HERE TO STAY? ASSESSING THE RELATIONSHIPS BETWEEN COLLEGE ACCESS PROGRAMS AND COLLEGE ENROLLMENT AND PERSISTENCE OUTCOMES OF UNDERREPRESENTED STUDENTS

by

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(Under the Direction of George C. Spencer)

ABSTRACT

Pre-college access programs in high schools commonly focus on assisting underserved and historically excluded populations of students in preparing for, applying to, and enrolling in postsecondary education. Through two analyses this study examines the efficacy of two types of access programs in facilitating postsecondary enrollment and persistence outcomes for students and schools served by access programs, over the past two decades. Using data from the Education Longitudinal Study of 2002, the first analysis relies on a propensity score weighting approach to examine college match and fit outcomes when applying to and enrolling in college for students participating in a federal pre-college program (Talent Search, Upward Bound, and GEAR UP). Results suggest that match and fit outcomes of program participants did not significantly differ when compared to similar non-participants. The second analysis examines the impact of three near-peer college advising programs in Michigan between 2007 – 2021. Using a difference-in-difference design, outcomes of partner schools served by programs of the College Advising Corps and Michigan College Access Network are compared to schools located in the same school districts, and to a comparable set of schools identified through Coarsened Exact

Matching. This analysis specifically investigates whether near-peer programs impacted schoolwide graduation rates, college-going rates, and college credit attainment rates of high school graduates. Given few school-level analyses of such programs and the focus these programs place on serving historically excluded populations, outcomes are disaggregated by race and ethnicity and sex to examine whether effects vary by subgroup. The results from the analysis vary contextually by subgroup and school sample, though the most consistent observed effects suggest programs increased rates of overall college-going for female students.

INDEX WORDS: college access, college access programs, college enrollment and persistence, underrepresented students

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DEDICATION

For Kyla J. Relaford and Dr. Michele Carpentier, who gave me the opportunity to go to college.

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CHAPTER 1: INTRODUCTION

Closing gaps in college enrollment and persistence between student subgroups has been a primary goal of many states throughout the nation over the past decade. Despite espoused goals, public schools across the nation have continued to face funding cuts and increased teacher and counselor to student ratios preventing schools from providing adequate support to students through the college admissions process. As recently as the 2015-16 school year, more than 1.7 million students across the nation were attending a school without access to a school counselor (Whitaker et al., 2019). Such conditions seem incongruent when juxtaposed against the emphasis placed by the federal and state governments on improving college enrollment and completion rates. This is particularly the case with researchers noting the important role played by school counselors in the postsecondary planning and application process. In lieu of counselors who until recently have been required to manage increasingly larger student caseloads, high school based pre-college programs have aimed to fill the gap in assistance for students. Despite the lengthy history of some of these programs, the empirical research on program efficacy leave several questions unanswered about whether different program structures and approaches are able to improve college enrollment and persistence outcomes by focusing on college match and fit of students during the application process.

Purpose of the Study

Literature examining issues of college access for marginalized and historically excluded populations has argued that pre-college access programs are intended to transmit social and cultural capital to the student populations served, with the most common function being assisting students through the college application process (Bettinger & Evans, 2019; E. W. Brewer & Landers, 2005; Cahalan & Goodwin, 2014; Duncheon & Relles, 2019; Epps et al., 2016; Harris & Nathan, 2014; Horng et al., 2013; Pitre & Pitre, 2009; Plank & Jordan, 2001). While access programs are aimed at helping students enroll in college, fewer studies have examined *where* participants enroll and *if* they remain and persist in college.

Research Questions

This empirical research aims to contribute to the literature by addressing this gap in the literature and is primarily motivated and guided by three overarching questions. Specifically, if pre-college access programs are designed to help students prepare for and enroll in – and hence *access* college:

1. Does the structure and approach of pre-college access programs influence outcomes for program participants?
2. What kinds of access do pre-college programs provide to student participants?
3. Do some students stand to benefit more than others from this assistance?

These three ‘big-picture’ questions influenced the overall design of this study and helped shape two additional sets of directly testable research questions. For context, **Table 1** provides an overview of the influence of these three questions and the research questions presented in the two analyses. I briefly discuss the relevance and role of these questions in informing this dissertation research.

Table 1
Overarching Questions Influence on Study Design and Analytic Research Questions

Overarching Guiding Question	Component of Research Design Influenced	Analysis 1 Participation in TRIO/GEAR UP and Student Match and Fit Outcomes	Analysis 2 Near-Peer Programs and Partner School Outcomes
Does the approach and structure of access programs influence outcomes for program participants?	Approach	Targeted Students / Cohort	All students; schoolwide.
	Structure	Students can participate over an extended period (e.g., multiple years).	The adviser primarily works with senior students though they may also work with younger students.
	Social Capital Mechanism	Program Staff, structured activities	Near-Peer Adviser
			RQ1: Do near-peer programs raise college-going culture/aspirations by improving school graduation rates?
What kinds of access do pre-college programs provide?	Considering college-going outcomes related to identifying: threshold access or qualified access, as conceptualized by Adelman (2007): Plainly, do programs improve: access to general enrollment (simply increasing college participation); or, access to 'match' institutions?	Is participation in a federally funded college access program associated with: RQ1: A student academically matching at both the point of applying to and enrolling in college? RQ2: Improved financial fit at the point of enrolling in college?	Do near-peer programs increase rates of: RQ2: College-going? RQ3: % of enrollees attending a four-year institution? RQ4: % of enrollees who earn 1 year of college credit (24 credits) in their first year 1?
Do some students stand to benefit more than others from this assistance?	Are effects equitably distributed among subgroups or are there differential effects by subgroup?	Two-thirds of participants in the federal pre-college programs are required to be either low-income or first-generation students.	The RQs noted are extended and ask whether effects differ by race and ethnicity and sex. Outcome data reflect this concern and are disaggregated to investigate whether potential effects vary between groups.

NOTES: RQ = Research Question

The first question seeks to examine how program design influences potential outcomes of students served by programs. While pre-college programs generally share commonalties in mission and goals, the way programs are organized and work with students in schools can vary substantially. Previewing the programs discussed in the following section, the pre-college federal access programs for example are often targeted interventions, requiring students to be either first-generation or low-income students. These programs range in how many students receive services, though they are commonly only offered to a subset of students enrolled in a given school. Moreover, students can participate in some of these programs in as early as middle school and over many years depending upon the program. Programs are operated by professional staff trained on topics relevant to college preparation, conducting searches, evaluating offers, and so on. This differs from near-peer programs where recent college graduates serve in high need schools helping students through the entirety of the application process. Though advisers generally perform schoolwide outreach, depending upon the school size and adviser capacity an adviser may work with students in all grades, though it is more common for the majority of an advisers time to be spent working with senior students. Other pre-college programs may focus more heavily on academic enrichment activities in specific areas (STEM instruction for example) instruction designed to supplement the school curriculum. Thus, this research seeks to examine whether how program approach and structure influence outcomes, by focusing attention on two types of programs – targeted federal interventions, and near-peer programs.

The second question serves to augment the first by leveraging differences in the program approach and structure to whether these differences affect the kind(s) of access participating in a program provides. In other words, do access programs provide what Adelman (2007) termed "threshold access" (e.g. walking through the door, or simple enrollment in any college), or can

they help students enroll in institutions which appropriately match their academic ability and promise? Targeted interventions permitting students to participate over an extended period of time, for example, may be able to improve the academic match between students and the institutions they choose if extended exposure to information through staff can influence student decision-making and choices during the enrollment process. This may be the case especially in such circumstances where students are able to thoughtfully engage with the application and admissions process rather than receiving help only for a shortened period of time during the senior year. Though near-peer programs often rely on working primarily with senior students, this question attempts to examine whether the specific element of being advised by a more relatable near-peer rather than an older adult results in a improved match and fit as a result of following through on the advisers guidance.

The final question posed seeks to examine whether program benefits generally accrue to all students served, or if impacts of programs depending upon student characteristics, along the lines of race and ethnicity and sex. Assuming participant outcomes are largely the same may overlook important differences not only in such outcomes between groups, but also may ultimately help inform policy and practice with respect to program delivery and approach.

Persistent Gaps in College Enrollment and Persistence

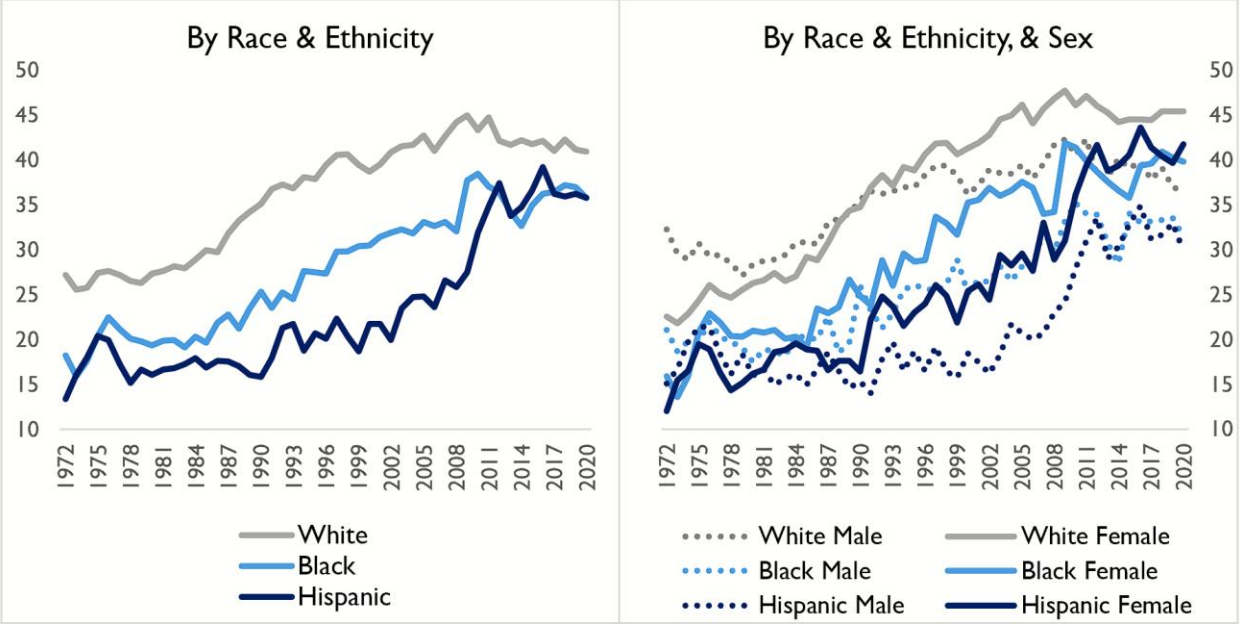
Since the creation of the first college access programs, college access has expanded considerably, and for the student populations these programs target. In the period between 1975 and 2016, the college enrollment rate[1] of recent high school graduates[2] from low-income

[1] Based on a 3-year moving average (the current year, and the years immediately preceding and following the current year).
[2] Defined as individuals between the ages of 16 and 24 who graduated from high school or completed a General Education Diploma or equivalent credential earlier in the same calendar year.

backgrounds[3] nearly doubled, from 35 to 67 percent (NCES, 2017). Though stubbornly persistent disparities have remained between high and low-income student populations, these increases in college enrollment are promising for the populations typically targeted by access programs[4]. Similar increases have been observed in the college-going rates of individuals aged 18-24, though gaps have remained between White students and their Black and Hispanic peers. Strikingly, as shown in Figure 1, differences in college-going even within these subgroups have emerged with Female student enrollment outpacing their Male peers.

Figure 1

Percentage of 18- to 24-year-olds enrolled in college: 1972-2020



SOURCE: National Center for Education Statistics. (2021, August). Table 302.60. Percentage of 18- to 24-year-olds enrolled in college, by level of institution and sex and race/ethnicity of student: 1972 through 2020. NCES Digest of Education Statistics; National Center for Education Statistics.

[3] Defined as students whose family income was in the bottom 20 percent.

[4] The 2015 average for low-income students (64.0 percent) is particularly striking for two reasons: it is simultaneously the first year where the average for low-income students ever exceeded that of middle-income students (63.6 percent), and also exceeded the rate for high-income students (63.7 percent) - in 1975 (by 2015, 83.2 percent of high-income students were enrolled in college). Over the same time period, when disaggregated by race and ethnicity, the enrollment rates for White students grew from 49 to 70 percent, slightly outpacing growth for Hispanic or Latinos (from 53 to 68 percent), and well ahead of Black enrollment (45 percent to 58 percent).

Moreover, concern has grown with respect to retention and completion which show persistently large gaps between student groups. In the period between the 2006-07 and 2019-20 academic years, roughly 20 percent[5] of first-time undergraduate students who enrolled at four year institutions did not return for their second year (NCES, 2021). The disparities in retention are uneven across college selectivity, with non-selective four-years retaining only 62 percent of first-time students compared to the 96 percent retention rate at institutions who accept fewer than one out of every four applicants. Fewer still of these students graduate within a traditional window of time with only slightly more than half of students (54.8 percent) who enrolled at a four-year college in Fall 2010 completing a credential within six years. Disparities in these outcomes disproportionately negatively impact Hispanic or Latino and Black students whose completion rates were significantly lower than their White and Asian peers (Shapiro et al., 2017). Thus, the current study is in part motivated to investigate whether college access programs are capable of reducing the stratification in outcomes of enrolling at different levels of selectivity. Are these programs able to improve match and fit outcomes of participants, and increase persistence as a result thereof?

To investigate these questions, this study undertakes two analyses investigating postsecondary enrollment outcomes of access program participants. In the subsections which follow, I provide background and context on the programs of interest in the current study before briefly detailing the analyses conducted of each program, beginning with the first college access programs, the federal TRIO programs.

[5] Approximately 19.8% based on the author's calculations based on the average "Percent of first-time undergraduates retained" for all periods from 2006-2007 to 2019-2020 from Table 326.30 of the Digest of Education Statistics (NCES, 2021).

