student outcomes, from improved cognitive performance for preschool age children to increased high school GPAs and improved college entry and persistence rates among adolescents (Bergman, 2013; York & Loeb, 2014; Castleman & Page, 2015; Castleman & Page, 2016; Page, Castleman, & Meyer, 2016).

We collaborated with the admissions office to design a texting campaign specifically aimed at encouraging early action admits and regular decision admits from Virginia to file their financial aid applications prior to the priority deadline. The texting campaign consisted of four messages sent to students between February 16, 2016, and February 26, 2016. The messages focused on conveying to the students the financial benefits of filing the FAFSA and CSS PROFILE in advance of the March 1 deadline. Drawing on prior studies, the messages leveraged behavioral principles to encourage students to work on the FAFSA and the CSS PROFILE, rather than put it off and potentially miss the March 1 deadline. For instance, one of the messages provided students with a concrete planning prompt by encouraging them to "set aside a couple hours [this week] to work on these forms" (Rogers et al., 2015). Since the campaign started before UVA made its regular admission decisions, early action students received slightly different messages because they had already been notified of their acceptance. Appendix A presents the full text message content and dates sent. The messages also encouraged early action students to respond to the texts and ask questions of a UVA financial aid counselor.

The Common Application for admissions asks students whether they intend to apply for financial aid, whether the colleges they apply to can contact them, and to provide a cell phone number. The application defaults students into receiving information from any of the colleges to which they have applied. Using this information, UVA considered students "text eligible" if they intended to apply for financial aid, opted to receive messages from all the schools they applied to, and provided a phone number. Around 65% of in-state applicants defaulted to receiving text messages from the schools to which they applied, and 62% of in-state applicants indicated an interest in financial aid. Just over 40% of in-state applicants were text eligible each year. Throughout February 2016, UVA sent messages to all in-state early action admitted students and regular decision applicants who met the eligibility criteria.

UVA had piloted the text message campaign in February 2015 with 58 high schools in the state identified as serving a predominantly low-income population. President Sullivan also sent principals at the schools personalized letters encouraging them to have their students apply to UVA. Because of the pilot rollout, we had the necessary pre-treatment eligibility information for students at non-targeted schools to run a difference-in-differences analysis of the 2016 intervention. We excluded students from the pilot schools, since eligible students received the treatment in both years. In Appendix Table A1, we show how average applicant characteristics at these pilot schools compared to the characteristics of applicants who attended high schools included in our analytic sample. Pilot schools tended to have lower rates of application to UVA, and those applicants were more likely to identify as Black or Hispanic. However, the pilot and rollout schools are comparable, with similar graduation rates, enrollment, and student/counselor ratios.

In addition to examining student enrollment and financial aid outcomes, we also examined the content of students' text message interactions throughout the intervention (see Appendix A). Due to staffing limitations, UVA administration decided to encourage only early action students to respond to texts with questions ("Text back if you have questions or need help!"). Nevertheless, both early action and regular decision students frequently responded to the automatic messages, and we examined the frequency and content of student replies for all students and some of the in-depth interactions that occurred between the early action students and financial aid administrators.

The texts sent to early action students explicitly asked those students to reply after the second round of texts to let the financial aid office know whether they had "completed" their financial aid forms or if they had "not yet" had a chance to complete the forms. Likely because of that explicit request for a response and other language encouraging students to write back with questions, the majority (67%) of treated early action students sent at least one text to UVA during the intervention. Among students who sent at least one text, the average number of texts was about 1.36 per student, with about 80% of texters only sending one response (although one very engaged student sent 17 text messages over the course of the campaign).

Among the students who sent at least one text, about 21% were directly replying to the prompt, stating they had completed their financial aid forms. About 10% of the students who texted back were asking a question[60], and many students had rich interactions with the UVA team. For example, one early action student had questions about how work-study would pay out and how he would know if he had received a work-study award; another student had questions about whether to submit W-2 forms or summaries to finalize financial aid. These questions suggest that there are very real knowledge gaps among prospective students around the financial aid process, and that students trust using text messaging to gather clarifying information.

Although regular decision students did not receive a prompt asking them to reply to the text messages they received, many still did so. About 21% of regular decision students sent a text during the intervention. When they did so, they received a message stating, "These messages are delivered through an automated system. We cannot respond to individuals. If you need assistance please email uvaapplicationinfo@virginia.edu." Given this clarifying message after a student's first text, it is unsurprising that about 93% of students who ever sent a text only sent one. Skimming student questions, however, there is evidence that regular decision applicants would have benefited from two-way communication similar to the communication received by the early action admitted students. About 15% of the texts regular decision students sent were coded as a question. Their questions included "What's the CSS?" or "If I don't fill out the CSS profile does that mean that [I] won't get any financial aid at all?" As resources allow, enabling two-way communication for all students would likely be beneficial to address such questions.

III. DATA

We received student-level data from UVA for the cohorts applying in 2015 and 2016. Our dataset contained background information students provided on their application, including gender, race, high

[60] We coded a student reply as a "question" if the student included a question mark in their text; therefore, this count may underestimate the number of true questions if students did not use punctuation in their text message communications.

school achievement (GPA and standardized test scores), what high school they attended, and whether they applied early action. Because of how UVA stores financial aid application data, we could only access CSS PROFILE filing data for admitted students, and we focused our analysis on the admitted pool. Although UVA and the research team would have liked to examine FAFSA filing and financial aid packages to better understand how filing relates to aid receipt, based on a mutual discussion and review of FSA regulations and U.S. Department of Education guidance on using student data for evaluation, the research partnership team determined we could not access these outcomes at the time of our analysis.

UVA also provided enrollment data for all applicants by matching our sample to the National Student Clearinghouse, which we merged with Barron's college selectivity rankings. Barron's Educational Series releases an annual directory of every accredited four-year college and university in the United States, which includes a selectivity ranking of each institution ranging from "noncompetitive" to "most competitive" (Barron's, 2017).

Our main analytic sample included about 8,000 first-year Virginia residents, across two cohorts, who were admitted early action or regular decision.[61] We defined students as eligible for the text messages if they indicated on their application that they planned to apply for financial aid and consented to receive text messages. In the treatment year, 2016, we identified 1,652 students as text-eligible. Our analysis and results used two different definitions of ineligible students for our comparison group: (a) students who expressed an interest in need-based financial aid but opted out from receiving text messages (ineligible due to "opt-out"), and (b) students in the first comparison group plus students who consented to receive text messages but did not express an interest in need-based financial aid (ineligible due to "any reason"). We discuss the validity of each comparison group in the following section.

In addition to student-level applicant data from UVA, we compiled school-level data from the Virginia Department of Education (VADOED) and the Federal Student Aid (FSA) and Common Core of Data (CCD) offices of the U.S. Department of Education. The VADOED data files include information on

[61] We dropped all transfer applicants because they were not eligible to receive text messages.

student enrollment and demographics, including percent of free or reduced-price lunch eligible students, and school graduation rates. The FSA data include the number of students at each high school filing the FAFSA in prior years. The CCD data include additional school-level characteristics such as the number of counselors at each high school.

Table 1 contains mean student characteristics by treatment status over the entire sample period and includes admitted students who applied via regular decision or early action. Slightly less than 60% of admitted students were female. The average SAT (math plus verbal) score was slightly lower among text-eligible students relative to text-ineligible students, 1360 and 1380 respectively, which only results in a difference of one percentile point in the national percentile rankings. Roughly 70% of admitted students were White or Asian, 15% identified as an underrepresented minority (Black or Hispanic), and the remaining balance did not report a race. Relative to the ineligible-for-any-reason sample, the text-eligible and opt-out samples were slightly more likely to be underrepresented minorities and less likely to be White or Asian.

Table 1 also shows how we constructed the text-eligible and text-ineligible groups, as well as the mean values of our main outcomes. The treatment indicators show that all text-eligible students opted in to the text campaign and intended to apply for aid. The opt-out sample all also intended to apply for aid, but did not opt in to the texts. Only 35% of the ineligible-for-any-reason sample intended to apply for aid, and 42% opted in to the text campaign. The difference in filing rate is consistent with the stated difference in intention to apply for aid. Slightly over 80% of students from the text-eligible and opt-out samples submitted the CSS PROFILE, while less than 50% of the students from the ineligible-for-any-reason group filed the CSS PROFILE. The lower filing rate among the ineligible-for-any-reason sample did not rule them out as a valid control group, but it raised concerns, which we discuss below.

IV. EMPIRICAL STRATEGY

To examine the effects of the financial aid text messaging campaign on financial aid filing behavior, we exploited variation between the treatment and control group in exposure to the text campaign.

Specifically, the treatment group was only texted in the post-period (spring 2016), while the control group was never texted. Using a difference-in-differences (DiD) empirical strategy, we compared the change in the filing rate between the pre- and post-period (spring 2015 compared to spring 2016) for our treatment group (text-eligible students) to the change in filing rate for our control group (text-ineligible students).

Our main difference-in-differences specification was as follows:

$$Y_{ist} = \beta_1 Text\ Eligible_{it} * Post_t + \beta_2 Text\ Eligible_{it} + \beta_3 Post_t + \varepsilon_{it} \quad (1)$$

Where Y_{it} is a financial aid filing or enrollment outcome for student i at time t. $Eligible_{it}$ is an indicator for student text eligibility and controls for constant difference between eligible and ineligible students. $Post_t$ is an indicator for the year when UVA initiated the texting campaign and controls for constant differences between the cohorts applying to UVA in 2015 and 2016. We also ran specifications including student-level characteristics (i.e., gender, race, SAT score), which did not substantially change our results.