Background Context of the Federal TRIO and GEAR UP Pre-College Access Programs

The passage of the federal Economic Opportunity Act (EOA) of 1964 signaled a shift in policy priorities of the federal government, aimed at addressing and improving conditions of impoverished communities across the United States. Until passage of the EOA, the federal role in education had, largely been limited to a few notable interventions, including the Morrill Land Grant Act and the Serviceman's Readjustment Act of 1945 (Butler, 2016; Groutt, 2003; Mumper et al., 2016). The federal role in education drastically changed with the EOA, as the government argued expanding access to education, and higher education specifically was the primary tool with which to combat conditions of poverty throughout the nation. The belief in postsecondary education as a route to improve ones' economic conditions spurred the government to develop programs and initiatives focused on providing support in pre-college preparation and activities to impoverished communities.

Borne out of the EOA, the first pre-college access program, Upward Bound (UB), was designed to identify academically underachieving low-income students as well as ethnically underrepresented students and provide them with social and academic environments and support services with the goal of motivating and ultimately preparing them to pursue postsecondary education (Groutt, 2003; Pitre & Pitre, 2009). During the Summer of 1965 the 17 UB pilot programs, served more than 2,000 high school students. Passage of the Higher Education Act later that year saw the creation of Educational Opportunity Grants, designed as the first federal scholarship grant specifically to be awarded to students from low-income backgrounds. Noting the goal of the HEA was to continue to expand postsecondary access by bolstering academic preparation and financial resources, President Lyndon B. Johnson argued it was "an incentive to stay in school. It means that a high school senior anywhere in this great land of ours can apply to any college or any university in any of the 50 states and not be turned away because

his family is poor" (Johnson, 1965). The second federal college access program emerged as a consequence of the difficulty the federal government found in distributing the funds for said scholarship. As Groutt (2003) describes, the program architects

> *"knew that very few low-income students participated in higher education. No one expected them to. Neither high school nor college personnel had experience working with diverse populations of youth in higher education… [they] saw that a large pot of money would go largely untouched due to lack of experience on the part of the students, high school counselors, and colleges" (Groutt, 2003, p. 3).* Seeking to promote the availability

of said funding, the *Contracts to Encourage the Full Utilization of Educational Talent* program was created (later named Educational Talent Search or ETS). The program served as a supplement to UB programs at the high school level, while the Special Services for Disadvantaged Students (now, Student Support Services) was created to operate at postsecondary institutions. Together, the three programs became known as the federal TRIO[6] programs, representing concerted efforts to encourage disadvantaged students to pursue a college education, so as to improve their own economic conditions. The creation of these programs, spurred by President Johnson's focus on the War on Poverty sought to democratize the ability of Americans of all backgrounds who wanted a college education to be able to attain one. Federal efforts to continue expanding access to pre-college higher education programs continued in 1998 through the creation of the federal Gaining Early Awareness and Readiness for Undergraduate Programs (GEAR UP) program. Though the GEAR UP program operates as a cohort model often serving either entire schools or specific grades within a school, the program was designed to operate

[6] TRIO refers to the fact that there were originally *three* federal access programs.

similarly to and achieve goals similar to ETS and UB in preparing student to pursue postsecondary education beginning while they are in middle and high school.

The first analysis focuses on the three federal pre-college access programs previously described (ETS, UB, and GEAR UP), and investigates whether participation in one of these programs is associated with improved match and fit outcomes of participants. This analysis utilizes data from the Education Longitudinal Study of 2002 (ELS:2002) , which followed a cohort of then tenth graders through their high school careers and into postsecondary education and examines whether students who participated in these programs at any point during high school were more likely to apply to and enroll in a postsecondary institution of selectivity at least matching their academic abilities. Additionally, this analysis also investigates whether participation was associated with a better financial fit for program participants. This analysis serves to add to the literature examining mechanisms which mitigate and/or address concerns of academic undermatching among students deemed 'at-risk', and who according to the literature would most stand to benefit from this type of intervention.

Background Context of Near-Peer College Advising Programs

To complement the federal programs, over time a number of new variations of college access programs began emerging across the nation, including near-peer access programs which placed recent college graduates in high schools to help students navigate college admissions. The National College Advising Corps (CAC) was one such program, and in 2004 CAC placed fourteen near-peer College Advisers[7] in high schools in the Charlottesville Virginia area, with the primary goal of assisting low-income and underrepresented students through the college application process. Under the near-peer model utilized by CAC recent college graduates who

[7] Then known as College Guides.

have earned a four-year degree (and who are close in age to high schoolers, having been no more than six years removed from their own high school graduation dates, hence ‘near-peer’) to work in high-need high school settings. College advisers typically work in partnership with school staff including school counselors and serve as the primary school personnel tasked with helping senior students conduct college searches, complete college and financial aid applications, and work through all facets of the college-going process. Advisers are encouraged to develop rapport with students given similarities in age and often background (Adams, 2015). In 2007, a generous philanthropic gift from the Jack Kent Cooke foundation, enabled the program to undergo a national expansion. The program changed its name to the College Advising Corps and began establishing partnerships with colleges and universities across the nation to establish CAC chapters intent on serving high need high schools with large student to counselor caseloads. The program continued its growth through the Great Recession and by the 2010-11 school year had developed partnerships with 15 colleges and universities across the nation, serving high schools in fourteen states (Supiano, 2011).

The second analysis of this dissertation examines three access programs operating under the ‘near-peer’ model of pre-college advising in the state of Michigan: the Michigan College Advising Corps established in 2010, the Michigan State University College Advising Corps started in 2011, and the Advise-Michigan program which began operating in 2015. Leveraging a school level panel dataset of Michigan high schools between 2007 through the class of 2021, this analysis uses a difference-in-difference design to identify the impact of these three programs on several partner school college-going outcomes including: high school graduation rates, college-going overall, proportional four-year college going, and rates of credit attainment of enrollees. In order to distinguish and identify whether programs have differential effects on varying student

subgroup populations, this analysis disaggregates these school level outcomes by race and ethnicity and sex.

To contextualize and situate the two analyses, the next chapter reviews the theoretical and empirical literature which guide this research, including a review of literature focused on pre-college advising, the role and impacts of pre-college access programs on college enrollment outcomes, and emergent literature on academic match and fit. Following the review of theoretical and empirical literature, I present both analyses of the current study and conclude with a discussion of findings and implications.

CHAPTER 2:

REVIEW OF THEORETICAL AND EMPIRICAL LITERATURE

This chapter provides a review of the empirical literature related to issues of postsecondary access and enrollment, for students who have been historically excluded. The chapter begins with an overview of the social capital theoretical framework which guides this study and the broad research conducted on college advisement of high schoolers within the high school context. Following this, I review empirical research specifically investigating the impact of college access programs on college enrollment outcomes, and thereafter review literature concerned with a specific subset of college enrollment literature focused on academic undermatching and the concept of *match and fit*.

Theory of Social Capital

For the current study I rely on both Bourdieu (2002/1986) and Coleman's (1988) theories of Social Capital to frame the work of college access programs in serving marginalized populations. While I rely on both to frame the study, it is important to distinguish the two theories are largely not interchangeable, and offer distinct, and in some cases competing narratives. It is helpful, however, to begin with where the two theories agree and share commonalities in assumption and premise. Though they discuss the concept differently, both Bourdieu and Coleman’s conceptualization of social capital frame it as investment in social relations/relationships and the resources that derive from and are contained within said social relations, with an expectation that such resources will benefit the actors involved (Portes, 1998; Rogošić & Baranović, 2016).

At the broadest levels, social capital can serve to be a source of social control or social support (whether originating internally or externally from ones' family network) (Portes, 1998; Rogošić & Baranović, 2016). The latter element in particular is widely used in educational research to explain how an individual's network of connections can help the individual achieve social mobility through providing access to such support (e.g., social capital). For both Bourdieu and Coleman, unlike other forms of capital (e.g., economic, human) which are held or within the individual, social capital is embedded within the social structure *around* the individual. Said differently, to have social capital, one *must* be related or connected to other individuals, and these individuals and connections are ultimately the *source* of such capital. Within the context of the current study, the most relevant form social capital can take is as an information channel where information is transmitted from an individual to another. The approach generally undertaken by college access programs including those in the current study involve access program staff (e.g., professional staff members of a federal program or a near-peer college adviser) scheduling one-on-one or group meetings with students, covering a range of topics on the college-going process. Such meetings typically include discussions of college types, college majors, scholarship opportunities, entrance exam scheduling (ACT/SAT) and requirements (where applicable), financial aid requirements, and other components of the application process (Bettinger & Evans, 2019; Gurantz et al., 2020; Horng et al., 2013).

While this general understanding underlies the theoretical framings, researchers have occasionally conflated the social capital theories of Bourdieu and Coleman as Musoba and Baez (2009) detail, despite the two perspectives largely diverging from this general point. Bourdieu's conceptualization is focused on how privileged groups employ social capital as a tool and strategy within the context of cultural capital more broadly to create distinctions and hierarchical

structures which aid in maintaining their position. Bourdieu understood social capital as the whole of resources (information and otherwise) that were available to the individual *as a direct result* of their family ties, and social obligations, network, and connections (Bourdieu, 2002). Though its nature is fluid, social capital is based on symbolic exchanges between agents, representing all available resources – whether realized or potential, within ones' network and based on social connections. This social capital, attained through membership in an exclusive group, provided unto the individual the legitimacy associated with recognition as a group member, while simultaneously reinforcing the values and norms associated with group membership. Social capital acquired through membership in the group could then be used by the individual, who in addition to converting said social capital to the other forms of capital (economic and cultural), could also help redefine and reproduce the group by conferring membership to new individuals. Explaining the value of social capital as he saw it, Bourdieu argued "[e]xchange transforms the things exchanged into signs of recognition and, through mutual recognition and the recognition of group membership which it implies, re-produces the group. ..[e]ach member of the group is thus instituted as a custodian of the limits of the group: because the definition of entry is at stake in each new entry, he can modify the group by modifying the limits of legitimate exchange" (Bourdieu, 2002, p. 287).

The approach undertaken by Coleman (1988) focuses attention on understanding the impacts of social capital on explaining differences in outcomes between individuals, and does not attempt to examine to contend with the notions of social reproduction advanced by Bourdieu. Coleman argued social capital was borne out of an interaction between an individual, their family, and their school, shaping the flow of information between individuals, establishing norms and imposing behavior regulating sanctions. Such forms of capital were reliant upon the

in-group trust, obligations, and expectations between members, with much of an individuals' social capital deriving from their family background and school community. This framing diverges from Bourdieu's, arguing that rather than inhering itself within the individual (as Bourdieu would argue it in part causes ones' *habitus* to do), social capital structures relationships and networks available to one, and as such individuals *providing* social capital to those without it should enable the individual to increase their own social capital and achieve social mobility.

Thus, from the contrasting perspective, Coleman's theory attempts to position social capital not as a constraining force, but as one which if provided to individuals could promote social mobility. Education researchers have largely relied on Coleman's approach to explain difference in educational achievement and trajectories, arguing school agents can serve as sources of social capital particularly during the college advising process for the students they serve, helping to provide tools which will help the student achieve social mobility by guiding students to consider and ultimately enroll in postsecondary education.

College Advising in the High School Context

A robust literature has examined the impacts of the high school context on how students prepare for and navigate their postsecondary transitions. Research has demonstrated that college preparation and advising efforts differ substantially between high schools (Engberg & Gilbert, 2014; Engberg & Wolniak, 2010, 2014; Hill, 2008), with respect to school type (Lee et al., 2017), how college information is organized, brokered, and disseminated (Hill, 2008; Weis, 2016), the school staff (primarily school counselors) tasked with overseeing college advising efforts (Belasco, 2013; Bryan et al., 2009, 2011; Farmer-Hinton, 2008; Farmer-Hinton & Adams, 2006; Holland, 2015; Karen Jeong Robinson & Josipa Roksa, 2016; McDonough, 2005b; McDonough & Calderone, 2006; Perna et al., 2008; Robinson & Roksa, 2016), and in the case of public schools the increasingly large student caseloads said counselors must manage (Hurwitz &

Howell, 2014; Vela-Gude et al., 2009; Woods & Domina, 2014). The concern with the latter has especially troubled scholars who argue that school counselors as sources of social capital will be unable to adequately provide college application guidance to students who might most benefit from such assistance if their student caseloads become unmanageable. This argument often contends students who would most benefit from assistance would be disproportionately negatively impacted in the absence of such assistance, particularly in circumstances where the student has no relatives or close others who have completed college and would be able to help guide them through the application process. Though the American School Counselor Association recommends a ratio of 250 students for every 1 counselor[8] (ASCA, 2022b), ratios nationally have significantly exceeded such thresholds from 588:1 in 1986 to 415:1 in the 2020-21 school year (ASCA, 2022a). Notably, the ASCA also recognizes that ratios vary widely, by grade level, socioeconomic status (of students and schools overall) and by state (ASCA, 2022b; Bridgeland & Bruce, 2011).

Pre-College Access Programs

Similarly, the underlying premise of providing social capital through additional support is arguably the basis for access programs as they were initially conceived, as students from backgrounds where no relatives or close others had pursued postsecondary education are at an information disadvantage relative to their peers. While empirical literature on high school contextual environments has found high schools can serve as postsecondary feeder networks (Engberg & Wolniak, 2010) with schools employing ‘college-linking strategies’ by serving as information brokers, or clearinghouses of information for their students (Hill, 2008), pre-college access programs working at high schools often have a similar set of goals in helping a targeted

[8] Notably, the 250 student threshold recommendation is largely made absent evidence or empirical research specifically focused on such a threshold (Kearney et al., 2021).

group of students meet markers throughout the postsecondary transition process. In particular, these programs are designed to serve low-income and disadvantaged students regardless (and often in spite of the lack of) of resources available in high schools. The logic underscoring the models suggests providing students with dedicated individuals in an access program *provided* social capital, chiefly in the form of information on preparing for, applying to, and choosing a college. Some programs also provide additional academic enrichment activities including tutoring, exam preparation, after-school and summer programming, and peer mentoring opportunities (Gandara, 2002; Swail & Perna, 2002).

Despite (and perhaps due to) the prevalence of college access programs across the nation, there is a surprisingly small literature base which provides an overview of the landscape of these programs. This is in part a consequence of the decentralized nature of the education system throughout the United States, which does not require all access programs to report operating statuses, student enrollment, outcome metrics, and other data to a single entity. While data is collected for the federal TRIO and GEAR UP programs, other access programs have been developed and operated by community groups, non-profit organizations like Americorps, colleges and universities, state legislatures, and combinations of these entities, typically serving high schoolers and high school age populations (Fung & Swail, 2011; Swail & Perna, 2002; Tierney et al., 2009). New programs have and continue to emerge to serve more students who may qualify for the federal programs but are not served for one reason or another (e.g. programs are not offered in their schools or community, programs are at capacity, and so on), with earlier research estimating federal programs served less than 10 percent of students who were eligible for these programs, nationwide (Swail & Roth, 2000). Beyond the federal programs some well-known pre-college outreach programs similar to those studied in the current study include MESA

(Mathematics, Engineering, and Science Achievement), AVID (Advancement Via Individual Determination), College Possible, Bottom Line, and Beyond12, among others. Notably, a 1999 National Survey of Outreach Programs yielded a response from 1,110 programs nationwide, and found the federal TRIO and GEAR UP programs accounted for less than half of programs offered across the nation, at roughly 42 percent of respondents (Swail & Perna, 2002). A follow up survey of outreach programs was conducted in 2010 to identify program goals, services, and strategies for access programs across the nation. The top three goals agreed upon by nearly all survey respondents noted their program goals included providing exposure to college (91.2 percent), increasing college awareness (92.0 percent), and increasing the likelihood of attending college (92.8 percent). Notably, the fourth goal programs noted was to increase the likelihood of completing college (79.9 percent) (Fung & Swail, 2011).

While access programs generally seek to achieve similar goals, there is surprisingly little uniformity as to how programs are organized and operate. Results from the 2011 Survey of Outreach Programs found 61 percent of programs required students to apply to the program, with only slightly over half of programs noting they target outreach towards specific student groups (52.7 percent). More than a third of programs noted the program duration was intended to last more than one academic year, and only 20 percent required parents to engage with the program in a formal capacity. Instructional offerings vary as well with some programs offering workshops (85 percent), mentoring (68 percent), classroom instruction and tutoring (nearly two-thirds each), and peer learning groups (44 percent) among others (Fung & Swail, 2011).

These differences serve to illustrate the great variation in programmatic structures, goals, and activities and approaches. This is similarly true of the federal programs which are grant funded and allow grantees flexibility in running day to day program activities, including in

establishing eligibility criteria above and beyond the federally mandated student requirements. Some programs may work with all students in a given school, or a particular grade level (commonly juniors and seniors preparing for their postsecondary transition) or cohort (like GEAR UP programs). Such differences between program sites (e.g., schools) have made conducting evaluations of these programs difficult as sites may impose different eligibility criteria for participation above and beyond the baseline federal requirements.

Impacts of Trio & GEAR UP on College Outcomes

Difficulties in evaluation notwithstanding, despite the lengthy history of the pre-college TRIO programs and the more recent creation of GEAR UP, empirical evaluation of these programs and their effectiveness is surprisingly limited. The most well-known evaluation of Upward Bound was a longitudinal randomized control trial of Upward Bound commissioned by ED and conducted by Mathematica Policy Research, inc. beginning in 1991 (Myers et al., 1999, 2004; Seftor et al., 2009). To assess effects of Upward Bound programs, a nationally representative sample of 67 UB programs hosted by two and four-year colleges and universities were chosen to randomly assign eligible applicants to either participate in UB (the treatment condition), or to not participate (the control group). Students were considered eligible to participate in the study if they were low-income, potential first-generation college students, and if they had been recruited to apply to involve in UB. In rare instances, students were exempted from the randomization process where it would impede normal program operations though these students were excluded from the analysis. In the final report of the study, the authors determined the program had “no detectable effect on overall postsecondary enrollment for the average eligible applicant, and did not affect the types of institutions eligible Upward Bound participants attended, either in level or selectivity” (Seftor et al., 2009, p. 41).

Since their study was published, other researchers have critiqued it for issues with sampling weights (most notably a single project site (project site 69) accounting for more than a quarter of the sample), questions of whether randomization procedures were faithfully implemented, and other instances that affected the reliability of results (Cahalan & Goodwin, 2014; Harris & Nathan, 2014). Additionally, researchers re-analyzing the project data found students were "placed in some other similar service precisely as a substitute for not being randomly selected to be given the regular UB program opportunity" (Cahalan & Goodwin, 2014, p. 18) thereby violating the receipt of treatment being given to only the treatment group. A re-analysis of the original RCT data found a 6.9 percentage point intent-to-treat difference in college enrollment (73 percent of participants enrolling in college within 18 months, compared to 66 percent of control group students) and an 11 percentage point treatment on the treated difference – both findings being significant at the $p < .001$ level (Cahalan & Goodwin, 2014). Additionally, after controlling for pre-selection characteristics, their results found that participants were 3.3 times more likely than non-participants to have obtained a Bachelor's degree within six years (Cahalan & Goodwin, 2014).

As was attempted with the Upward Bound study, the most rigorous evaluation of the efficacy of college access programs broadly, and the federal programs specifically, would use random assignment to assess causal impacts of program services and participation on student outcomes. Among the challenges researchers face in evaluating the federal college access programs are the practical limitations and ethical considerations in designing randomized control trials that provide program services at random to students, while denying services to disadvantaged students who might most benefit from them. Indeed, even within the UB study, students who were not assigned to the UB treatment group were often placed in other similar

opportunities *because* they did not receive the treatment (Cahalan & Goodwin, 2014). Given the practical and ethical issues related to designing and implementing randomized control trials, many researchers instead rely on quasi-experimental statistical approaches that attempt to provide more appropriate and comparable comparison groups of students to program participants, and control for other possible confounding variables (Bowman et al., 2018; Constantine et al., 2006; Domina, 2009; Glennie et al., 2015).

Constantine et al. (2006) conducted a multi-state evaluation of the effect of Talent Search projects on financial aid application and college enrollment outcomes of participating students by using propensity score matching to match participants to similar non-participants based on baseline characteristics. Their evaluation began by collecting data on ninth graders in 1995-96 in the states of Florida, Indiana, and Texas who participated in Talent Search. Talent Search participants were matched with a comparison control group of similar students attending the same schools but who did not participate in these programs despite having similar propensities to have participated. Across the three states, results suggest that when compared to their similar non-participating peers, Talent Search students had financial aid application rates between 14 to 28 percent higher on average and enrolled at in-state institutions at rates 6 - 18 percent higher than non-participants.

Despite the large number of pre-college access programs (particularly the federal programs) operating at high schools across the country, only limited data on these programs have been made available for researchers to conduct empirical studies on student outcomes (McCann, 2019; Swail & Perna, 2002). Given limitations in availability of administrative data of programs, researchers have relied on college access program data collected included in national longitudinal studies conducted by NCES. In one of the earliest studies to do so, Domina (2009)

constructed two quasi-experimental analyses using data from the Education Longitudinal Study of 2002 (ELS:2002) to examine how program structure and targeting strategies impacted outcomes for participants *and* non-participants. ELS surveyed and followed a nationally representative cohort of tenth graders in 2002 through their high school and postsecondary trajectories, surveying students in the cohort as well as their parents/guardians, teachers, school counselors, and school administrators. In the first follow-up of the study in 2004, when most students were in the twelfth grade, students and school administrators provided data on participation in college access programs. Students were asked whether they participated in an access program (Talent Search, Upward Bound, or GEAR UP) at any point during high school, and school administrators indicated whether schools had one of these programs, and if so whether the programs performed schoolwide or targeted outreach to students. In the first quasi-experiment conducted using this data, Domina (2009) utilized propensity score matching to construct a comparison group of students who were similar to program participants but who themselves never participated in one of the programs. Restricting the sample to students who were enrolled in public high schools during the first two survey waves (in 2002 and 2004), a logistic regression was used to predict students' odds of participation in a program conditional upon demographics (race and/or ethnicity, first language, parental educational attainment, family income, scales of school engagement, school trouble, family resources, etc.), academic characteristics (GPA during ninth grade, test scores, hours spent on homework) and school level characteristics. Thereafter, each program participant was matched with up to three students who did not participate but whose propensity scores were within .01 of their own. With the exception of slightly lower scores on a standardized mathematics assessment, the analysis found outcomes

of program participants were not significantly different compared to non-participants, including with respect to college enrollment overall, and enrolling in a four-year college specifically. Arguably, the most notable findings from the analysis came from the matching model itself. Specifically, despite the inclusion of a large number of conceptually and theoretically relevant predictive covariates, the matching model itself was found to explain less than one-fifth of the between-student variation of participating in an access program ($R^2 = .19$). Moreover, the average probability of participating in a program – was .135 for participants and .046 for non-participants. These estimates suggest there may be great variation in eligibility requirements, outreach efforts, or criteria considered for program participation above and beyond the large number of factors included in the models. This is striking as the predictive model accounted for demographic, academic, school context, and several measures of social capital, which may suggest either omission of significant variables or perhaps considerations for participation that diverge from the target populations. The second quasi-experiment of the study investigated potential spillover effects of access programs that conduct schoolwide outreach on outcomes for non-participating students. The matching strategy and procedure was employed to match students who attended a school with an access program that conducted schoolwide outreach (*but who themselves did not indicate participating in a program*) to similar peers at non-schoolwide outreach program schools, though again no significant differences between groups was found with respect to enrollment in college.

In a study similar to the first quasi-experiment conducted by Domina (2009), Glennie et al. (2015) also used a PSM strategy with the ELS:02 dataset to examine the effects of participating in a TRIO or GEAR UP program on a range of college admission process outcomes including taking a college admissions exam, exam scores (both standardized and college

admissions), seeking college information, submitting college and financial aid application, being offered financial aid, and attending: any college, a four-year, full-time, and finally persistence through the second year. Their study examined whether program participation was associated with taking a college admissions test, seeking college information, applying for financial aid, and enrolling in college. While participants and non-participants took ACT/SAT exams at similar rates, participants were significantly more likely to seek information about college, apply to college, and be offered financial aid. Notably, participants and non-participants enrolled in college at similar rates, though participants were less likely to persist into their second year of college (76.6 percent compared to 82.3 percent of non-participants), specifically through to the second semester of their second year.

The analyses conducted by Domina (2009) and Glennie et al. (2015) offer valuable contributions not only in their substantive findings on the limited efficacy of these pre-college access programs in improving specific markers during the admissions process, but also in demonstrating the use of a nationally representative dataset in investigating program effectiveness when compared to students who are similar to participants but who did not participate. Despite such contributions, these studies have notable limitations which impact their generalizability. First, both studies employed mean substitution imputation for background variables with missing data which may have introduced bias to obtained estimates. Additionally, decisions in the incorporation of the complex survey weights are somewhat unclear and may also impact these studies, with respect to constructing appropriate comparison groups as well as the findings. For example, Domina (2009) incorporated the base year student weights in the logistic regression model used to obtain the propensity scores themselves. Propensity score matching is commonly utilized following discovery of existing imbalance in covariates of treated and

untreated groups within a given sample. As such, evolving approaches to matching methods argue that PSM is an *in-sample* concept and thus should not be weighted to be representative of the population (DuGoff et al., 2014). Moreover the research conducted by Glennie et al. (2015) is unclear as to whether and how survey weights were incorporated, though the researchers do identify the analysis utilized radius matching which allowed cases which are most closely matched to contribute more weight to the analysis (based on the propensity score weight). If survey weights were omitted from their analytic models, generalizations would not necessarily represent average program effects for students nationally. Importantly, these investigations rely on older statistical software routines which may have limited the analyses presented by these researchers[9].

In a recent study, Bowman et al. (2018) relied on a difference-in-difference strategy to investigate whether GEAR UP program implementation in Iowa impacted school-level college enrollment and persistence rates. Their study examined college enrollment and persistence outcomes of cohorts of GEAR UP Iowa students compared to other cohorts of students at the same schools who did not receive the treatment, similar to Constantine et al. (2006). Their difference-in-difference estimates suggest that cohorts receiving the GEAR UP treatment experienced a three to four percentage point increase in college enrollment within a year of high school completion relative to non-GEAR UP cohorts, though GEAR UP cohorts were not found to persist in college at rates significantly higher than their peers (Bowman et al., 2018).

The empirical research discussed suggests participation in TRIO and GEAR UP have a varied, though generally positive impact on college preparation and enrollment outcomes. Participating in these programs is associated with increased rates of participation in advanced

[9] Domina (2009) and Glennie et al. (2015) utilized Stata's *prmatch* and *psmatch2* routines, respectively.

placement courses (Bausmith & France, 2012), promoting college seeking information behaviors and submitting greater numbers of college applications (Glennie et al., 2015), and increased rates of financial aid application submission and overall college enrollment (Bowman et al., 2018; E. W. Brewer & Landers, 2005; Cahalan & Goodwin, 2014; Constantine et al., 2006; Fogg & Harrington, 2015; Martinez et al., 2018), though results remain mixed with respect to college persistence outcomes. Though the federal access programs remain the largest in the nation, in terms of number of sites, funding, and scope, they are still unable to serve the entire student population that might benefit from such services and additional support (Bergerson, 2009), which has resulted in an ever-expansive list of access programs working at school sites across the nation. In the next section I review a newer variation on these pre-college programs, developed using a 'near-peer' model.