Our coefficient of interest, β_1, represents the effect of receiving the text campaign on whether students applied for financial aid at UVA prior to the priority deadline, and whether they matriculated to UVA. Since we could not observe who actually opened and read the text reminders, we estimated the intent-to-treat (ITT) effect of being sent a text message reminder, rather than the effect of the reminder. From a policy perspective, the ITT is most relevant because an institution cannot mandate that students open their text messages.

The main assumption under which β_1 identifies the effect of the text reminders is that the difference in filing rate between ineligible students in 2015 and 2016 is a good counterfactual for how much filing rates would have increased for eligible students over the same period in the absence of the intervention. Our choice of comparison group presents a tradeoff between precision and bias. The opt-out sample provided a natural comparison group because they also all intended to apply for aid, looked similar on background characteristics to the text-eligible sample, and had a nearly identical financial aid filing rate in the pre-period. Since this sample opted out of being texted by all schools to which they applied, we do not believe the decision to opt out reflects a lack of interest in attending UVA. The opt-out sample was, however, much smaller than the pool of students who were ineligible for any reason. Using the ineligible-for-any-reason group as a comparison would likely bias our results in the positive direction because a lower share intended to apply for aid, which could mean their trend in filing was different than that of the text-eligible students.

One way to test our assumption would be to run a placebo test and compare the trends in outcomes between the eligible and ineligible groups using multiple years of pre-intervention data. If the ineligible students are a valid counterfactual, then the eligible and ineligible student outcomes should be trending similarly prior to the intervention. Unfortunately, we only had access to data from the year prior to the intervention. Ultimately, we relied on the opt-out sample as our main control group and used the ineligible-for-any-reason group for robustness, but we acknowledge the potential bias introduced by using this group.

We also assumed that the delivery of the text campaign was the only policy changing differentially for the text-eligible students between the pre- and post-cohorts. If other university policies changed simultaneously to make text-eligible students more likely to enroll at UVA, then we could not separate the impact of the text campaign from another policy change. This should not be a concern, because eligibility for campaign did not affect how students were treated in the admissions process or how much aid they were offered if accepted.

Lastly, treatment spillover between text-eligible and text-ineligible students presented a potential threat to identification. However, spillovers would bias our results toward finding no effect, since ineligible students would also be more likely to file for financial aid because of the text campaign. We carried out our analysis assuming ineligible students were unaffected by the texts sent to their eligible schoolmates.

To provide support for the main identifying assumption, we tested for any changes in the observable student characteristics for eligible students over the pre- and post-period relative to ineligible students. If our identifying assumption is true, then exposure to the text campaign should be the only change between eligible and ineligible students. To test for compositional changes, we ran our DiD model without any demographic controls, and replaced the outcome with an observable demographic characteristic. Any statistically significant, observable differences suggest there could also be unobservable compositional differences between the pre- and post-period.

Table 2 reports the β_1 coefficient from these models for all admitted students and then separately for early action and regular decision admitted students, using both the full ineligible group and the subsample of students ineligible for treatment only because they opted out of receiving messages. We observed two consistent, statistically significant changes in student composition: overall, text-eligible, admitted students in 2016 were more likely to be White and less likely to leave the race category on their application blank, which appears to be driven by early action students. The compositional differences were larger using the opt-out sample; for this reason, we used the ineligible-for-any-reason sample as a control group. To account for changes in relative composition of treatment students, we present results from models including individual-level covariates (i.e., student gender, race, SAT score).[62]

The change in composition of students across cohorts was likely due to the increase in the opt-in rate, and thus the size of our treatment-eligible group, between 2015 and 2016. During 2015, about 60% of applicants (62% of admitted students) defaulted into receiving text messages from colleges and universities. In 2016, the share of students who opted in receive messages increased to about 69%–70% of applicants and admitted students. As far as we can ascertain, the language for that question on the Common Application did not change between application cycles. We surmise that this increase likely reflects a time trend of growing trust of text messages for official purposes such as communication with a college or university.

V. RESULTS

Our main financial aid filing outcomes are CSS PROFILE filing and on-time filing, and our enrollment outcomes include whether a student enrolled at UVA and whether the student enrolled at a selective college (as defined as an institution being in one of the top two Barron's selectivity categories).[63] We examined overall selective college enrollment because the text campaign could have caused students to file for financial aid at other colleges as well as at UVA, making all selective colleges more affordable

[62] We determined which student-level covariates to include based on availability across the two cohorts of students.
[63] This includes schools that Barron's ranks as "most competitive" or "highly competitive plus." UVA is a Barron's 1, "most competitive" institution.

and increasing the likelihood of selective college enrollment. As noted earlier, we could only examine CSS PROFILE filing among admitted students, and we did not observe financial aid offers to link filing behavior with award amounts. This lack of information limited our ability to explore the mechanisms through which effects on filing behavior would translate to enrollment outcomes.

Our main regression results appear in Table 3. Using the opt-out sample as our main comparison group, the text campaign increased the CSS PROFILE filing rate by a statistically insignificant 3.4 percentage points and on-time filing by 3.1 percentage points. For robustness, we used the ineligible-for-any-reason comparison group, and found the impact on ever filing was 5 percentage points and the effect on on-time filing was 4.3 percentage points, both of which were statistically significant. However, as we discussed in the previous section, estimates using this sample could be biased upwards. The impacts on overall filing were slightly larger, suggesting that the text campaign was more effective at raising awareness about the benefit of completing the CSS PROFILE than it was at nudging students to submit the CSS PROFILE prior to the deadline. Across both samples, we found that the text campaign did not impact whether a student enrolled at UVA or at any selective institution.

We repeated our main analysis separately for early action and regular decision applicants and report findings in Table 4. While our sample size with subsamples limited our ability to detect effects, the treatment point estimates on filing were larger among early action students. There are two potential explanations for this difference. As noted earlier, early action students received slightly different messages than regular decision students, and the differences may have led to differences in their effectiveness. However, we expect the differential responsiveness relates more with students' knowledge of their admission status. While we restricted our analysis to admitted students due to data limitations, early action students knew they had been admitted to UVA when they received messages, while regular decision students had not yet been notified. We hypothesize that students are more responsive to outreach about specific financial aid tasks when they have certainty that completing the task is necessary (as opposed to regular decision students who may or may not need to complete the CSS PROFILE depending on what institution they attend).

We were also interested in examining whether treatment effects varied by student characteristics. In Table 5, we present analyses on the subgroups of above- and below-median SAT scorers (the median score was 1370) and comparing students by underrepresented minority (URM) status.[64] We did not see significant effects for either group, although point estimates were slightly higher for students with above-median SAT scores. Within this sample, above- and below-median SAT scores both represent very high-achieving students, and the two groups may not be substantially different from each other, making a lack of difference in point estimates unsurprising.

In Table 5, we do observe differential responsiveness to the treatment based on student race. We found zero-to-negative and statistically insignificant treatment effects for underrepresented minority students, but treated White and Asian students (non-URM) were 5.3 percentage points more likely to complete the CSS PROFILE, and they were 4.3 percentage points more likely to do so by the March 1 deadline (although the on-time point estimates were not statistically significant). As we discuss below, this finding is similar to results from recent examinations of other college and financial aid information interventions.

VI. DISCUSSION

Our analyses contribute to a growing body of research demonstrating that students face ongoing challenges and obstacles applying for financial aid even after submitting the FAFSA. Most efforts to support students to successfully apply for and receive financial aid have been conducted at the high school or community level, despite increasing calls for higher education institutions to make more investments to increase socioeconomic diversity. Our results provide suggestive and encouraging evidence that students' financial aid decisions, such as whether to submit applications in advance of priority deadlines and whether to complete supplementary forms like the CSS PROFILE, are responsive to outreach from their college or university.

[64] We define underrepresented minority as a student identifying as Black, Hispanic, Native American, multi-race, or unknown.

Our heterogeneous treatment effects also shed preliminary light into for whom such interventions may be most successful. This project stemmed from a broad university interest in outreach to low-income and underrepresented minority students across the commonwealth of Virginia, and the pilot version of the program specifically targeted schools with historically low application rates to UVA. Evidence from the 2016 rollout of the program suggests mixed success at achieving this goal. We observed that underrepresented minority students were not significantly responsive to outreach, while their White and Asian peers were more responsive. This is consistent with findings from a few recent studies of how high school students interpret information about college options and financial aid. In 2015, the U.S. Department of Education launched the "College Scorecard," a consumer tool for students and families to use comparing institutions on various metrics such as graduation rates or student debt. In an analysis of the Scorecard, researchers found that students were more likely to send SAT scores to colleges with higher earnings reported on the Scorecard, but that those results were concentrated among White and Asian students and students whose parents had some postsecondary education (Hurwitz & Smith, 2016). Similarly, while recent changes to FAFSA filing[65] appear to have resulted in more students filing the FAFSA, students attending schools with higher shares of White students and with fewer students eligible for free- or reduced-price lunch were more responsive to the policy shifts (Hillman, Bruecker, & Crespin-Trujillo, in progress). To the extent that students with existing cultural capital about college-going are more responsive to these types of interventions, they may fall short of any goals relating to reducing inequality in college outcomes.

Our overall findings are highly relevant to colleges and universities across the country interested in applying similar communication strategies. Many institutions have the resources and data infrastructure in place to replicate a similar campaign; students may be particularly likely to engage and respond to messages they receive from the colleges to which they have applied and hope to attend, rather than from the high school from which they are ready to move on. While our paper focuses on a text campaign to improve completion of FAFSA and CSS PROFILE filing, colleges and universities could leverage what

[65] Specifically, enabling applicants to use prior-prior year tax data and opening the application in October as opposed to January.

are often very robust student information systems along with periodic opportunities to collect and update contact information to provide students with simplified information and timely prompts to complete other important processes, like early course registration or financial aid renewal.

Furthermore, colleges could harness the predictive analytics strategies that a growing number of institutions employ to provide personalized, behaviorally informed guidance information about pathways students could pursue (e.g. which courses to take) that better position them to complete their program of study. Colleges are also well positioned to communicate directly with students about large-scale policy shifts, such as the changes to FAFSA filing noted above, and helping students navigate new systems.

We caution higher education administrators from interpreting the results of our paper to suggest that text messaging as a communications channel is the primary factor underlying the results of our intervention. While texting is effective at the moment as a means of connecting with and informing young people, it is also becoming increasingly utilized by the postsecondary education sector. As texting becomes increasingly saturated, students will inevitably migrate to other means of communication. The broader principles that we believe underlie our results are the combination of (a) utilizing communications channels that at a point in time are effective at reaching students; (b) communicating from an organization with whom the student has a valued relationship; (c) leveraging behavioral science principles to design campaigns and content in a way that maximizes student engagement and responsiveness. While texting provides an optimal channel through which to implement these strategies in the near term, practitioners and researchers will likely have to explore other channels in the years to come.

In sum, our paper provides further indication that students face a series of complex and confusing junctures on the road to and through college. Strategic, behaviorally informed outreach by higher education institutions can help students navigate these critical junctures and access resources to help them gain access to and succeed in college.

Table 3.1: In-State Admitted Students, 2015 and 2016 Cohorts

	Treatment-eligible	Ineligible: Opt-out	Ineligible: Any reason
Student Characteristics			
% Female	0.580	0.581	0.556
	[0.494]	[0.494]	[0.497]
% White	0.492	0.509	0.605
	[0.500]	[0.500]	[0.489]
% Black	0.101	0.091	0.050
	[0.301]	[0.287]	[0.217]
% Hispanic	0.062	0.069	0.052
	[0.240]	[0.253]	[0.222]
% Asian	0.220	0.170	0.152
	[0.414]	[0.376]	[0.359]
% Race not reported	0.052	0.094	0.084
	[0.223]	[0.292]	[0.277]
SAT (math + verbal)	1361	1381	1386
	[180]	[175]	[170]
Missing SAT	0.008	0.006	0.007
	[0.088]	[0.080]	[0.082]
% Early action	0.527	0.456	0.523
	[0.499]	[0.498]	[0.500]
Treatment Indicators			
% Opt in for texts	1.000	0.000	0.420
	[0.000]	[0.000]	[0.494]
% Interested in financial aid	1.000	1.000	0.351
	[0.000]	[0.000]	[0.477]
Select Outcomes			
% Filing CSS	0.821	0.813	0.472
	[0.383]	[0.390]	[0.499]
% Matriculate to UVA	0.596	0.510	0.602
	[0.491]	[0.500]	[0.490]
% Matriculate to "highly selective" college	0.869	0.858	0.883
	[0.338]	[0.350]	[0.321]
N students	3101	1707	4863

Notes: Standard deviations in brackets. Summarizes student characteristics, treatment eligibility, and select outcomes for our analytic sample, comparing treatment-eligible students to students ineligible for treatment because of opting out from receiving messages and to students ineligible for treatment because of any reason, either opting out or not indicating interest in financial aid (2015 and 2016 cohorts pooled).

Table 3.2: Difference-in-Differences Estimates of Changes to Student Composition

	Admitted pool		Early action admitted pool		Regular decision admitted pool	
	Ineligible: Opt-out	Ineligible: Any reason	Ineligible: Opt-out	Ineligible: Any reason	Ineligible: Opt-out	Ineligible: Any reason
% Female	-0.037	0.013	-0.054	-0.006	-0.019	0.036
	(0.029)	(0.022)	(0.042)	(0.029)	(0.041)	(0.033)
% White	0.094***	0.068**	0.155***	0.092**	0.048	0.044
	(0.028)	(0.022)	(0.042)	(0.031)	(0.041)	(0.032)
% Black	-0.032~	-0.013	-0.027	-0.004	-0.038	-0.025
	(0.017)	(0.012)	(0.024)	(0.016)	(0.027)	(0.021)
% Hispanic	-0.005	0.003	-0.013	-0.001	-0.001	0.006
	(0.015)	(0.011)	(0.020)	(0.013)	(0.021)	(0.017)
% Asian	-0.007	-0.021	-0.035	-0.029	0.012	-0.012
	(0.022)	(0.015)	(0.036)	(0.024)	(0.029)	(0.025)
% Race not reported	-0.047**	-0.032*	-0.061**	-0.044***	-0.034	-0.018
	(0.017)	(0.013)	(0.021)	(0.013)	(0.028)	(0.023)
SAT (math + verbal)	9.777	-1.332	13.419	1.363	11.658	1.331
	(7.821)	(10.394)	(9.665)	(7.052)	(12.158)	(17.352)
Missing SAT	0.001	0.006	-0.002	0.002	0.003	0.011
	(0.001)	(0.006)	(0.002)	(0.002)	(0.003)	(0.011)
% Applying early action	-0.039	-0.046*	0.000	0.000	0.000	0.000
	(0.030)	(0.023)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	4,808	7.964	2.413	4,177	2,395	3,787

Notes: Standard errors in parentheses. Each row reports the coefficient on the eligible-for-text and post interaction from a difference-in-difference model with each student characteristic as the outcome of interest. Each column uses a different group of ineligible students as the comparison group for analysis. Within each category (all admitted students, admitted early action, and admitted regular decision students), the first comparison group consists of students who intended to apply for financial aid but opted out from receiving text messages, and the second comparison group consists of those student plus students who consented to being contacted but were ineligible to receive text messages because they did not intend to apply for financial aid.
~p < 0.10, *p < 0.05, **p < 0.01, ***p < 0.001

Table 3.3: Main Results

	(1) Filed CSS	(2) Filed CSS on-time	(3) Enrolled at UVA	(4) Enrolled at "highly selective" institution	(5) Filed CSS	(6) Filed CSS on-time	(7) Enrolled at UVA	(8) Enrolled at "highly selective" institution
Post	-0.025	-0.032	0.009	-0.008	-0.037**	-0.039**	-0.008	0.002
	(0.018)	(0.019)	(0.026)	(0.017)	(0.014)	(0.014)	(0.014)	(0.010)
Eligible	-0.009	-0.010	0.079***	0.002	0.312***	0.316***	-0.013	-0.018
	(0.017)	(0.017)	(0.019)	(0.015)	(0.018)	(0.019)	(0.016)	(0.011)
Post*Eligible	0.034	0.031	-0.022	0.019	0.050*	0.043*	-0.005	0.010
	(0.024)	(0.024)	(0.028)	(0.021)	(0.021)	(0.022)	(0.020)	(0.015)
Comparison mean	0.813	0.789	0.510	0.858	0.472	0.447	0.602	0.883
Observations	4,808	4,808	4,808	4,808	7,964	7,964	7,964	7,964
R^2	0.005	0.011	0.063	0.008	0.131	0.125	0.061	0.007
Ineligible group	Opt-out	Opt-out	Opt-out	Opt-out	Any reason	Any reason	Any reason	Any reason

Notes: Standard errors clustered at high school in parentheses. Outcomes listed at the top of each column. This table includes in-state student applicants. In panel A, the ineligible group is students who intended to apply for aid but did not opt-in to the messages, and in panel B, the ineligible group also includes students who opted in but did not intend to apply for aid. All models include student-level covariates indicating gender, race, SAT score (and an indicator for SAT missing) and whether the student was an early action applicant.