A Variation in Outreach: Near-Peer College Advising Programs

The 2000's saw the emergence of a new variation on pre-college advising in high schools across the nation based upon a near-peer model of college advising. The emergence of these additional access programs were premised, in part, on the argument that though school counselors were integral to facilitating college-going processes in schools, counseling staff were being required to manage increasingly large student caseloads (Bryan et al., 2009, 2011; McDonough, 2005a; Robinson & Roksa, 2016; Woods & Domina, 2014). Developed in part as a way to help overburdened school counselors with large caseloads, the near-peer advising program model places recent college graduates in high schools to help students manage the college application process (Horng et al., 2013), first with the *Admission Possible* non-profit (now known as College Possible), and followed by the *College Guide* program (now the College Advising Corps). These programs partner with the AmeriCorps service program, placing recent

college graduates in high schools to help students through the college and financial aid application processes. By requiring the college adviser to be a recent college graduate (no more than two years removed from college graduation, and six years from high school completion) the program model aims to provide students with a similar aged peer (hence 'near-peer') who can provide guidance and help in learning about college and navigating the application process. The idea is in part premised in Bandura's Social Learning Theory which posits that "seeing or visualizing people similar to oneself perform successfully typically raises efficacy beliefs in observers that they themselves possess the capabilities to master comparable activities" (1997, p. 87). In discussing the program model and advisers, CAC founder Nicole F. Hurd noted "[t]he advisers feel an obligation. Before they advance, they feel they need to turn back and pull somebody through that door as well…they have credibility because they are so close in age and circumstances to the students" (Adams, 2015, p.46). The utilization of this relational capacity (perhaps its own form of (relational) capital) is leveraged by the near-peer advisers in order to build trust with student participants so as to channel information to students and maintain/build the trustworthiness Coleman (1988) emphasizes as critical in transactions of social capital. Through participation in program activities (e.g., college representative visits coordinated by the adviser, financial aid workshops and other similar activities), and exposure to both role-modeling behaviors of the advisers and acquisition of college knowledge, programs have opportunities to provide to and facilitate between students, the transmission and cultivation of social capital.

The National College Advising Corps (then *College Guide* program), piloted their first cohort of advisers in Virginia in 2004, placing fourteen recent college graduates in partner high schools. The program continued to operate in Virginia high schools and in 2007, after receiving an investment of twelve million dollars for a four-year period, from the Jack Kent Cooke

Foundation and Lumina Foundation for Education, the program was renamed as the National College Advising Corps (NCAC, shortened to CAC in 2013), moved its headquarters to the University of North Carolina at Chapel Hill, and began partnering with Americorps which helped facilitate a national expansion. The rapid expansion of the program was in part fueled by concern over swelling school counselor caseloads nationally, with CAC citing the issue as the first argument when presenting 'the need' for the program:

> *"Nationally, the student-to-counselor ratio is over 482:1, leaving many students with as little as 20 minutes a year with a college counselor. High-need students often lack the guidance and support they need to prepare for college, apply to best-fit schools and for financial aid, enroll and graduate" (College Advising Corps, 2014)*.

The college advisers are intended to serve as a support for students, helping them complete college and financial aid applications, conduct scholarship searches, and navigate other components of the application processes. Importantly, they are intended to supplement and not supplant the work of high school counseling staff. The program has since been scaled nationally, and by the 2019-20 school year employed nearly 800 near-peer advisers in 17 states across the nation (College Advising Corps, 2020).

Similar to the federal TRIO programs, CAC partners with colleges and universities (host chapters), and programs operate at the host institution, with chapter staff recruiting near-peer college advisers and managing local school partnerships to place advisers at their service sites. Unlike the federal programs, near-peer advisers do not serve a target population within the schools they serve, but rather are available to all students, for help with conducting college searches, completing financial aid applications, reviewing application materials, and so on. As the program has operated for a relatively short time (relative to the federal programs), there are

relatively few evaluations of the efficacy of the program, though results are mixed as to program impacts.

As part of a study on school-based counseling efforts, Belasco (2014) used a difference-in-difference design to examine the impact of near-peer program implementation on several school level outcomes including overall and four-year postsecondary enrollment rates, across three states. His study found no discernable effect of the intervention from partner schools in Missouri or North Carolina, though he observed a modest increase in postsecondary enrollment rates of partner schools in Pennsylvania, though limitations on the postsecondary enrollment data used varied by state and likely biased estimation. For example, data for North Carolina omitted data on two-year enrollments, and out-of-state enrollment data were unavailable for both North Carolina and Missouri. Clayton (2016) also studied the CAC intervention, examining the impact of the Carolina College Advising Corps (CCAC) on participation in college preparation courses, SAT test taking, and SAT scores. Results suggested that while CCAC did not impact college preparatory course taking rates, the intervention did appear to result in a 15-point decrease in average SAT scores, likely due to a significant increase of 3.5 percent in the proportion of students who took the SAT exam. While this study also did not have access to postsecondary enrollment outcome data for schools, the increase in SAT participation rates is encouraging as the program may have induced students who might not have originally intended to look at or apply to four-year colleges to do so. Still, the data limitations these earlier studies faced make it difficult to discern the full scope of the interventions effects.

Though these efforts faced limitations in their ability to access data, NCAC did establish a relationship with a research team at Stanford partnering with them to conduct a randomized control trial evaluation of program effects on students enrolled at partner schools in Texas.

Capitalizing on a planned expansion of NCAC partner high schools in the state, the Stanford evaluators worked with the Texas Higher Education Coordinating Board to identify Texas high schools with: at least 35 percent of students receiving free/reduced price lunch, a college-going rate lower than 70 percent, and at least 55 percent of students in a college curriculum academic track. As the Texas CAC program expanded from 15 schools in the 2010-11 school year, to 120 schools in the following year, their evaluation of the "Advise Texas" program (chapter of the NCAC) was able to leverage a large sample of students, examining postsecondary enrollment and persistence outcomes for more than 38,000 students. Using the intent-to-treat as their estimand, though they found no impact during the initial year on the college going outcomes of Black students, nor on four-year enrollment overall, the evaluation did find a modest increase in overall two-year college enrollment, of 2.4 percentage points in the implementation year, a marginally significant impact on enrollment of low-income students of two percentage points, and a larger 3.4 percentage point effect for Hispanic students. Examining pooled three-year average outcomes following program implementation, they found insignificant program effects on postsecondary outcomes, including persistence of enrollees. Though their pooled results showed no significant impact on outcomes, the evaluators noted a large amount of non-compliance with respect to treatment assignment conditions, as by the 2015-16 school year, 20 of the 36 schools originally assigned to the treatment condition no longer had an adviser, while 17 of the 75 control schools had an adviser, which ultimately may have contaminated some of their observed results. Given that this newer variant of college access programs offer similar services to students though offering services to all students in a school rather than a targeted population, near-peer adviser outreach is critical to reaching and meeting those who may most benefit from outreach but who are least likely to seek it out. The latter point is particularly important as a joint

effort conducted by the College Board and NCAC evaluated the impact of an offer of virtual advising with a CAC adviser (Gurantz et al., 2020). Their evaluation used a sample of 16,000 high achieving students from low- and middle-income backgrounds, randomly assigning students to receive an offer of treatment by a CAC adviser. Advisers worked with students in broad geographic regions, as opposed to a single high school, offering one on one advising with their assigned students in sessions conducted remotely over the web. Advisers helped students with general college planning and the completion of college applications. Additionally, advisers were directed to promote exploration of and enrollment in a specific group of colleges with high graduation rates[10]. Results from the evaluation offer mixed evidence of effectiveness. Approximately 44 percent of students took up the treatment offer, and while the treatment was determined not to have impacted overall college enrollment rates, being offered virtual advising did appear to increase enrollment in designated high graduation rate colleges. Curiously, the evaluators reported finding evidence of homophily effects with respect to race and ethnicity. Specifically, students of color submitted 15 percent more applications to and were more likely to enroll in (4.6 percentage points higher) a designated "College Point" college when they were randomly assigned to an adviser who was not White. A similar effect was observed with four year colleges as this group of students was 3.7 percentage points more likely to enroll in a four-year institution (Gurantz et al., 2020).

The premise in which many of these programs are grounded is that providing supportive individuals not only with knowledge on how to navigate the college application process, but with individuals who will support them at each step through the process will transmit (though arguably, develop is a more accurate term) social capital to the student, which will aid the

[10] This group of 290 colleges were known as "College Point" colleges, and all had graduation rates in excess of 70 percent.

student in successfully transitioning to postsecondary education. In recent years, researchers have begun turning attention from focusing on not only *if,* but also *where* students go to college, in an effort to identify interventions that maximize the students' chances of *persisting and completing college.* Specifically, research on "match and fit" examines two components of college enrollment that have been suggested to impact the students' likelihood of persisting and completing college.

The 'Match' in Match and Fit: Academic Undermatching

Research on academic undermatching, can be largely categorized as a subset of the greater literature on college choice, broadly. The literature classifies undermatching as a college enrollment outcome where students apply to and enroll in colleges that are less selective than what they are presumably prepared for and would be admissible to, based on their academic record. Interest in undermatching emerged as researchers argued enrolling in a less selective institution than a student was academically qualified to attend would likely have deleterious results for the student in both the short and long-term. Such risks, which have since borne out in empirical research include lower amounts of financial aid offered, longer time-to-degree completion (Bowen et al., 2009), increased risk of not graduating (Bowen et al., 2009; Ovink et al., 2018), and missed opportunity to receive larger economic returns from college particularly for low-income students (S. B. Dale & Krueger, 2002), as well as for Black and Hispanic students (S. Dale & Krueger, 2011), among others. Attention to the topic in the mid to late 2000s came at the same time other research was finding that after accounting for educational expectations, students who began college at community colleges were less likely to complete a four-year degree even within extended time frames (Alfonso, 2006; Long & Kurlaender, 2009). As researchers contend that loss of credits during the transfer process itself may serve as the primary cause of decreased four-year degree attainment for students (Melguizo & Dowd, 2009;

Monaghan & Attewell, 2015), increasing the chance a student enrolls in an institution that is an academic match at the outset of their college career may insulate them from risks associated with transfer credit loss, thus removing a potential barrier to persistence and attainment.

Though the study of undermatching preceded their work by a few years, consequential findings from Hoxby & Avery (2013) help situate the issue. Their study examined college application and enrollment behavior of all ACT and SAT taking students from the high school class of 2008. Surprisingly, their review of the data revealed that high achieving students from low-income backgrounds were far less likely to apply to any selective institutions where they would likely be admissible. For every low-income high achieving student applicant, there were roughly 8 to 15 applicants from high income backgrounds. The disparity was particularly striking given there were only twice as many high-income students (34 percent) in this pool compared to low-income students (17 percent). Additionally, they found that not only do many low-income students not apply to 'match' schools, but more than 40 percent of low-income high achievers applied *only* to schools with median SAT or ACT scores that were more than 80 points lower than the score they achieved on these exams. The contrast was further amplified when considering only roughly five percent of students from high income backgrounds exhibited this application behavior.

Those interested in investigating undermatching have been careful not to categorize students' college enrollment choices as "bad" or assign judgement, though undermatching itself is often described as a perplexing student outcome, commonly being referred to as a *phenomenon*. The language used to describe it is often shaped by two assumptions best described by Ovink et al. (2018): "that students would not choose to undermatch if they had better information (i.e. the information deficit" argument) and that undermatching leads to less

advantageous college and postgraduate outcomes (i.e. the "maximization argument)… [both] are typically elaborated within a rational choice framework, focusing on individual choices and decision-making" (p. 555).

Empirical Research on Academic Undermatching

The earliest studies sought to detail and investigate the existence of patterns and correlates of undermatching within specific regions of the nation finding undermatch rates ranging from 40 percent in North Carolina (Bowen et al., 2009) to 62 percent of Chicago public school graduates (Roderick et al., 2008). These initial investigations spurred new lines of inquiry as researchers became interested in identifying the extent of undermatching, nationally. Subsequent studies have generated perhaps as many questions as answers, with research estimates of undermatching rates nationally varying widely even when using the same dataset (many including the current study utilize the ELS:02 longitudinal dataset). For example, Roksa and Deutschlander (2018) found that 18 percent of students undermatched at the point of applying to college indicating that nearly 1 out of 5 students nationally did not apply to a single institution at an commensurate selectivity level. By contrast, Belasco and Trivette (2015) found that approximately 60 percent of all ELS students with access to a four-year institution had not applied to a single institution to which they were qualified to attend. When examining enrollment patterns, undermatching rates have ranged from 28 (Belasco & Trivette, 2015), to 39-41 percent of ELS students (Ovink et al., 2018; Smith et al., 2013) to as high as 56 percent (Rodriguez, 2015a).

Differences in obtained rates of undermatching are likely due to the variation in how it is constructed and operationalized as no single approach has yet been widely accepted as the 'appropriate' method. As such it is clear that undermatching rates are highly sensitive to how

researchers operationalize and categorize/organize selectivity[11] levels at the institutional level as well as in considering what constitutes appropriately matched qualifications at the student level. With respect to the latter, such considerations include what appropriate qualification metrics are, and the benchmarks or aptitude needed to sufficiently demonstrate fulfilling or holding said qualifications). Additionally, while some have approached the topic by creating their own measures of institutional selectivity, the majority of the research on undermatching relies on the use of the Barron's Selectivity Index. The index, produced by NCES categorizes institutions into selectivity groups, which are based on metrics including the institutional admission rate, class ranking thresholds and test scores of the incoming class of students.

There are primarily three approaches researchers have used in developing undermatching constructs, as summarized by Rodriguez (2015a): the 'enrollment rate' (Lee et al., 2017; Roderick et al., 2006, 2008, 2011), 'acceptance rate' (Belasco & Trivette, 2015; Bowen et al., 2009), and 'predicted probability' methods (Deutschlander, 2017; Lee et al., 2017; Ovink et al., 2018; Rodriguez, 2015b; Roksa & Deutschlander, 2018; Smith et al., 2013), with the latter arguably being the most popular approach. Under the enrollment rate approach, students are sorted into groups based upon combinations of GPA ranges and ACT/SAT scores, and are deemed as having 'matched' for the selectivity level where the greatest proportion of similarly qualified students attended. The acceptance rate method similarly creates a distribution of group combinations based on the same measures (GPA and ACT/SAT scores), though the approach deems students as having 'matched' when a designated threshold (often 80 – 90 percent, as determined by the researcher) of similarly qualified students received at least one acceptance to

[11] See *Tradeoffs and Limitations: Understanding the Estimation of College Undermatch* by Rodriguez (2015b) for a comprehensive overview and discussion on the implications of using varying selectivity schemas for undermatching research and results.

an institution at a given selectivity level. Under circumstances where a given group of students exceeds such a threshold at multiple selectivity levels, match status is assigned to the highest selectivity level. Finally, under the predicted probability method, researchers determine a students' presumed selectivity match level by predicting the probability of admission and enrollment in an institution (typically using observed data on submitted applications, acceptances, and enrollment) at each designated selectivity level. Such prediction models are often conditional on a student's grade point average, ACT/SAT exam scores, and participation in AP or IB courses among other characteristics (Ovink et al., 2018; Rodriguez, 2015b, 2015b; Roksa & Deutschlander, 2018; Smith et al., 2013). Once all predicted probabilities are obtained, students are assigned to the highest selectivity level for which their probability of being admitted exceeds a pre-determined threshold (researchers often use a 90 percent chance of admission).