~p < 0.10, *p < 0.05, **p < 0.01, ***p < 0.001

Table 3.4: Filing Results by Application Round

	Regular decision				Early action			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Filed CSS	Filed CSS on-time	Enrolled at UVA	Enrolled at "highly selective" institution	Filed CSS	Filed CSS on-time	Enrolled at UVA	Enrolled at "highly selective" institution
Post*Eligible	0.023	0.018	-0.034	0.022	0.048	0.046	-0.007	0.016
	(0.035)	(0.038)	(0.036)	(0.028)	(0.035)	(0.036)	(0.044)	(0.031)
Comparison mean	0.801	0.765	0.511	0.849	0.828	0.818	0.508	0.868
Observations	2,395	2,395	2,395	2,395	2,413	2,413	2,413	2,413
R^2	0.008	0.020	0.061	0.008	0.005	0.005	0.067	0.008

Notes: Standard errors clustered at high school in parentheses. Outcomes listed at the top of each column. This table includes in-state student admitted students. The ineligible group is students who intended to apply for aid but did not opt-in to the messages.
~$p < 0.10$, *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$

Table 3.5: Filing Results by Student Characteristic

	Below-median SAT score				Above-median SAT score			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Filed CSS	Filed CSS on-time	Enrolled at UVA	Enrolled at "highly selective" institution	Filed CSS	Filed CSS on-time	Enrolled at UVA	Enrolled at "highly selective" institution
Post*Eligible	0.024	0.023	-0.032	0.013	0.042	0.035	-0.014	0.026
	(0.038)	(0.042)	(0.045)	(0.036)	(0.031)	(0.032)	(0.035)	(0.026)
Comparison mean	0.786	0.744	0.617	0.840	0.831	0.818	0.439	0.869
Observations	2,122	2,122	2,122	2,122	2,686	2,686	2,686	2,686
R^2	0.140	0.131	0.034	0.013	0.127	0.126	0.058	0.005
	Non-URM				**URM**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Filed CSS	Filed CSS on-time	Enrolled at UVA	Enrolled at "highly selective" institution	Filed CSS	Filed CSS on-time	Enrolled at UVA	Enrolled at "highly selective" institution
Post*Eligible	0.053~	0.043	-0.014	0.016	-0.004	0.006	-0.035	0.031
	(0.028)	(0.028)	(0.033)	(0.025)	(0.041)	(0.046)	(0.054)	(0.038)
Comparison mean	0.809	0.794	0.523	0.857	0.821	0.779	0.482	0.859
Observations	3,366	3,366	3,366	3,366	1,442	1,442	1,442	1,442
R^2	0.005	0.010	0.067	0.011	0.005	0.016	0.064	0.007

Notes: Standard errors clustered at high school in parentheses. Outcomes listed at the top of each column. This table includes in-state student admitted students and examines heterogeneous treatment effects by student characteristics. First, we examine whether a students' responsiveness differed by if that student's combined math and verbal SAT score was above or below the median score among UVA matriculates in 2015 (1,370). Then we examine whether a students' responsiveness differed by if that student was an underrepresented minority (Black, Hispanic, Native American, multi-race, or unknown). The ineligible comparison group includes students who intended to apply for aid but did not opt in to receive messages.

~p < 0.10, *p < 0.05, **p < 0.01, ***p < 0.001

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APPENDIX TABLES AND FIGURES

Appendix Table 1.1: Outcome-Specific Bandwidths

Variable	Bandwidth	N Schools	Bandwidth	N Schools
A. Course and Test Taking				
Share students taking AP	121	120	129	132
Share students taking Algebra I early	164	175	169	181
Share students taking Algebra II	154	166	157	170
Share students taking Calculus	138	146	146	157
Share students taking ACT/SAT	138	146	105	104
B. Discipline				
Any suspension, all students	100	96	77	77
In School Suspension, all students	101	98	125	125
Out of School Suspension, all students	106	105	164	175
Any suspension, students without disability	97	94	95	93
In School Suspension, students without disability	98	96	126	126
Out of School Suspension, students without disability	103	102	150	161
Any suspension, students with disability	85	83	97	94
In School Suspension, students with disability	104	104	121	120
Out of School Suspension, students with disability	81	78	117	116
Expulsion, all students	103	102	91	91
Expulsion, students without disability	94	92	68	68
Expulsion, students with disability	164	175	118	116
C. School Engagement				
Chronic absenteeism	136	140	122	122
Chronic absenteeism, students with disability	139	147	104	104
Retained in 10th grade	115	114	87	87
Retained in 10th grade, students with disability	148	159	104	104
Kernel	Triangular		Uniform	

Notes: Mean-squared error optimal bandwidths estimated using *rdbwselect* command in Stata developed by Calonico, Cattaneo, & Titiunik (2014). Each bandwidth rounded to the nearest whole number.

Appendix Table 1.2: Continuity of non-outcome variables

	Pooled			2013-14	2015-16
Average number of teachers	0.598	0.272	0.523	0.922	0.051
	(1.885)	(2.169)	(2.608)	(2.881)	(2.332)
Locale: Urban	0.084	0.082	-0.077	0.105	0.063
	(0.054)	(0.060)	(0.081)	(0.077)	(0.077)
Locale: Rural	-0.118	-0.127	-0.050	-0.135	-0.097
	(0.090)	(0.094)	(0.140)	(0.125)	(0.130)
Title I eligible	-0.063	-0.005	0.070	-0.098	-0.021
	(0.075)	(0.079)	(0.112)	(0.109)	(0.100)
Whether school employs security	-0.080	-0.037	-0.205	-0.112	-0.055
	(0.092)	(0.097)	(0.137)	(0.126)	(0.133)
Female	-0.013	-0.003	-0.000	-0.011	-0.014
	(0.008)	(0.006)	(0.013)	(0.012)	(0.010)
American Indian/Native	0.026	0.059~	0.143**	0.023	0.029
	(0.032)	(0.032)	(0.049)	(0.047)	(0.043)
Black	-0.036	-0.047	-0.123**	-0.027	-0.046
	(0.030)	(0.036)	(0.047)	(0.045)	(0.039)
White	-0.009	-0.011	0.043	-0.036	0.018
	(0.037)	(0.040)	(0.057)	(0.057)	(0.047)
Subsidized lunch eligible	0.258	0.004	-0.446	0.475	0.027
	(0.209)	(0.073)	(0.421)	(0.402)	(0.057)
Documented disability plan	-0.008	0.000	0.006	-0.014	0.000
	(0.011)	(0.011)	(0.017)	(0.018)	(0.014)
Functional Form	Linear interaction	Quadratic	Quadratic interaction	Linear interaction	Linear interaction
Number of Schools	788	788	788	392	396

Notes: Robust standard errors clustered by running variable in parentheses.
~p<0.10, *p<0.05, **p<0.01, ***p<0.001

Appendix Table 1.3: Reduced form and Regression discontinuity estimates, non-discipline outcomes

	Pooled		2013-14		2015-16	
	Reduced form	2SLS	Reduced form	2SLS	Reduced form	2SLS
Share students taking AP	-0.002	-0.003	-0.005	-0.007	0.000	0.001
	(0.019)	(0.027)	(0.026)	(0.033)	(0.028)	(0.046)
Share students taking Algebra I early	0.023	0.032	0.049*	0.063~	-0.009	-0.014
	(0.018)	(0.026)	(0.024)	(0.036)	(0.026)	(0.042)
Share students taking Algebra II	0.030*	0.043~	0.051**	0.065*	0.006	0.010
	(0.015)	(0.022)	(0.019)	(0.030)	(0.021)	(0.033)
Share students taking Calculus	-0.001	-0.002	-0.012~	-0.016	0.008~	0.013
	(0.004)	(0.006)	(0.007)	(0.010)	(0.005)	(0.008)
Share students taking ACT/SAT	0.017	0.025	0.014	0.017	0.017	0.027
	(0.031)	(0.045)	(0.044)	(0.056)	(0.042)	(0.067)
Chronic absenteeism	0.036	0.051	0.036	0.046	0.030	0.050
	(0.023)	(0.033)	(0.038)	(0.049)	(0.028)	(0.046)
Chronic absenteeism, students with disability	0.011	0.016	-0.028	-0.035	0.045	0.073
	(0.032)	(0.046)	(0.053)	(0.064)	(0.038)	(0.065)
Retained in 10th grade	0.003	0.004	0.003	0.003	0.003	0.006
	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.005)
Retained in 10th grade, students with disability	0.004	0.005	0.009	0.011	-0.001	-0.001
	(0.004)	(0.006)	(0.007)	(0.009)	(0.006)	(0.010)
Observations	788	788	392	392	396	396
Functional form	Linear interaction	Linear interaction	Linear interaction	Linear interaction	Linear interaction	Linear interaction
Bandwidth	Full	Full	Full	Full	Full	Full

Notes: Robust standard errors clustered by running variable in parentheses. Pooled year estimates include year fixed effects. All models include measures of percent American Indian/Native students, percent black students, percent white students, percent female students, percent students with documented disability, the total number of teachers employed, whether the school employs a security guard, whether school is Title I eligible, and school locale.
~p<0.10, *p<0.05, **p<0.01, ***p<0.001

Appendix Table 1.4: Heterogeneity of Counselor Effect by Minority Student Enrollment

	Majority Minority Enrollment	Majority White Enrollment		Below-Median Enrollment, Students with disabilities	Above-Median Enrollment, Students with disabilities	
	Reduced Form	Reduced Form	p(above = below)	Reduced Form	Reduced Form	p(above = below)
First stage: Additional school counselor	1.306***	0.259*	0.000	0.497**	0.876***	0.151
	(0.250)	(0.130)		(0.162)	(0.209)	
Any suspension, all students	0.042	0.016	0.629	0.031	0.020	0.819
	(0.045)	(0.026)		(0.029)	(0.035)	
In School Suspension, all students	0.041	0.013	0.520	0.029	0.021	0.842
	(0.036)	(0.020)		(0.023)	(0.028)	
Out of School Suspension, all students	0.002	0.003	0.975	0.002	-0.002	0.883
	(0.022)	(0.013)		(0.016)	(0.016)	
Any suspension, students without disability	0.047	0.015	0.587	0.021	0.026	0.902
	(0.047)	(0.027)		(0.028)	(0.035)	
In School Suspension, students without disability	0.036	0.014	0.634	0.021	0.022	0.966
	(0.039)	(0.019)		(0.022)	(0.028)	
Out of School Suspension, students without disability	0.011	0.001	0.700	-0.000	0.004	0.859
	(0.021)	(0.014)		(0.015)	(0.016)	
Any suspension, students with disability	0.003	-0.024	0.827	-0.048	0.026	0.468
	(0.109)	(0.046)		(0.068)	(0.072)	
In School Suspension, students with disability	0.069	-0.022	0.235	0.007	0.032	0.718
	(0.066)	(0.033)		(0.043)	(0.052)	
Out of School Suspension, students with disability	-0.070	-0.007	0.303	-0.067~	-0.004	0.220
	(0.055)	(0.025)		(0.039)	(0.032)	
Expulsion, all students	0.039*	-0.013**	0.004	-0.000	0.014	0.259
	(0.018)	(0.005)		(0.005)	(0.013)	
Expulsion, students without disability	0.046*	-0.011*	0.008	-0.001	0.022	0.121
	(0.021)	(0.004)		(0.005)	(0.015)	
Expulsion, students with disability	0.014	-0.017	0.172	0.005	-0.012	0.435
	(0.019)	(0.012)		(0.014)	(0.016)	
Observations			788			788

Notes: Robust standard errors clustered by running variable in parentheses. Control mean represents the mean of the outcome for schools just below the policy threshold. All models include year fixed effects and measures of percent American Indian/Native students, percent black students, percent white students, percent female students, percent students with documented disability, the total number of teachers employed, whether the school employs a security guard, whether school is Title I eligible, and school locale.
~p<0.01, *p<0.05, **p<0.01, ***p<0.001

Appendix Table 1.5: Reduced form and 2SLS Discipline Outcomes, Multiple Counselor Margins

	Pooled		2013-14		2015-16	
	Reduced form	2SLS	Reduced form	2SLS	Reduced Form	2SLS
Any suspension, all students	-0.007	-0.015	0.027	0.077	-0.033	-0.061
	(0.023)	(0.052)	(0.037)	(0.121)	(0.032)	(0.063)
In School Suspension, all students	0.003	0.007	0.029	0.085	-0.022	-0.040
	(0.017)	(0.038)	(0.027)	(0.098)	(0.024)	(0.046)
Out of School Suspension, all students	-0.010	-0.021	-0.003	-0.008	-0.011	-0.021
	(0.012)	(0.027)	(0.021)	(0.058)	(0.015)	(0.028)
Any suspension, students without disability	-0.008	-0.017	0.043	0.124	-0.047	-0.087
	(0.023)	(0.051)	(0.037)	(0.132)	(0.030)	(0.063)
In School Suspension, students without disability	0.001	0.003	0.037	0.108	-0.030	-0.056
	(0.017)	(0.038)	(0.027)	(0.103)	(0.023)	(0.046)
Out of School Suspension, students without disability	-0.009	-0.020	0.005	0.016	-0.017	-0.031
	(0.012)	(0.027)	(0.021)	(0.060)	(0.015)	(0.028)
Any suspension, students with disability	-0.022	-0.047	-0.087	-0.254	0.043	0.080
	(0.052)	(0.113)	(0.083)	(0.269)	(0.060)	(0.104)
In School Suspension, students with disability	-0.007	-0.016	-0.041	-0.119	0.016	0.029
	(0.035)	(0.076)	(0.054)	(0.168)	(0.045)	(0.077)
Out of School Suspension, students with disability	-0.014	-0.031	-0.046	-0.134	0.027	0.050
	(0.028)	(0.058)	(0.047)	(0.144)	(0.028)	(0.051)
Expulsion, all students	0.006	0.013	0.018	0.052	-0.000	-0.000
	(0.007)	(0.015)	(0.014)	(0.048)	(0.005)	(0.009)
Expulsion, students without disability	0.009	0.019	0.024	0.069	0.001	0.002
	(0.008)	(0.018)	(0.016)	(0.056)	(0.004)	(0.008)
Expulsion, students with disability	-0.005	-0.010	-0.005	-0.015	-0.003	-0.005
	(0.009)	(0.020)	(0.017)	(0.045)	(0.010)	(0.018)
Observations	371	371	190	190	181	181
Functional form	Linear interaction	Linear interaction	Linear interaction	Linear interaction	Linear interaction	Linear interaction
Bandwidth	Full	Full	Full	Full	Full	Full

Notes: Robust standard errors clustered by running variable in parentheses. Pooled years include year fixed effects. All models include measures of percent American Indian/Native students, percent black students, percent white students, percent female students, percent students with documented disability, the total number of teachers employed, whether the school employs a security guard, whether school is Title I eligible, and school locale.
~p<0.10, *p<0.05, **p<0.01, ***p<0.001

Appendix Table 1.6: Reduced form and 2SLS Discipline Outcomes, Counselor-Student Ratio as Treatment

	Pooled		**2013-14**		**2015-16**	
	Reduced form	2SLS	Reduced form	2SLS	Reduced Form	2SLS
Any suspension, all students	0.025	0.008	0.056~	0.050	-0.007	-0.046
	(0.022)	(0.041)	(0.032)	(0.054)	(0.030)	(0.073)
In School Suspension, all students	0.025	0.013	0.050~	0.045	-0.002	-0.032
	(0.018)	(0.033)	(0.026)	(0.044)	(0.024)	(0.057)
Out of School Suspension, all students	-0.000	-0.005	0.006	0.004	-0.005	-0.014
	(0.011)	(0.020)	(0.016)	(0.027)	(0.014)	(0.031)
Any suspension, students without disability	0.025	0.011	0.074*	0.074	-0.023	-0.069
	(0.022)	(0.041)	(0.032)	(0.054)	(0.029)	(0.069)
In School Suspension, students without disability	0.022	0.010	0.061*	0.057	-0.018	-0.054
	(0.018)	(0.033)	(0.027)	(0.043)	(0.023)	(0.058)
Out of School Suspension, students without disability	0.003	0.001	0.013	0.016	-0.006	-0.015
	(0.011)	(0.020)	(0.016)	(0.027)	(0.015)	(0.031)
Any suspension, students with disability	-0.007	-0.053	-0.116	-0.122	0.095	0.054
	(0.050)	(0.087)	(0.071)	(0.110)	(0.063)	(0.132)
In School Suspension, students with disability	0.021	-0.006	-0.034	-0.060	0.074~	0.074
	(0.033)	(0.061)	(0.048)	(0.077)	(0.044)	(0.097)
Out of School Suspension, students with disability	-0.033	-0.047	-0.082*	-0.062	0.012	-0.019
	(0.025)	(0.045)	(0.039)	(0.060)	(0.028)	(0.057)
Expulsion, all students	0.007	0.010	0.019	0.022	-0.007	-0.004
	(0.008)	(0.013)	(0.013)	(0.018)	(0.005)	(0.012)
Expulsion, students without disability	0.011	0.015	0.026~	0.027	-0.006	0.001
	(0.009)	(0.014)	(0.015)	(0.021)	(0.005)	(0.011)
Expulsion, students with disability	-0.004	-0.006	-0.011	0.010	0.000	-0.024
	(0.011)	(0.016)	(0.017)	(0.024)	(0.013)	(0.026)
Observations	788	259	392	136	396	123
	Linear interaction	Linear interaction	Linear interaction	Linear interaction	Linear interaction	Linear interaction
Bandwidth	Full	|si|225	Full	|si|225	Full	|si|225

Notes: Robust standard errors clustered by running variable in parentheses. All models include measures of percent American Indian/Native students, percent black students, percent white students, percent female students, percent students with documented disability, the total number of teachers employed, whether the school employs a security guard, whether school is Title I eligible, and school locale.

~p<0.10, *p<0.05, **p<0.01, ***p<0.001

Appendix Table 1.7: Year-to-Year Variance in School Characteristics by Student Enrollment

	Below		Above	
	2013-14	2015-16	2013-14	2015-16
Total Teacher FTE	24.204	24.329	39.492	37.462
	[9.538]	[10.214]	[15.757]	[7.948]
School Has Security Officer	0.289	0.286	0.487	0.487
	[0.455]	[0.454]	[0.506]	[0.506]
Percent American Indian/Native	0.244	0.237	0.242	0.244
	[0.170]	[0.164]	[0.170]	[0.165]
Percent Subsidized Lunch	0.527	0.578~	0.546	0.570
	[0.209]	[0.193]	[0.224]	[0.219]
Percent IDEA	0.159	0.158	0.150	0.158
	[0.068]	[0.066]	[0.053]	[0.038]
Total Instructional Salaries/1000	994.648	1043.580	1514.680	1615.371
	[660.994]	[711.199]	[580.370]	[403.913]
Total Personnel Salaries/1000	1921.184	1376.029~	2167.119	2272.370
	[2591.331]	[842.003]	[894.529]	[799.785]
Number of Observations	97	97	39	39

Notes: Standard deviation in brackets. Each column reports the average school characteristic for schools with student enrollments above or below the policy threshold in a given data collection year and notes whether that mean difference is statistically significant. ~p<0.10, *p<0.05, **p<0.01, ***p<0.001