An influential series of reports published by the Consortium on Chicago School Research at the University of Chicago (Roderick et al., 2006, 2008, 2009, 2011) provided a comprehensive overview of the prevalence of undermatching among Chicago Public School (CPS) graduates. Initially working with limited student data, the first report (Roderick et al., 2006) saw the authors develop a rubric to identify and classify match[12] categories and statuses (the aforementioned 'enrollment rate' method). Under this approach the authors found only 20 percent of CPS graduates had 'access to' selective (16 percent) or very selective colleges (4 percent). Moreover, nearly a third (31 percent) of graduates had access only to two-year colleges. The 2008 report found that despite nearly three-quarters (72 percent) of CPS graduates reporting an aspiration to earn a four-year degree, only 59 percent ever applied to a four-year college. Ultimately, the

[12] While the 2006 report (Roderick et al., 2006) did not formally adopt a term, in subsequent reports the authors refer to the (later termed) undermatching concept as "mismatch" (Roderick et al., 2008, 2009, 2011).

report found an overall undermatch rate of 62 percent – in other words, roughly one out of every three CPS graduates enrolled in a college that was an academic match. Similar to concerns highlighted by Hoxby & Avery (2013), an equivalent proportion of CPS graduates with access to the most selective institutions undermatched by enrolling in a less-selective college or not enrolling at all (38 percent). Their illustration of how college-going aspirations did not necessarily translate to enrolling in a four-year college, provided important contextual and descriptive support for the need to understand the experiences of students through the application process. Encouragingly, the authors observed two specific actions that students could take to increase their probability of being admitted to, and ultimately enroll in a four-year college: submitting more than three or six college applications and filing a FAFSA by May of the graduating year (50 percent increased probability of enrolling in a four-year even after controlling for demographic and academic characteristics). Notably, students who completed the FAFSA were also much more likely to enroll in a match institution. In addition to these two specific actions, authors noted the strongest factors appearing to correlate with enrollment in a match college was attendance at a school with a strong 'college-going culture' operationalized through four measures: (1) teacher survey responses assessing the school's college activities and climate[13], and the percentage of prior-year graduates who: (2) applied to three or more colleges, (3) completed a FAFSA, and (4) enrolled in a four year college directly after high school. In their final report Roderick et al. (2011) investigated the strength and influence of said college-going culture measures using the CPS data. Their examination employed two-level hierarchical

[13] The teacher assessment measure of college-going culture asked teachers the extent to which they agreed (from strongly agree to strongly disagree) that teachers in the school "(1) expect most students to go to college, (2) help students plan for college,… (3) the curriculum is focused on helping students get ready for college,… (4) teachers feel that it is a part of their job to prepare students for success in college,… (5) many of our students are planning to go to college" (Roderick et al., 2011, p. 183).

generalized linear models with student level demographic and academic, school level controls, and the college-going culture measure. Results indicated that students who applied and were accepted to a four-year institution and who attended schools with strong teachers assessments of college activities and planning were 12 to 17 percentage points more likely to enroll in a match or 'overmatch' college. Additionally, throughout their analysis, high FAFSA completion rates remained a consistently reliable predictor of students applying, being accepted to, and enrolling, in a four-year college overall, and at match or overmatched institutions specifically. Ultimately, the authors conclude by noting the importance of placing attention to creating school environments where students and families are able to access information, resources, and supports focused on guiding students through these processes. Such recommendations are in line with the conceptual and theoretical perspectives reviewed earlier suggesting providing additional sources of social and cultural capital to students while in high school (Bergerson, 2009; McDonough, 2005a; Perna, 2006; Tierney et al., 2005) - through interventions like access programs would expose students to college going activities and provide the consistent and robust information on college options to students and their families, ultimately addressing such an information deficit, if one exists.

While the regional portrait presented in the CPS reports (Roderick et al., 2006, 2008, 2009, 2011) are helpful in understanding and contextualizing undermatching, in the interest of capturing national rates, researchers have conducted a series of undermatching studies largely relying on the Education Longitudinal Study of 2002 (ELS) dataset which provides robust data capable of allowing extensive investigations of undermatching. Among the first to do so, Smith

et al. (2013)[14] assessed undermatching outcomes using the predicted probability method to construct undermatch measures, predicting access to each level of selectivity conditional on indicators for whether the student took the ACT, SAT, or participated in AP/IB, and their GPA, SAT, and ACT scores. Their analytic models relied on using linear probability models to examine how predictive socioeconomic and demographic factors were in contributing to academic undermatch outcomes. Using a sequential model building approach with variables added subsequent models, they found that across nearly all models, whether demographic and academic characteristics were controlled for or not, students in the lowest SES quartile were significantly more likely to undermatch relative to students in the highest quartile. Overall, they also observed the majority of low-SES students undermatched (55 percent) compared to 41 percent of all students.

In their own undermatching study using the ELS sample, Belasco and Trivette (2015) were critical of the findings of Smith et al. (2013), taking issue with the statistical models used both to create the undermatch constructs, as well as the analytic models. Their critique argued the former (predictive probit models) were inappropriate given they may have granted selectivity access to high levels in circumstances where the student had high SAT scores but low a GPA, and the latter (analytic models) controlled for too few variables resulting in models that were unable to adequately contextualize a students considerations and college-going preferences nor their surrounding environmental influences. To ameliorate their concerns, Belasco and Trivette, (2015) relied on the acceptance rate method to assign selectivity access and multilevel analytic models to account for students' high school context and examined how behavioral and

[14] Smith et al., 2013 also used the National Education Longitudinal Study of 1988, though I focus my review on the ELS student sample as it is much more recent. Of note, between the 1992 and 2004 high school cohorts, the authors note an eight point drop in undermatch rates of graduating seniors, from 48.9 percent to 40.9 percent of ELS students.

information-seeking activities influenced students chances of undermatching. They examined three binary outcomes examining if a student undermatched: overall, by selectivity, or by level. The latter outcome was conditioned on students with access to a four- or two-year enrolling in a two-year or not enrolling at all, respectively. Notably, their results suggested that African American and Hispanic students were less likely to undermatch relative to their White peers diverging from some prior results which found Black students had a much greater chance at undermatching by selectivity relative to their White peers (Bowen et al., 2009) or that Hispanic students were as likely to undermatch (Roderick et al., 2011). The finding regarding Black students being less likely to undermatch was however similarly observed by Smith et al. (2013) though the conclusions arrived at by the researchers diverged, and highlight potential implications in sensitivity of different undermatch constructs. Belasco and Trivette (2015) attributed the result to the additional controls included in their models while Smith et al. (2013) posited the finding may be a function of only a small proportion of Black students estimated as having access to selective colleges. Despite the contradictory results, they also found that students were less likely to undermatch by selectivity and level when they reported having visited a school counselor for college information and completed a FAFSA, respectively, corroborating results on the importance of both school staff as sources of social capital and of a schools college-going culture (Roderick et al., 2008, 2011). While not causal interpretations, these findings are conceptually important in that they suggest students who report seeking out college information (via school staff e.g., counselors etc.) may do so as they understand applying to institutions that are at least somewhat selective adds additional steps and complexity to both the materials required to be submitted for admission, as well as the application process overall (filing a FAFSA to access financial aid, for example). Through structured activities and guidance

designed to help students navigate such complexities through the college and financial aid application process, programs like TRIO and GEAR UP are well positioned to provide similar benefits to participating students. Moreover, given that students may participate in these programs over multiple years from middle school through high school, sustained exposure to these forms of social capital over time are theoretically well positioned to tighten the academic match for students when they apply and enroll in colleges.

Beyond considerations of school context alone, prior literature has recognized the importance of familial sources of social capital in the application process as students rely on relatives as primary sources of acquiring information about college (Bell et al., 2009). Given such influences, Roksa and Deutschlander (2018) extended the undermatching line of inquiry to investigate the extent to which familial resources and other sources of social capital help explain occurrences and rates of undermatching with specific focus on undermatching at the point of applying to college. Their focus on the point of application serves to underscore prior findings highlighting the importance of submitting three or more applications on having more favorable outcomes with respect to undermatching (Roderick et al., 2008), noting: "If they do not apply to those [selective/match] institutions, they will inevitably undermatch at the point of enrollment, which makes application a crucial step in the process… students from socioeconomically disadvantaged backgrounds are more likely to attend less selective or two-year institutions than their more socioeconomically advantaged peers largely because of where they apply in the first place" (Roksa & Deutschlander, 2018, p. 9). Their study used approaches similar to those employed by Smith et al., 2013 both in using the predicted probability method as well as in their analytic model building approach adding additional controls accounting for family and social resources characteristics. Their research highlights several important findings for how students

college preferences and family resources impact academic match outcomes. First, students who reported the cost of college, and of being able to live at home while attending, as *very important* undermatched at rates 32 percent and 1.7 times higher than their peers, respectively. Second, when accounting for other sources of social capital, visiting a school counselor was not related to undermatching at the application stage. In fact, counselor visits were only related (though not significantly) to undermatching in the application phase when no other information sources were included in the models, as counselor visits were found to be highly correlated with having access to more sources of information. This reinforces the notion that students who seek out counselors may already have access to or seek information from more sources than their less well-connected or resourced peers. This may also help explain why Belasco and Trivette (2015) found counselor visits reduced the rate of students enrolling in less selective institutions than they might otherwise be qualified or prepared for. Additionally, they found that each additional information source available to a student reduced their probability of undermatching by eight percent.

While the growth in undermatching research continues, there has been surprisingly little literature examining whether college access programs have an effect (in either direction) on rates of academic undermatching, for students served by such programs. This may, in part, be due to the great variation in college access programs, including in program models, services offered, and strategy. In the section which follows, I discuss the concept of match and fit, and review literature which ties together the role of access programs in facilitating match and fit outcomes of participants.

The Importance of 'Fit'

While researchers have focused on the importance of maximizing the academic match between students and the colleges and universities they choose, focus on factors beyond academic considerations including affordability or financial fit have also been investigated to determine the role they play in facilitating college enrollment outcomes. Such factors are particularly important to college advising professionals working in access programs as they understand there are many other considerations students make when determining where to apply and ultimately enroll in when looking at college options. Describing the importance of these considerations in advising services, Achieve Atlanta, an intermediary college access organization incorporates discussions of match and fit into their training models, noting:

> " "match" typically describes the degree to which a student's academic credentials match the selectivity of the college or university in which they enroll. Match encompasses the quantitative elements of choosing a postsecondary option; it is more science than art. "Fit" is a more nebulous concept that refers to how well a prospective student might mesh with an institution once on campus: socially, emotionally, financially, and otherwise" (Allen & Schulz, 2020, p.16). Components of fit may include a college or universities geographic location, the proximity to home for students, major offering, or considerations of financial aid availability or net cost, among other factors.

For example, researchers have recently investigated the extent to which proximity and location matter for mismatching outcomes, recognizing that the geographic dispersion of institutions results in physical limitations which redefine what is accessible (practically speaking, e.g., 46 percent of all elite institutions located in the Northeast) and that students from disadvantaged groups may possess fewer resources with which to pick up and move to locations distant from their homes and families (Ovink et al., 2018). Their study found that on average

students were five percent less likely to undermatch when they lived within 50 miles of a match institution. Notably, even when students indicated they wanted to live near home, they were less likely to undermatch when they lived within 50 miles of a match institution. Subgroup analyses were particularly striking as Black students were slightly less likely to undermatch relative to White students overall. Arriving at a similar conclusion to Smith et al. (2013) the authors suggest this finding may be a result of Black students on average being predicted to have access to schools of lower selectivity levels. Still, similarly to patterns noted by Hoxby & Avery (2013), the analysis also suggested Black students who were qualified to attend the most selective institutions were significantly more likely to undermatch compared to similar White students. Their results provide support for the importance of considerations of fit when students are applying to college, and in the role fit may play in undermatching outcomes.

Given such considerations, in addition to academic undermatch I am also interested in the extent to which these programs impact financial aid participants receive at the institution where they ultimately enroll. While much has been written about affordability in higher education, no consensus has been reached on ways to measure such a concept, given differences in preferences and priorities for students and families in determining value, to say nothing of the fact that higher education affordability could be evaluated from the perspective of institutions, states, and other entities (Baum & Ma, 2014; Fergus, 2019; Gándara & Ness, 2019; Sharpe & Webster, 2016; St. John et al., 2000). Focusing in part on the type of financial aid received is particularly important as a body of literature suggests students of color, and students who are from low-income backgrounds are particularly sensitive to and adverse of financial aid in the form of student loans with these forms of aid impacting undergraduate enrollment decision-making and behavior (Burdman, 2005; Heller, 2008; McDonough & Calderone, 2006; Paulsen & St. John, 2002;

Perna, 2000; St. John, 1990; St. John et al., 2000). For students navigating the college admissions process, part of the process of identifying colleges to apply to and the choice of where to enroll often requires a review of financial aid packages offered and available for their colleges of interest, with packages often comprising a mix of grant aid (e.g., Pell and potentially other state and local need or merit-based aid), and loan offers.

The nuance and complexity involved in these decisions may have costly implications for students down the line – for example, as Pell grants are often insufficient in covering costs of tuition and fees, students may need to decide between taking out significant amounts in student loans, or reducing their attendance intensity to part-time status in order to work part or full-time to be able to meet basic needs. Access program target populations such as low-income students in particular face these decisions more frequently. Prior research has found 74 percent of all full-time undergraduates report working while enrolled, with 20 percent of these students working full-time. This is compared to 62 percent of low-income students who report working full-time in order to pay tuition, fees, and living expenses (King & Bannon, 2002). In a study of the impacts of willingness to acquire debt in order to afford and persist through an undergraduate education, Mendoza (2012) derived a debt/EFC (Pell Estimated Family Contribution) measure and found students with a debt/EFC ratio greater than the median 1.5 times more likely to work less than 30 hours per week and were 1.6 times more likely to graduate from college within six years when compared to students below the median. The implications here are complicated in that they suggest students willingness to take on debt may help persistence efforts through allowing students to work fewer hours which then allow more time dedicated to academic work, which then increase their chances of graduating with a four-year degree within six years. These findings suggest that access programs may be able to best position students to succeed not by encouraging

students to avoid loans entirely, but through encouraging thoughtful consideration as to how taking on debt may assist students in reasonably achieving their academic goals while avoiding being overworked or attending college at a reduced intensity.