Appendix Table 1.8: Year-to-Year Variance in Incident Rates by Student Enrollment

	Below		Above	
	2013-14	2015-16	2013-14	2015-16
Any suspension, students with disability	0.290	0.254	0.271	0.323
	[0.327]	[0.269]	[0.203]	[0.267]
Any suspension, students without disability	0.188	0.201	0.220	0.164
	[0.199]	[0.198]	[0.196]	[0.105]
In School Suspension, students with disability	0.140	0.121	0.158	0.186
	[0.187]	[0.154]	[0.151]	[0.168]
In School Suspension, students without disability	0.104	0.117	0.146	0.102~
	[0.110]	[0.130]	[0.140]	[0.072]
Out of School Suspension, students with disability	0.149	0.133	0.113	0.136
	[0.199]	[0.142]	[0.093]	[0.136]
Out of School Suspension, students without disability	0.084	0.084	0.074	0.062
	[0.113]	[0.098]	[0.074]	[0.063]
Expulsions, students with disability	0.020	0.019	0.012	0.013
	[0.062]	[0.054]	[0.042]	[0.050]
Expulsions, students without disability	0.009	0.009	0.029	0.004~
	[0.023]	[0.030]	[0.078]	[0.010]
Number of Observations	97	97	39	39

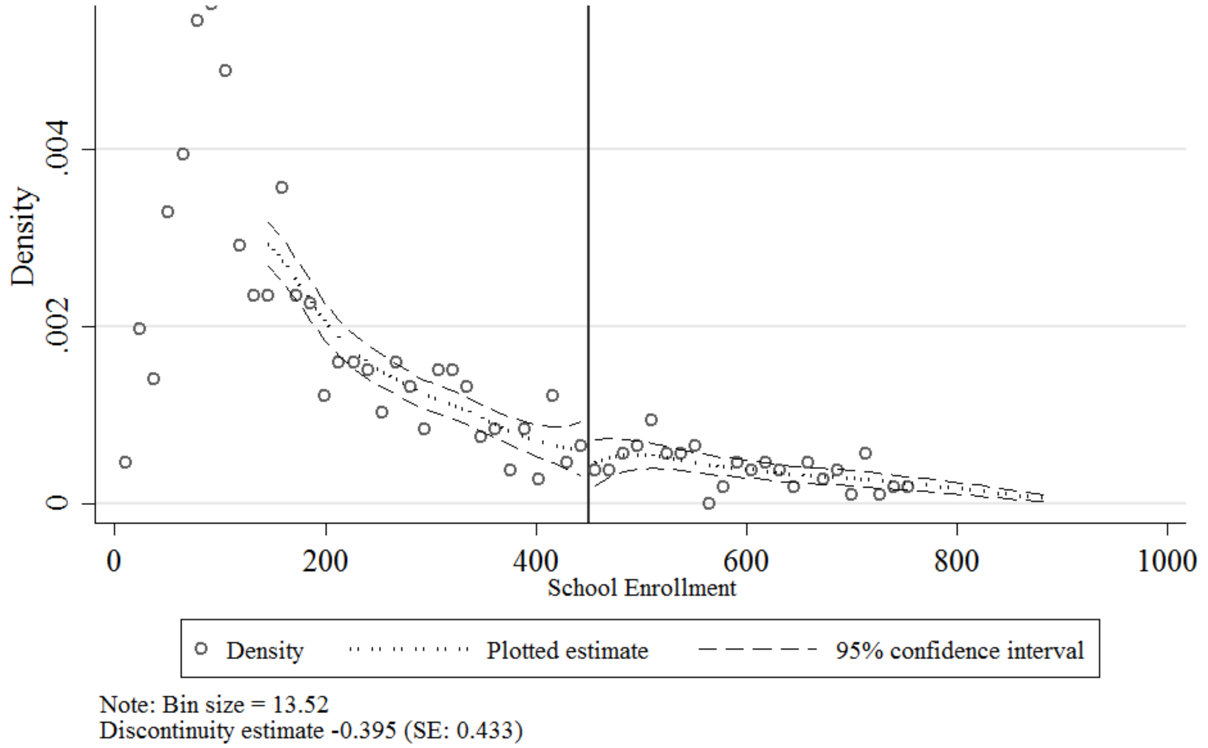
Notes: Standard deviation in brackets. Each column reports the average incident rate for schools with student enrollments above or below the policy threshold in a given data collection year and notes whether that mean difference is statistically significant.
~p<0.10, *p<0.05, **p<0.01, ***p<0.001

Appendix Table 1.9: RD Results, Counselor Staffing and Characteristics

	Pooled	2013-14	2015-16
Counselor FTE, OK state records	0.477***	0.425*	0.529**
	(0.137)	(0.210)	(0.174)
Number of employees performing counseling	0.689***	0.597**	0.789***
	(0.148)	(0.221)	(0.193)
Percent counselors serving only one school	-0.047	0.015	-0.116
	(0.069)	(0.089)	(0.106)
Average percent of time counselors spend on counseling	-0.136***	-0.118**	-0.157***
	(0.028)	(0.038)	(0.042)
Average wages paid to counseling	25343***	23007*	27660**
	(6835)	(9511)	(9781)
Average years of experience	3.549*	3.008	4.146
	(1.685)	(2.225)	(2.558)
Percent American Indian/Native Counselors	-0.023	-0.077	0.033
	(0.046)	(0.058)	(0.072)
Percent White Counselors	-0.023	0.020	-0.068
	(0.064)	(0.089)	(0.091)
Percent Female Counselors	-0.048	-0.032	-0.067
	(0.047)	(0.072)	(0.061)
Number of Observations	757	378	379

Notes: Standard deviation in brackets. Each column reports the average incident rate for schools with student enrollments above or below the policy threshold in a given data collection year and notes whether that mean difference is statistically significant.
~p<0.10, *p<0.05, **p<0.01, ***p<0.001

Figure A1.1: McCrary Density Test



Appendix Table 2.1: Continuity of Non-Outcome Variables

Variable	Sample	MSE-Optimal Bandwidth	RD Estimator	p-value	Number of Observations
Female	2016	650	*-0.093*	*0.083*	1386
	2017	1680	0.001	0.984	4057
	2018	970	0.013	0.749	2374
Asian	2016	890	**0.073**	**0.019**	1870
	2017	1400	0.029	0.228	3365
	2018	700	-0.025	0.452	1687
Black	2016	620	-0.070	0.113	1326
	2017	1100	-0.013	0.691	2611
	2018	910	-0.058	0.107	2229
Hispanic	2016	1030	-0.015	0.628	2159
	2017	1230	-0.012	0.628	2954
	2018	1010	0.015	0.578	2474
White	2016	1350	0.026	0.379	2859
	2017	1270	-0.013	0.657	3068
	2018	950	-0.005	0.877	2312
First Generation	2016	1390	**0.084**	**0.021**	2954
	2017	1160	0.026	0.506	2755
	2018	810	0.012	0.789	1954
Dependent student	2016	1110	-0.008	0.577	2315
	2017	720	0.003	0.841	1674
	2018	900	**0.034**	**0.007**	2200
Age	2016	630	*0.352*	*0.098*	1347
	2017	920	-0.167	0.367	2186
	2018	1000	*-0.314*	*0.055*	2441
High school credits	2016	890	0.073	0.816	1870
	2017	850	-0.396	0.191	2010
	2018	870	0.192	0.508	2117

Notes: Robust standard errors in parentheses. Includes application year and admitted college fixed effects. RD estimator column represents the difference at the $3,000 EFC threshold between waiver eligible and ineligible students on various characteristics.

Appendix Table 2.2: Effect of Waiver eligibility on students' enrollment, 2016 cohort excluded

	Enrolled at CUNY	Enrolled in College	Enrolled Four-Year	Enrolled Public College	Enrolled in New York State
Waiver Eligible	-0.017	-0.003	-0.016	-0.001	0.013
	(0.028)	(0.019)	(0.021)	(0.026)	(0.023)
Distance from Cutoff	0.000	0.000	-0.000	0.000~	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligibility*Distance	-0.000	-0.000	0.000	-0.000*	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	5317	5317	5317	5317	5317
R^2	0.027	0.086	0.326	0.028	0.054

Notes: Robust standard errors clustered on the running variable in parentheses. Restricted to 2017 and 2018 cohorts. Models include application year and admitted college fixed effects. Enrolled at CUNY is a measure whether students in a given application year immediately enrolled at a CUNY institution the fall after application. Enrolled in college indicates whether students either enrolled at CUNY or at another institution reporting to the National Student Clearinghouse; enrollment at a four-year, public, or New York State institution indicates the type of institution at which a student enrolled. Models include application year and admitted college fixed effects as well as a vector of student covariates including age, race, sex, parental education, financial aid dependency, and high school academic preparation. Sample limited to students who completed the Free Application for Financial Aid (FAFSA) with a non-missing expected family contribution (EFC) value reported prior to May 1, the commitment deposit deadline.