The Role of College Access Programs in Facilitating Match and Fit

The most relevant and closely aligned research at the intersection of literature on pre-college access programs, academic undermatching, and considerations of fit arguably through strategies designed to transmit social capital was an RCT evaluation of Upward Bound projects. The evaluation focused on assessing whether implementation of a new package of college advising strategies randomly assigned to UB projects can impact match and fit outcomes for UB participants (Linkow et al., 2021; Martinez et al., 2018). U.S. ED released the first report in 2018 and described the initiative as a packaged support which included several advising practices and strategies, written material, planning tools and resources for use during the college application process, and webinars sharing promising advisement practices for program staff. The evaluation was designed to determine whether the package of advising strategies and services increased the number of college applications students submit, impacted student considerations when applying, increased FAFSA filing rates, and principally closed undermatching gaps for UB participants who were high school seniors during the 2015-16 academic year. Of 702 eligible[15] UB projects, 194 volunteered for participation, and among the 194 projects 98 were randomly assigned to receive the *Find the Fit* treatment. Though random assignment was not used at the project site level, the resulting sample of project sites were similar in characteristics and demographic enrollment when compared to all UB projects nationally. The student sample size for the study included 2,336 students who were in programs that received the treatment, while 2,107 students

[15] Programs were eligible to participate if the project had received federal funding from U.S. ED during the 2012 fiscal year.

were enrolled in control project sites were not. Similar to the UB model in general, while treated project sites were provided training on and an overview of the Find the Fit initiative, projects still ran independently and were able to implement (or not) the practices as they wanted. As with prior research, the evaluators relied on the Barron's Selectivity Index (though they do not collapse categories together) to assign undermatching status. With respect to financial outcomes, though the initiative did not appear to impact filing a FAFSA by the March 15 date[16] for the overall treatment group, the intervention did appear to significantly increase such rates of filing for Black students (64.1 percent compared to 54.2 in the control group) and students who scored in the lowest exam quartile on a college entrance exam (60.0 percent compared to 52.2 percent in the control group). Such rates of filing may provide additional sources of financial aid these participants might otherwise not have had access to, and potentially lower the amount of loan aid participants need, to a level they find more acceptable and create a perceived better financial fit.

Preliminary results also suggested students at treated project sites were roughly nine percent more likely to report applying to at least four postsecondary institutions, supporting results from prior research that suggested programs induce participants to submit more college applications than they otherwise would have (Glennie et al., 2015). Importantly, this increase in applications was accompanied by treatment group students applying to significantly more selective institutions than their peers. When breaking out results by subgroups, female students experienced larger gains in terms of the proportion applying to higher selectivity levels, particularly at greater levels of selectivity. Moreover, increases were generally enjoyed across selectivity levels for Hispanic students, and Black students. Finally, students at the highest end of the college entrance exam distribution (those in the top quartile) experienced the largest gains as

[16] The March 15th deadline is commonly used as a priority deadline by institutions and states for qualifying for aid beyond the federal Pell grant, including institutional and state aid.

nearly 19 percent more treated students (relative to the control group) applied to institutions of the greatest selectivity. While increasing applications[17] to more selective institutions could *potentially* reduce academic undermatching outcomes, the initial report did not was released prior to those outcomes being observed.

The follow-up report was released in November of 2021[18], with evaluators reporting the *Find the Fit* package of strategies and practices did *not* reduce academic undermatch outcomes for treated students. While slightly fewer students in the treatment group undermatched (39 percent) compared to students in the control group (41 percent), these differences were not significantly different. Similarly, students who received the treatment were no more likely to persist (52 percent) than control group students (49 percent). There was evidence, however, to suggest the initiative increased student attendance near the top rungs of selectivity. Specifically, Female students as well as Black, non-Hispanic students in the treatment group were roughly 4 percentage points more likely to enroll in a school that was at least "highly competitive" compared to control group students. While these results are suggestive of potential findings for the current study, the inclusion of programs beyond only Upward Bound in this analysis mean results may differ in part due to programs considered.

[17] It is important to note that a greater proportion of a group applying to greater numbers of more selective institutions does not necessarily equate to these students having applied to institutions constituting a 'match'.
[18] The results of the *Find the Fit* evaluation were released after the initial completion of this literature review, while analyses for the dissertation was underway. To ensure the literature review captured this especially relevant report, the findings were added to augment the review of the initial report.

The results from the *Find the Fit* study may be explained by a finding from the first report (Martinez et al., 2018) - when asked whether academic quality was very important to their college choice, evaluators found no notable difference in responses between students in the treatment and control groups. The concern here is that while the treatment of providing additional (structured) information for college advising professionals to use with advisees may impact concrete behaviors (e.g., submitting more applications, applying to more selective institutions, filing a FAFSA by a certain date, etc.), the information provided to students may not change their perceptions or personal assessment of why academic quality should matter when they choose a college. If proponents of undermatching contend students at an informational disadvantage would not choose to undermatch if they had access to additional information - conceivably information which would help students distinguish and consider academic factors and differences between institutions – then providing students with social capital in the form of information and individuals dedicated to helping them make an informed decision should *theoretically* address such concerns. If this is not the case, and providing additional information, resources, and support in the form of college advising professionals is more effective in facilitating concrete behavioral change rather than impacting decision making or general considerations, such interventions may not be effective on their own in addressing concerns of match and fit, or in reducing 'undermatching'.

Importantly, the evaluation highlights one of the principal concerns of the current study: do these programs impact *where* students enroll? And beyond that (if they do), do some students benefit more than others? In the two chapters which follow, I move to answer these questions by presenting results from the two analyses conducted in this study. Chapter three presents the first analysis, relying on ELS data to employ a propensity score-weighting strategy in order to

investigate whether participation in any of the three federal pre-college access programs (Talent Search, Upward Bound, or GEAR UP) impacts academic undermatching outcomes of participants as they apply to and enroll in college, in addition to their financial fit at their institution of choice. Chapter four examines the effects of three near-peer programs on school-level college-going outcomes in the state of Michigan between 2007-2021 and disaggregates said outcomes by race and ethnicity to examine whether effects are enjoyed across populations or benefit specific student subgroups.

CHAPTER 3:

EXPLORING ACADEMIC MATCH AND FINANCIAL FIT OUTCOMES FOR FEDERAL COLLEGE ACCESS PROGRAM PARTICIPANTS

Few studies have examined the impact of targeted intervention pre-college access programs on academic undermatching outcomes of participants, particularly of the federal access programs. Unlike other models of pre-college access programs which work with primarily senior students, the federal programs typically work with students over an extended period of time. Given prolonged exposure to college advising personnel program staff who are meant to structure the delivery of college knowledge and provide practical preparation and guidance through the application process, participation in these programs should help students build knowledge helping them to distinguish differences between colleges and universities as they apply to college. The social capital transmitted arguably best situates these programs to influence undermatching outcomes of participants as they often target and work with students over an extended period, deliberately helping over time to build lists of colleges to research, sort through, and ultimately apply and enroll in. The first analysis in this dissertation relies on employing an inverse propensity score weighting strategy to examine whether (and if so, how) participation in a federal college access program – specifically, Talent Search, Upward Bound, and/or Gear Up improve postsecondary match and fit outcomes, by answering the following research questions:

Research Questions

1. Is participation in a federally funded college access program associated with a student academically matching at both the point of applying to and enrolling in college?
2. Is participation in a federally funded college access program associated with improved financial fit at the point of enrolling in college?

Methodology

The subsections that follow outline my analytic strategy in conducting the first analysis. I first discuss the data used in this analysis and then detail the steps taken in handling missing data, estimating and constructing undermatching variables, creating an appropriate comparison group to compare to CAP students, and finally the analytic models used.

Data

To answer the research questions posed above, I use data from the Education Longitudinal Study of 2002 (ELS:2002), which followed a nationally representative cohort of tenth graders in 2002, collecting information on their experiences in high school and throughout their postsecondary transition into work or college. The base year surveyed students, their parents and/or guardians, and teachers and administrators from schools where they were enrolled. The survey followed up with these students in 2004 when most were twelfth graders, and again in 2006 when most students in the cohort had been out of high school for two years. In total, the survey contains data for approximately 16,200 students.

College Access Program Data

The primary variable of interest identifies students who participated in one of the three federal college access programs (hereafter CAP): Educational Talent Search, Upward Bound, and Gear Up. The program participation data come from the first follow-up survey in 2004, when most students were in their twelfth-grade year (variable *F1S23*). The question asked students: "Talent Search, Upward Bound, and Gear Up are programs that help economically disadvantaged high school students to prepare for entering and succeeding in college. At any time during high school, have you participated in these programs or a similar program?" (Ingels et al., 2006, p. 122). Importantly, this question was shown to a specific subset of the ELS sample who received the full student questionnaire[19]: students who participated in the base year survey, and who were still enrolled at the same high school (had not transferred, graduated early, left high school, etc.). Due to this limitation, roughly more than one-third of students surveyed did not receive the question and (as well as an additional approximately 120 students who received but did not answer the question). I use this data to derive a binary *CAP* participant variable, with participants coded as 1, and non-participants as 0. Though not without its limitations, grouping students in this way is largely due to the similarity in program goals, an approach also employed by Domina (2009) and Glennie et al. (2015). While each of the programs have some unique features, the core activities and purposes function similarly to each other. Each is designed to promote the importance of attaining a college education for participants and provide practical assistance and guidance to students as they navigate through the college application process.

[19] Readers should note, particularly given the current study's interest in minoritized and historically excluded populations, the full student questionnaire was produced only in English (Ingels et al., 2007).

Missing Data and Multiple Imputation Procedures

The first step in my analytic strategy concerned dealing with missing data. Missing data in the context of the current study present significant issues if left unaddressed. The first is that students with missing data on any measure used to predict selectivity access and/or undermatch outcomes will be omitted from such predictive models, likely introducing bias to the undermatch constructs themselves. The second, and as concerning, relates to the small number of students who indicated participating in a CAP – missing data for students in this group would result in a much smaller sample size, likely less representative of participants. Literature regarding both the number and types of imputations one should use to obtain valid estimates ranges widely, even within prior undermatching studies using this same dataset[20]. Discussing the use of multiple imputation methods in higher education research, Manly & Wells (2015) while not identifying a specific number of imputations, emphasize that the number of imputations used should exceed at least five imputations which was previously considered sufficient. They rely on recommendations of other researchers to consider between twenty and one-hundred imputations (Graham et al., 2007). Finally, they suggest complex survey components like survey weights, and stratum and primary sampling unit identifiers should be included in both the imputation and analytic models when utilizing datasets like ELS (Manly & Wells, 2015).

To address missing data, I imputed twenty[21] datasets using multiple imputation with chained equations (MICE). As Wulff and Ejlskov (2017) succinctly summarize the procedure, "MICE changes the imputation problem to a series of estimations where each variable takes its

[20] While some studies drop cases with missing data (Smith et al., 2013), others rely on imputation strategies. Some are unclear as to the number of imputations conducted (Belasco & Trivette, 2015), others impute five (Callahan & Humphries, 2016; Roksa & Deutschlander, 2018) to ten datasets (Rodriguez, 2015).

[21] Twenty was chosen as the number of imputations in order to keep within the recommendations of Manly & Wells (2015) and also keep the computing time required to impute the data manageable.

turn in being regressed on the other variables. MICE loops through the variables predicting each variable dependent on the others" (p. 42). The MICE procedure allows simultaneous imputation of data and is capable of specifying different regression models depending upon the variable type (binary, continuous, truncated continuous, etc.) and its distribution in order to accommodate differences between variable. The flexibility provided by this approach avoids relying upon a single statistical model which may be appropriate for some variables though inappropriate for others (Beaumont & Demirtaş, 2013; Mitra & Reiter, 2011; Wulff & Ejlskov, 2017).

Three rounds of imputations were conducted:

> In the first, schools with missing data on the percent of students receiving free or reduced-price lunch were separately imputed using the ELS school level file, and later merged with the student level data in order to ensure students attending the same school were assigned the same values within each imputation, as recommended in prior literature concerning imputation of group level data (Gelman & Hill, 2007; van Buuren, 2014). This data was later merged with the individual student level data.
>
> The second-round imputations, imputed background data used to estimate measures of academic undermatch, including missing data on high school GPA, number of AP credits earned, math course taking pipeline, and student participation in extracurricular activities. This round included students who: graduated on-time or early, participated in the first three survey waves, and who were traditionally college-bound (not attending a Special Education or Vocational school, and not enrolled in special education). The sample size for this round of imputations was approximately, 10,730 students.
>
> The third and final round of imputations imputed background data on measures used to estimate the propensity to participate in a CAP, including data on students 9th grade GPA,

whether the students indicated in the base year that their favorite teacher, or school counselor wanted the student to go to college, time spent on homework, whether the student had worked during the base year, and several scales measuring resources available to the student. This round of imputations is limited to students who graduated on-time or early, participated in the first three survey waves, were enrolled in a public high school in 2002 and 2004, and answered "yes" or "no" to the CAP question. All variables included in the analytic models discussed later (including the dependent variables) were included as predictors in the imputation equations. The sample size during this round of imputations was approximately 5,940 students. **Table 2** shows differences for imputed variables pre and post imputation, as well as the imputation models specified for each variable.

Estimating Academic Undermatch

Next, I utilized the data available (including the imputed data) to estimate and construct measures of academic undermatch. This is possible as the second ELS follow-up survey in 2006 asked students to provide data on their postsecondary transitions, and for students who reported having ever applied to college, collected information on each college a student applied to, and whether they were accepted and ultimately enrolled. I use this data to create dependent variables identifying whether a student undermatched when applying to and enrolling in college. Given variation in how researchers derive undermatch measures, the next section details the steps I followed in creating the undermatch dependent variables.