~p<0.1, *p<0.05, **p<0.01, ***p<0.001

Appendix Table 2.3: Heterogeneous Enrollment Effects by Student Characteristics, 2016 Cohort Excluded

	Enroll CUNY	Enroll Any	Enroll CUNY	Enroll Any	Enroll CUNY	Enroll Any
Waiver Eligible	-0.119	-0.179	-0.073	-0.069	-0.032	0.002
	(0.141)	(0.142)	(0.067)	(0.054)	(0.030)	(0.021)
Distance from Cutoff	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligibility*Distance	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligibility*Age	**0.006**	**0.010**				
	(0.007)	**(0.008)**				
Age	0.006	-0.012~				
	(0.008)	(0.007)				
Eligibility*High School Credits			**0.003**	**0.004**		
			(0.004)	**(0.003)**		
High School Credits			0.008**	0.012***		
			(0.003)	(0.002)		
Eligibility*First Generation					**0.034**	**-0.011**
					(0.028)	**(0.019)**
First Generation					-0.002	-0.002
					(0.021)	(0.015)
Observations	5317	5317	5317	5317	5317	5317
R^2	0.027	0.086	0.028	0.086	0.028	0.086

Notes: Robust standard errors clustered on the running variable in parentheses. 2016 application cohort excluded. Models include application year and admitted college fixed effects. Enrolled at CUNY is a measure whether students in a given application year immediately enrolled at a CUNY institution the fall after application. Enrolled in college indicates whether students either enrolled at CUNY or at another institution reporting to the National Student Clearinghouse. Models include application year and admitted college fixed effects as well as a vector of student covariates including age, race, sex, parental education, financial aid dependency, and high school academic preparation. Sample limited to students who completed the Free Application for Financial Aid (FAFSA) with a non-missing expected family contribution (EFC) value reported prior to May 1, the commitment deposit deadline.
~p<0.1, *p<0.05, **p<0.01, ***p<0.001

Appendix Table 2.4: Effect of Waiver eligibility on students' enrollment

	Enrolled at CUNY	Enrolled in College	Enrolled Four-Year	Enrolled Public College	Enrolled in New York State	Deposit Discontinuity
Waiver Eligible	0.013	0.013	0.009	0.032	0.029	0.029***
	(0.022)	(0.015)	(0.016)	(0.020)	(0.018)	(0.019)
Distance from Cutoff	0.000	0.000	-0.000	0.000*	0.000	0.000~
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligibility*Distance	-0.000	0.000	0.000	-0.000*	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	9150	9150	9150	9150	9150	9150
R^2	0.024	0.091	0.330	0.026	0.056	0.099

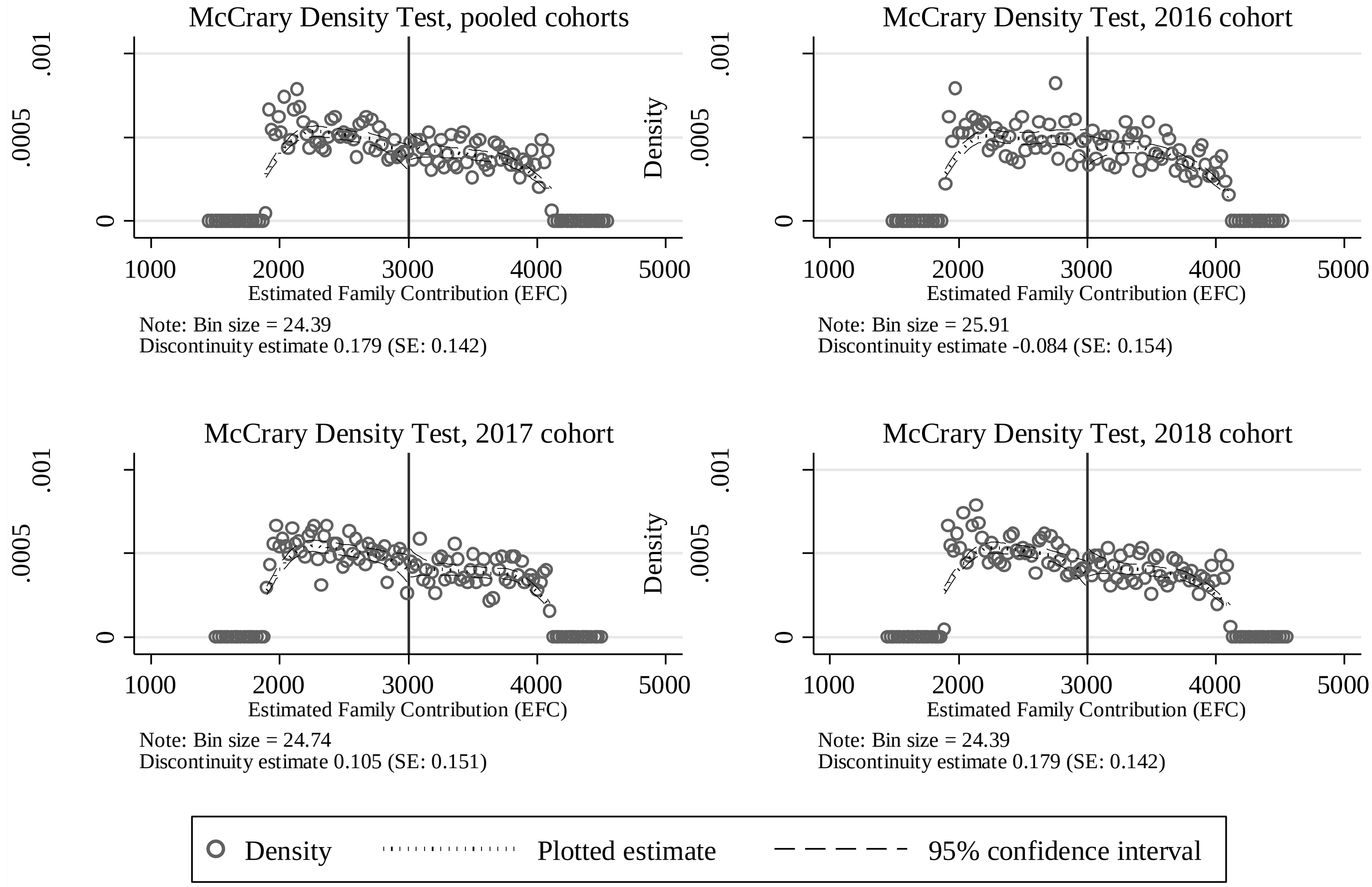
Notes: Robust standard errors clustered on the running variable in parentheses. Models include application year and admitted college fixed effects. Enrolled at CUNY is a measure whether students in a given application year immediately enrolled at a CUNY institution the fall after application. Enrolled in college indicates whether students either enrolled at CUNY or at another institution reporting to the National Student Clearinghouse; enrollment at a four-year, public, or New York State institution indicates the type of institution at which a student enrolled. Models include application year and admitted college fixed effects as well as a vector of student covariates including age, race, sex, parental education, financial aid dependency, and high school academic preparation. Sample limited to students who completed the Free Application for Financial Aid (FAFSA) with a non-missing expected family contribution (EFC) value reported prior to May 1, the commitment deposit deadline. ~p<0.1, *p<0.05, **p<0.01, ***p<0.001

Appendix Table 2.5: Effect of Waiver eligibility on students' enrollment by sector

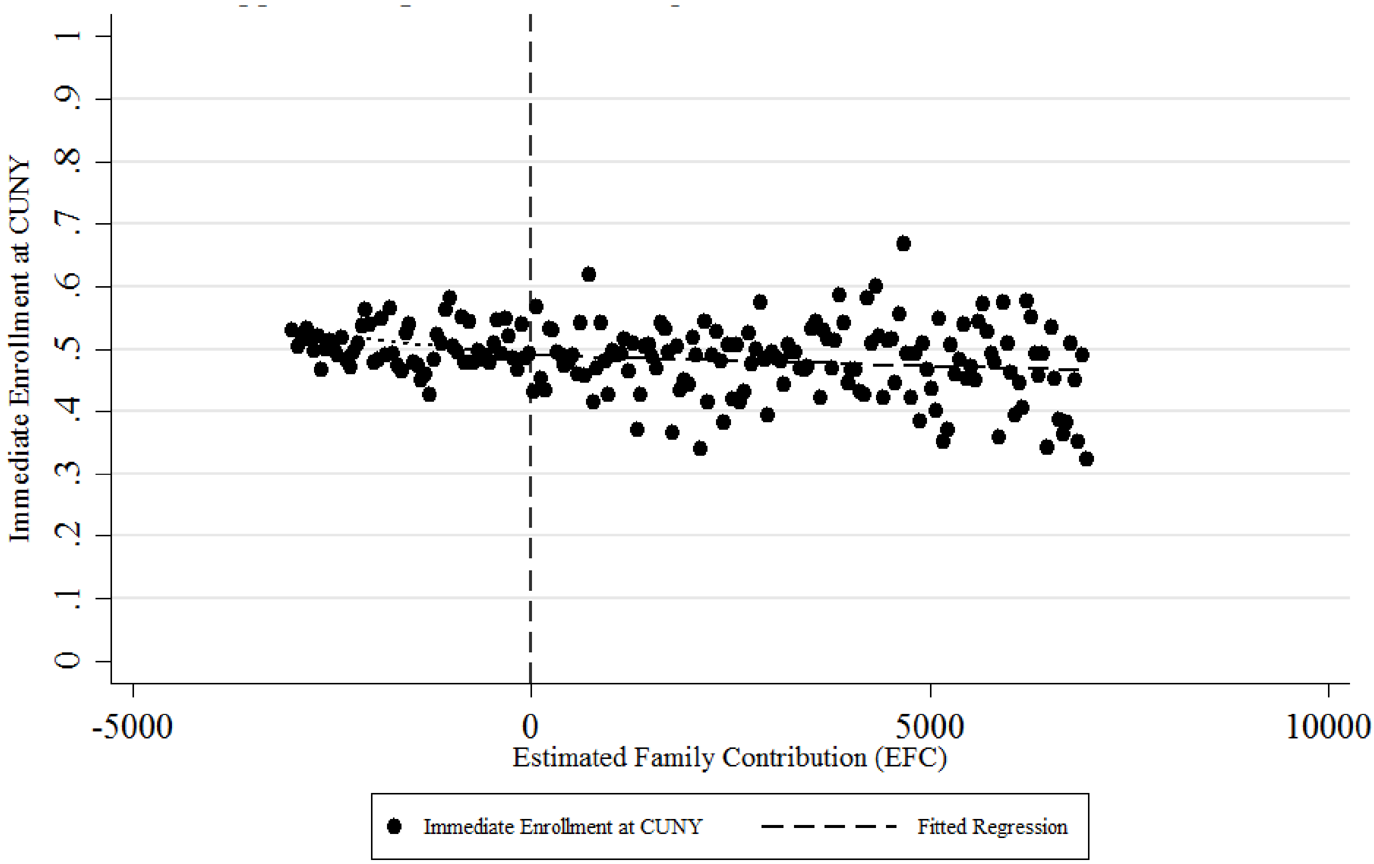
	Enrolled at CUNY	Enrolled in College	Enrolled Four-Year	Enrolled Public College	Enrolled in New York State
BW = 1,100					
Waiver Eligible	-0.000	-0.007	-0.010	0.016	0.015
	(0.023)	(0.016)	(0.017)	(0.022)	(0.019)
Observations	7614	7614	7614	7614	7614
R^2	0.024	0.088	0.330	0.026	0.054
BW = 1,000					
Waiver Eligible	0.008	-0.004	-0.007	0.029	0.019
	(0.024)	(0.017)	(0.018)	(0.023)	(0.020)
Observations	6898	6898	6898	6898	6898
R^2	0.025	0.093	0.335	0.027	0.055
BW = 800					
Waiver Eligible	0.017	-0.008	-0.008	0.045~	0.020
	(0.027)	(0.019)	(0.020)	(0.026)	(0.022)
Observations	5492	5492	5492	5492	5492
R^2	0.028	0.096	0.339	0.030	0.059
BW = 700					
Waiver Eligible	0.018	-0.013	-0.008	0.036	0.018
	(0.029)	(0.020)	(0.022)	(0.028)	(0.024)
Observations	4799	4799	4799	4799	4799
R^2	0.033	0.106	0.349	0.032	0.064

Notes: Robust standard errors clustered on the running variable in parentheses. Models include application year and admitted college fixed effects. Enrolled at CUNY is a measure whether students in a given application year immediately enrolled at a CUNY institution the fall after application. Enrolled in college indicates whether students either enrolled at CUNY or at another institution reporting to the National Student Clearinghouse; enrollment at a four-year, public, or New York State institution indicates the type of institution at which a student enrolled. Models include application year and admitted college fixed effects as well as a vector of student covariates including age, race, sex, parental education, financial aid dependency, and high school academic preparation. Sample limited to students who completed the Free Application for Financial Aid (FAFSA) with a non-missing expected family contribution (EFC) value reported prior to May 1, the commitment deposit deadline.
~p<0.1, *p<0.05, **p<0.01, ***p<0.001

Appendix Figure 2.1: McCrary Density Test by Cohort



Appendix Figure 2.2: Relationship between EFC and CUNY Enrollment, Full Bandwidth



Notes: EFC is centered at the cutoff ($3,000). Each dot represents the average CUNY enrollment rate for EFC bins of $50. Includes observations with an EFC from $0 to $10,000 (N=89295 observations)

Appendix Table 3.1: Text Message Content

	Early Action Admitted Students	Regular Decision Applicants
Message Purpose: Introductory Message *Delivery Date*: 2/16/2016	*Message to Student:* Hi [STUDENT NAME], this is Kelsey from UVA admissions. We want to make sure you get all the financial aid you're eligible for! (1/2) Stay tuned for 3-4 text messages over the next month w/ important financial aid-related info and reminders. Text back if you have questions or need help! (2/2)	*Message to Student:* AGE TO STUDENT: Hi [STUDENT NAME], this is Kelsey from UVA admissions. We want to make sure you get all the financial aid you're eligible for, if you're admitted to UVA! (1/2) Stay tuned for 3-4 text messages over the next month w/ important financial aid-related info and reminders. (2/2)
Message Purpose: Importance of timely filing *Delivery Date*: 2/18/2016	*Message to Student*: Hi [STUDENT NAME], it's Kelsey again from UVA. Did you know that getting your FAFSA and CSS/Profile in by March 1 can mean $1000s in financial aid to you? (1/2) Reply "completed" if you've already done the FAFSA and CSS or "not yet" if you haven't completed either application. (2/2)	*Message to Student*: Hi [STUDENT NAME], it's Kelsey again from UVA. Did you know that getting your FAFSA and CSS/Profile in by March 1 can mean $1000s in financial aid to you?
Message Purpose: Provide resources *Delivery Date*: 2/23/2016	*Message to Student*: Hi [STUDENT NAME], it's Kelsey again from UVA. Did you know that getting your FAFSA and CSS/Profile in by March 1 can mean $1000s in financial aid to you? (1/2) Visit virginia.edu/costestimator to see how much aid you would receive from UVA. Complete the FAFSA & CSS/Profile to receive YOUR share of financial aid (2/2)	*Message to Student*: Hi [STUDENT NAME]. Between federal and state grants and financial aid we offer, UVA may be much more affordable than you think! (1/2) Visit virginia.edu/costestimator to see how much aid you would receive from UVA. (2/2)
Message Purpose: Timely reminder; scheduling prompt *Delivery Date*: 2/26/2016	*Message to Student*: Hi [STUDENT NAME], only 5 days left before the March 1 deadline for the FAFSA & CSS/Profile. Applying by 3/1 can mean $1000s more in aid. (1/2) Is there a day this week when you could set aside a couple hours to work on these forms? Text back if you need help. (2/2)	*Message to Student*: Hi [STUDENT NAME], only 5 days left before the March 1 deadline for the FAFSA & CSS/Profile. Applying by 3/1 can mean $1000s more in aid. (1/2) If you can, find a day this week when you could set aside a couple hours to work on these forms. (2/2)

Appendix Table 3.2: Summary Statistics, 2015 Pilot Schools and 2016 Full Implementation Schools

	2015 pilot schools	2016 rollout: high FRPL schools	2016 rollout: low FRPL schools	2016 rollout: all schools
UVA 2015 applicant individual characteristics				
% Female	0.523	0.546	0.545	0.542
	[0.500]	[0.498]	[0.498]	[0.498]
% White	0.482	0.624	0.509	0.549
	[0.500]	[0.484]	[0.500]	[0.498]
% Black	0.221	0.082	0.077	0.077
	[0.415]	[0.274]	[0.266]	[0.267]
% Hispanic	0.083	0.063	0.059	0.061
	[0.277]	[0.242]	[0.235]	[0.239]
Missing SAT	0.019	0.007	0.006	0.009
	[0.135]	[0.082]	[0.077]	[0.092]
SAT (math + verbal)	1178	1306	1312	1307
	[244]	[185]	[188]	[198]
N of 2015 applicants	539	2078	4755	7391
School characteristics (2014-15 academic year)				
% Students applying to UVA	0.035	0.062	0.076	0.074
	[0.024]	[0.061]	[0.073]	[0.071]
% applicants accepted to UVA	0.451	0.357	0.455	0.411
	[0.306]	[0.259]	[0.197]	[0.232]
% Students filing FAFSA (March 1)	0.271	0.337	0.326	0.327
	[0.070]	[0.175]	[0.090]	[0.104]
% UVA applicants filing CSS	0.374	0.225	0.314	0.267
	[0.296]	[0.227]	[0.186]	[0.304]
% UVA admits filing CSS	0.794	0.594	0.673	0.624
	[0.262]	[0.328]	[0.227]	[0.337]
Graduation rate	0.876	0.906	0.919	0.917
	[0.046]	[0.061]	[0.045]	[0.047]
FRPL %	0.612	0.977	0.313	0.610
	[0.126]	[0.088]	[0.126]	[0.349]
Student/counselor ratio	285	278	300	297
	[68]	[54]	[112]	[106]
School enrollment	1064	1064	1299	1269
	[618]	[635]	[685]	[682]
N of schools	58	155	192	555

Notes: Standard deviations in brackets. SAT, student/counselor ratio, and enrollment rounded to nearest whole number. Other values rounded to three significant digits. FRPL refers to students eligible for free- or reduced-price lunch. This table compares the average characteristics of schools selected for the 2015 pilot of the text message intervention and the schools that received text messages as part of the 2016 rollout of the program. As a result of merge limitations between UVA student-level and Virginia school-level files, not all schools have a FRPL value, and thus the "all schools" column includes more schools than the sum of high- and low-FRPL schools.