Table 2

Analytic Sample Comparison Pre and Post Data Imputations

	Means		%	MICE Regression
	Original	Imputed	Imputed	Model Specified
Worked for Pay In 10th Grade	35.50%	36.50%	0.10%	Logit
Student participated in:				
- Interscholastic sports	42.2%	40.5%	0.50%	Logit
- School band/chorus	22.9%	22.6%	0.50%	Logit
- Intramural sports	16.8%	16.5%	0.50%	Logit
- School play/musical	14.4%	14.0%	0.60%	Logit
- Vocational clubs	18.4%	18.0%	0.70%	Logit
- Academic clubs	25.3%	24.6%	0.80%	Logit
- Yearbook	17.3%	16.5%	0.90%	Logit
- Service clubs	21.0%	19.3%	0.90%	Logit
- Honor society	29.5%	26.6%	1.00%	Logit
- Student government	15.3%	14.4%	1.10%	Logit
- Hobby clubs	11.3%	11.4%	1.30%	Logit
Honors weighted GPA	3.0	2.9	1.50%	Truncated; ll(0) ul(5)
Number of AP credits earned	1.1	1.0	1.50%	Truncated; ll(0)
Math course taking pipeline	5.9	5.8	1.50%	Ordered Logit (1-8)
Homework hours per week	10.7	10.7	2.70%	Truncated; ll(0) ul(30)
School trouble scale	1.4	1.4	3.30%	Truncated; ll(1) ul(4)
9th Grade GPA	2.9	2.9	3.40%	Truncated; ll(0) ul(4)
Class resources scale	1.7	1.8	3.90%	Truncated; ll(0) ul(4)
Literary resources scale	4.0	4.0	9.10%	Truncated; ll(1) ul(5)
School % free/reduced lunch	24.4%	25.4%	9.60%	Truncated; ll(0) ul(100)
Parental communication scale	0.1	0.1	11.50%	OLS
Wants student to go to college:				
- School counselor	66.7%	65.9%	15.10%	Logit
- Favorite teacher	68.8%	67.7%	15.80%	Logit

NOTE: Data is weighted using the F2F1WT complex survey weight. Data imputed using multiple imputation with chained equations. ll = lower limit, ul = upper limit. Scale scores were derived for observations with observed data for each variable included in creating the scale. For scales with missing data, MICE was used to impute scores rather than to impute missing data on the individual variables used to derive them, to reduce the number of variable imputations required. This is a practice other researchers recommend when Cronbach alpha coefficients are sufficiently high (Graham, 2009; Wulff & Ejlskov, 2017). Pre-imputation alpha coefficients are: a = .8127 (Class Resource scale); a = .6908 (School Trouble Scale); standardized a = .8261 (Parent comm. Scale), comparable to those obtained by Domina (2009).

SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), “Base Year, 2002”, “First Follow-up, 2004”.

1. As student application and admission data in ELS contains each institutions IPEDS Unit ID for each institution a student applied to, I merged the application and admission data with the 2004 Barron's Selectivity Index, which categorizes institutional selectivity based on a combination of admission rates, the percentage of incoming first-year students who have standardized test scores exceeding the institutions minimum required ACT and/or SAT scores, and incoming first-year students class rankings (Belasco & Trivette, 2015). Adopting the approach of prior studies, I recategorized the Barron's categories to account for sparseness in certain categories, and in order to group similar institutions.

   Table 3 presents the recategorized Barron's Selectivity Categories used in this study. Consistent with the approaches of prior research (Belasco & Trivette, 2015; Ovink et al., 2018; Rodriguez, 2015a; Roksa & Deutschlander, 2018; Smith et al., 2013) the two highest selectivity categories in Barron's (*Most Competitive* and *Highly Competitive*) were reclassified as "Most Selective", the *Very Competitive* and *Competitive* classifications were kept in the same groupings (renamed *Selective* and *Less-Selective*), and Barron's *Less Competitive* and Non-*Competitive* were collapsed into a *Non-Selective* category. As Barron's index does not categorize all four-year institutions, those not included in the index were added to the non-selective category, as these institutions display similar characteristics to those originally classified as non-competitive[22]. Finally, two-year colleges were added to a newly created "Two-Year Colleges" category.

[22] For reference, **Appendix A** provides summary statistics comparing four-year institutions classified as "Non-Competitive" in Barron's to those four-year institutions that are not categorized. Comparatively, the Barron's categorized group have, on average, slightly higher admission rates than those excluded from Barron's, in addition to slightly lower graduation rates. Uncategorized institutions were also slightly less likely to require applicants to: submit secondary school grade point averages (17.3% to 32.7%); transcripts (29.7% to 39.3%), or test scores (14.6% to 40.2%), though more likely to require recommendations (12.4% compared to only 9.3 percent of categorized institutions).

Table 3
Reclassification of Barron's Selectivity Index

Barron's Classification	Current Study Reclassification	Notes on Additions and Exclusions
Most Competitive Highly Competitive	Most Selective	
Very Competitive	Selective	
Competitive	Less Selective	
Less Competitive Non-Competitive	Non-Selective	Four-year institutions not categorized by Barron's were added to the "Non-Selective" reclassification.
	Two-Year Colleges	As two-year colleges are not rated by Barron's, I grouped these institutions into a separate "Two-Year Colleges" category. [1]
Special	Excluded	Special focus institutions are difficult to compare to institutions with selectivity classified, so they are excluded from the analysis consistent with approaches of other researchers. [1]

[1] This categorization is consistent with prior undermatching research approaches, conducted by (Belasco & Trivette, 2015; Ovink et al., 2018; Rodriguez, 2015a; Roksa & Deutschlander, 2018; Smith et al., 2013). Similarly, these researchers also excluded the "Special" categorized Barron's institutions as well.

2. Following selectivity reclassifications, data from student applications were then aggregated such that each student observation identified the highest selectivity level to which a student applied and was admitted. Organizing data this way resulted in a single student observation for every student, whether they submitted multiple applications, one, or none.
3. I then created a binary indicator for each level of selectivity, identifying if a student was admitted to at least one institution at that level. Thus, a student who applied to four of the most selective institutions was coded with a 1 if they were admitted to *at least* one.

4. These indicators were then used to run a series of probit models to predict the probability that a student who applied to an institution at a given level would be admitted, based on their honors weighted high school grade point average, the number of advanced placement credits earned, the highest math course taken, participation in extracurricular activities[23] and scores on a set of NCES administered exams during both the tenth and twelfth grades. The NCES scores are used in lieu of and as a proxy for SAT/ACT scores for primarily two reasons: there were substantially fewer missing data points for these scores when compared to SAT and ACT scores, and the scores are highly correlated[24] to the two latter scores, anyway. Probit model coefficients were used to predict each student's likelihood of acceptance to institutions at each selectivity level.
5. I then assigned students as having *access* to a selectivity level if they were predicted as having an (at least) 80 percent probability of being admitted to an institution at each level, for. Though this is a more relaxed threshold than the 90 percent employed by others, (Roksa & Deutschlander, 2018; Smith et al., 2013) it still represents a student with good odds of being admitted to an institution. Additionally, the inclusion of a number of covariates above and beyond pure academic measures (e.g., extracurricular activities) likely raises the precision of these estimated probabilities, a point raised by Rodriguez (2015a) who also relied on this threshold.
6. Undermatching variables were then derived based on whether a student applied to and or enrolled in an institution at the highest selectivity level to which they were deemed as

[23] Extracurricular activities include intramural sports, interscholastic sports, school band/chorus, school play/musical, student government, academic honor society, yearbook/newspaper, service clubs, academic clubs, hobby clubs, or vocational clubs

[24] Pairwise correlation coefficients for the analytic sample show SAT scores are highly correlated with the standardized exam scores: (.8168 for the NCES 10th grade composite score; .7807 for the 10th grade math score; and .8389 for the 12th grade math score, all significant at $p < .001$ level). A similar approach in using NCES rather than ACT/SAT scores was taken by Ovink et al. (2018).

having *access*[25]. Students determined to have undermatched during the application phase, were omitted from the coding scheme/analysis of enrollment undermatch, as they would have no possibility to enroll in a match institution given they did not submit a single application to one. As the selectivity recategorization ranks two-year colleges at the lowest end, students determined to have access to these institutions, were classified as "undermatching" if they did not apply to any institution or did not enroll in a two-year institution or higher. Though some prior literature have included an 'overmatched' outcome for students who enroll in institutions with higher selectivity levels than they are predicted to have access to, I opted not to adopt this approach as students who have been admitted to such an institution have been determined to have 'access' to at least one institution of the given selectivity level by virtue of their admission (arguably, these students would instead undermatch if they did not enroll).

Estimating Financial Fit

Definitions of 'fit' and affordability (particularly for prospective students applying to college) and ways to derive such measures vary widely, and there is no consensus on how to operationalize these measures. Such measures are difficult to identify as affordability is ultimately decided by each individual's assessment of the value and cost (real or ascribed) of a given item/experience/investment. To construct the financial fit measure, I merged IPEDS financial data from the 2004 year for the first institution where students enrolled and constructed a net price measure using published tuition and fees, estimated costs of room and board, and

[25] For example, consider a student who applies to three schools (1 most selective, 1 selective, and 1 less-selective) and based on probit analyses controlling for demographics and academic characteristics has predicted admissibility scores of 75%, 82%, and 88% respectively. The student is then coded 1 in the selective category and 0 in the most selective and less selective categories (the former because the students probability of admission did not exceed 80 percent, and the latter because the student exceeded an 80 percent probability of admissibility to an institution of greater selectivity).

average cost of books – each measure utilized the appropriate variables identifying in versus out-of-state enrollment. Next, I calculated the total amount of Pell grant funding students received for the first year, as well as the total amount of loans. Finally, the financial fit measure was derived as a binary indicator set equal to 1 if the total amount of Pell aid exceeded the net price, or if the loan amount borrowed was less than or equal to 20 percent of the total net price (after factoring in grant aid). If loan aid exceeded more than 20 percent, the financial fit indicator was set equal to 0. This rather crude approximation is unable to factor in additional institutional based or state provided scholarship grant aid, as well as other forms of loans available to students given data on these measures was not collected, though it may help identify whether access programs are able to assist participating students in lowering their costs and perceived loan burdens. This construct though measuring a different construct is similar to the approach taken by Mendoza (2012) who operationalized willingness to take on debt in college by deriving a debt/EFC measure.

Analytic Sample

After estimating and deriving measures of match and fit, I removed from the sample all students who did not receive question *F1S23* (college access program question). Thereafter, the sample is reduced to students who responded to the first three survey waves, were not enrolled in special education, graduated from high school on-time, (similar to Rodriguez, 2015a, 2015b; Roksa & Deutschlander, 2018), and who were enrolled in public schools in 2002 and 2004, similar to Domina (2009) who also studied the same college access programs. Given that the programs of interest focus specifically on preparing disadvantaged student populations to enroll and succeed in postsecondary education, examining outcomes of participants compared to all non-participants would mask any potential program effects, as students self-select into program

participation (Murnane & Willett, 2011), and not all students would necessarily qualify for program participation. In order to ensure statistical models are able to capture program effects (if any), I examined covariate balance by comparing means of variables that should explain program participation, and determined participants and non-participants were significantly dissimilar on several measures.

The first four columns of **Table 4** show significant differences in baseline characteristics between these groups. CAP students are significantly more likely to be female, Black or African-American, potential first-generation college students, low-income, and not be native-English speakers, among other differences. To address differences in these characteristics I rely on propensity score methods to compare the group of CAP students to a group of students who did not participate in a CAP, but who are most similar to their peers who did.

Inverse Probability of Treatment Weighting Strategy

Researchers using observational data commonly rely upon propensity score methods in order to create such comparable treatment and control groups. Propensity scores themselves are simply a predicted probability of a person/group/unit of interest receiving a or participating in a treatment, based on their observable characteristics (Guo & Fraser, 2015; Rosenbaum & Rubin, 1983). Having previously observed significant differences in the CAP and non-CAP student groups, I used a logistic regression to estimate the probability of student participation in a CAP as a function of their observed characteristics.

While ideally all covariates included in the predictive model would be captured during and representative of only the pre-treatment period, data limitations preclude this as the base year survey occurred during the tenth-grade year. Despite this limitation, as ELS provides extensive student data at each survey wave, I rely on data drawn primarily from the base year to identify

Table 4

Pre- and Post-Matching Balance Check, Across Weighted Samples

	Complex Survey Weighted			IPT Weighted			Composite Weighted		
	Non	CAP	Diff	Non	CAP	Diff	Non	CAP	Diff
Female	0.53	0.64	0.11***	0.55	0.52	- 0.03	0.54	0.50	- 0.04
Native English Speaker	0.89	0.78	- 0.11***	0.83	0.83	0.00	0.88	0.87	- 0.01
Asian	0.04	0.07	0.03*	0.12	0.10	- 0.02	0.04	0.05	0.01
Black	0.10	0.31	0.21***	0.11	0.12	0.01	0.11	0.12	0.01
Hispanic	0.13	0.17	0.04	0.12	0.14	0.02	0.13	0.14	0.01
Other	0.05	0.10	0.06*	0.05	0.05	- 0.01	0.05	0.05	- 0.00
White	0.69	0.35	- 0.34***	0.60	0.59	- 0.00	0.67	0.65	- 0.02
1st Gen College Student	0.10	0.24	0.14***	0.12	0.14	0.02	0.11	0.13	0.02
Low Income	0.59	0.72	0.14***	0.60	0.63	0.03	0.59	0.62	0.03
1st Gen and Low Income	0.12	0.28	0.16***	0.14	0.16	0.01	0.13	0.14	0.01
Student Expects BA+	0.80	0.78	- 0.01	0.80	0.77	- 0.03	0.80	0.75	- 0.05
Parent Wants BA+ for Student	0.90	0.88	- 0.02	0.90	0.88	- 0.02	0.90	0.89	- 0.01
College-Track	0.56	0.53	- 0.03	0.57	0.51	- 0.06	0.56	0.50	- 0.06
9th Grade GPA	2.86	2.79	- 0.07	2.87	2.81	- 0.06	2.86	2.79	- 0.07
Homework Hours Per Week	10.70	11.27	0.57	10.82	11.10	0.28	10.72	11.05	0.33
10th Grader Worked for Pay	0.37	0.31	- 0.06	0.35	0.38	0.03	0.37	0.38	0.01
School Trouble Scale	1.42	1.46	0.05	1.41	1.44	0.03	1.42	1.44	0.02
Class Material Resource Scale	1.79	1.86	0.07	1.79	1.84	0.04	1.79	1.82	0.03
Parent Communication Scale	0.06	0.06	0.01	0.03	-0.00	- 0.04	0.06	0.01	- 0.04
Literary Resource Scale	4.00	3.46	- 0.53***	3.91	3.77	- 0.14	3.97	3.81	- 0.16
10th Grade Class Size	370	346	- 24	350	340	- 10	369	347	-22
% Free or Reduced Lunch	24.7	38.9	14.2***	26.5	27.4	0.83	25.30	27.00	1.7
% LEP or Non-English Proficient	4.15	6.40	2.25	4.26	4.08	- 0.18	4.26	3.81	- 0.44
% Non-White	30.3	49.3	19.0***	32.82	31.23	- 1.58	31.17	30.28	- 0.89
School Locale: Urban	0.22	0.34	0.12**	0.23	0.24	0.01	0.23	0.22	- 0.02
School Locale: Suburban	0.54	0.49	- 0.05	0.53	0.51	- 0.02	0.54	0.55	0.01
N (Unweighted)	5,620	330		5,620	330		5,620	330	
N (Weighted)	1,602,559	86,029		5,936	5,878		1,688,324	1,648,669	

NOTE: Differences in participant statuses are statistically significant at: * $p < .05$,** $p < .01$, *** $p < .001$. Complex survey weighted represent data weighted by the ELS 2002 1st - 2nd follow up panel weight. Not shown are the complex survey components: the F2F1WT, the stratum identifier and the primary sampling unit identifier. The survey weight is significant at the $p < .001$ level in the F2F1WT model, and at the $p < .05$ level in the composite weighted model, though not significantly different in the IPTW sample.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), "Base Year, 2002", "First Follow-up, 2004."

covariates I believe to be most pertinent in helping explain student participation in access programs. In addition to commonly used demographic measures (race, sex, English speaking status, and educational expectations and aspirations of students and their parents), following Domina (2009) and Glennie et al. (2015) I also derive several composite scale variables representing measures of social capital, including student-parent communication, literary resources in the home, students class material resources, and a school trouble scale. Additional variables are also included and identify students' grade point averages in the preceding ninth grade year, the number of hours spent on homework per week, and whether they worked a job for pay, and whether they were on a college academic track. Given that programs operate independently at each site, admission requirements at each program site may vary with some programs actively seeking and recruiting participants, while others require students, parents' and school staff to complete applications and recommendations for admission to participate. As programs may require school recommendations and input from school staff, I include two responses from the student base year survey which identify whether the students school counselor and their favorite teacher indicated wanting them to go to college. These serve as proxy measures for identifying school staff who may serve as an additional source of social capital helping to connect students with a CAP by either proactively reaching out to CAP staff to recommend the student for participation in the program, or who, alternatively may complete program application recommendations for students who are required to fill out an application to participate. Finally, I include three primary eligibility requirement predictors: A first-generation indicator, a low-income indicator[26], and an interaction between both. These variables are

[26] I derived this measure using household size, base-year income, and the 2001 poverty levels published by the Department of Health and Human Services (2001). Variables used to construct the low-income indicator are: byincome, byfcomp, bysibhom, byp07i, byp07j, byp07k, byp07l.

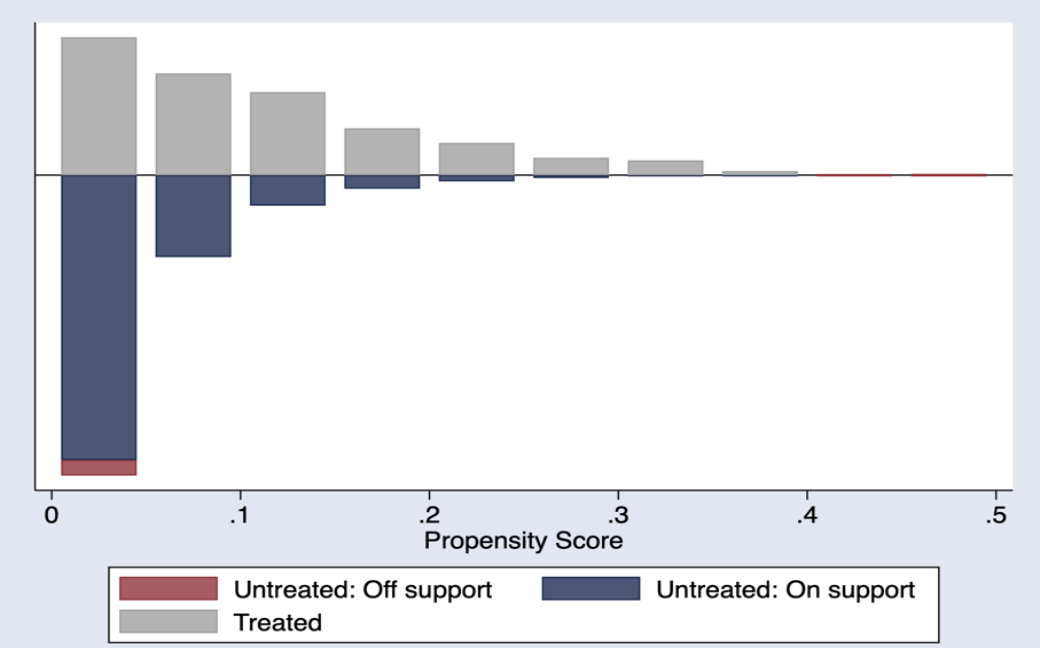
included given program requirements of both Talent Search and Upward Bound[27]. Taken together the variables in the model cover a broad range of academic, familial, school resource, and demographic characteristics which should best explain student participation in the programs.

As estimation of propensity scores attempt to address imbalance in baseline characteristics between participants in the sample, the propensity score model is itself not survey weighted, following recommendations of other researchers (DuGoff et al., 2014; Reynolds & DesJardins, 2009; Zanutto, 2006). While the model is not weighted, the complex survey weight, stratum identifier, and primary sampling unit identifiers are included, in keeping with recommendations of DuGoff et al. (2014), who argue the inclusion of these covariates "may [help] capture relevant factors, such as where individuals live, their demographic characteristics, and perhaps variables relating to their probability of responding to the survey…[i]including the weight may thus help satisfy the assumption of unconfounded treatment assignment" (p. 289). **Figure 2** shows sufficient common support shared between both CAP and non-CAP students, though notably, using the array of covariates previously noted results in identifying CAP students with an at most nearly 50 percent propensity to participate in one of the programs. The mean propensity score for CAP students is 11.0 percent, compared to 5.2 percent for nonparticipants, similar to prior research (Domina, 2009)[28] only a small portion of non-CAP students were not matched.

[27] Talent Search requires that two thirds of participants in a given program be first-generation (both parents do not have a four-year degree) and low-income students (living under 150 percent of the poverty line) (U.S. ED, OPE, 2011); and Upward Bound requires two-thirds of participants to be either first-generation or low-income students, and the remaining third must be either first-generation, low-income, or students who have a high risk of academic failure (U.S. ED, OPE, 2009). GEAR UP uses a 185 percent poverty level for eligibility, so the 150 percent indicator used in this study may be conservative for GEAR UP participants (Kuenzi, 2005)

[28] In his 2009 study examining CAP students using the ELS dataset, Domina found the mean propensity scores were .135 for participants, and .046 for non-participants.

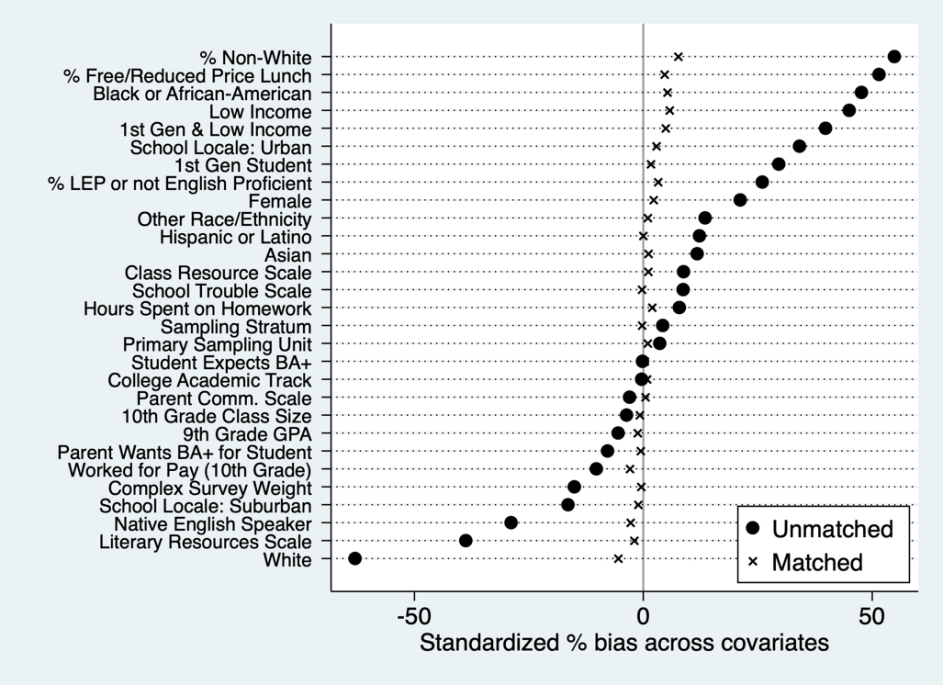
Figure 2
Common Support across all different IPTW and Composite Weighted Specifications



NOTE: Figure shows the common support shared between CAP participants (Treated) and non-participants (Untreated).

With sufficient common support, I assessed the quality of the matched sample by examining the reduction in standardized bias. Both Figure 3 and **Table 5** show the reduction in standardized bias among the predictive covariates of the matched sample. Reduction in bias is examined using primarily two measures: the first is the absolute standardized difference in means of the propensity scores between CAP and non-CAP groups both pre and post matching (Rubin's B). Said another way, Rubin's B shows how many standard deviations apart the means of CAP and non-CAP samples are. Rubin's R represents the ratio of the CAP to non-CAP variances of the propensity scores. Rubin (2001) recommends both measures should be close to one after matching, with values at or beyond .5 and 2 being "far too extreme" (p. 175). Across nearly all variables, the matching procedure reduced standardized bias, with most covariates at or near 1, with the exception of the complex survey weight which retained its variance.

Figure 3
Reduction in Standardized Bias



NOTE*:* Shown is the reduction in standardized bias in the original dataset, though similar figures are observed within each imputed dataset.

Table 6 summarizes the reduction in bias, presenting overall measures of Rubin's B and R showing significant reduction in bias between the matched samples. The remaining three '% concern' represented in this summary table is due to the remaining bias held in the complex survey weight, as it remains the lone variable of concern among the twenty-eight total covariates. After obtaining the propensity scores and examining the extent to which they removed bias from baseline covariates, I used the scores to create propensity score weights. This weighting technique is appropriate under conditions that assignment to participate in a given treatment condition is independent of the outcome variables of both treated and untreated units.

Table 5
Reduction in Standardized Bias by Propensity Score Matched Samples

Variable		CAP	Non-CAP	% Bias	% Reduction	t	p>t	Var(T)/ Var(C)
Female	U	0.65	0.55	20.90		3.61	0.00	0.91
	M	0.65	0.64	2.20	89.60	0.27	0.78	1.02
Native English Speaker	U	0.72	0.84	-29.10		-5.63	0.00	1.63*
	M	0.72	0.73	-2.80	90.50	-0.32	0.75	1.03
Asian	U	0.16	0.12	11.90		2.22	0.03	1.36*
	M	0.16	0.16	1.10	90.70	0.13	0.90	1.04
Black or African-American	U	0.29	0.10	48.00		10.41	0.00	2.41**
	M	0.29	0.26	5.30	88.90	0.57	0.57	1.07
Hispanic or Latino	U	0.16	0.12	12.20		2.28	0.02	1.33*
	M	0.16	0.16	-0.10	99.10	-0.01	0.99	0.97
Other	U	0.08	0.05	13.60		2.68	0.01	1.83*
	M	0.08	0.08	0.90	93.50	0.10	0.92	1.09
White	U	0.31	0.61	-63.50		-10.90	0.00	0.91
	M	0.31	0.34	-5.50	91.30	-0.69	0.49	1.02
First-Gen and Low-Income	U	0.27	0.11	40.00		8.31	0.00	1.94*
	M	0.27	0.25	4.90	87.70	0.53	0.59	1.02
First Gen Student	U	0.73	0.59	30.20		5.08	0.00	0.93
	M	0.73	0.72	1.60	94.70	0.21	0.83	1.01
Low Income Student	U	0.32	0.13	45.20		9.28	0.00	1.72*
	M	0.32	0.30	5.80	87.20	0.63	0.53	1.00
Student Expects BA+	U	0.79	0.80	-0.40		-0.07	0.94	1.13
	M	0.79	0.79	0.20	41.40	0.03	0.98	1.07
Parent Wants BA+ for Student	U	0.87	0.90	-7.90		-1.45	0.15	1.36*
	M	0.87	0.88	-0.60	92.00	-0.07	0.94	1.12
College-Track	U	0.57	0.57	0.00		0.00	1.00	1.01
	M	0.57	0.56	0.80	-3810.60	0.10	0.92	0.99
9th Grade GPA	U	2.83	2.87	-5.60		-0.96	0.34	0.98
	M	2.83	2.84	-1.50	72.90	-0.18	0.85	0.96
Hours Spent on Homework	U	11.55	10.86	7.90		1.41	0.16	1.21
	M	11.55	11.44	1.30	83.50	0.15	0.88	1.05
Worked for Pay (10^{th} Grade)	U	0.25	0.30	-10.90		-1.86	0.06	0.92
	M	0.25	0.26	-1.50	86.20	-0.19	0.85	0.99

School Trouble Scale	U	1.44	1.40	8.90		1.54	0.12	0.95
	M	1.44	1.44	-0.20	97.80	-0.02	0.98	0.87
Class Material Resource Scale	U	1.86	1.79	8.80		1.59	0.11	1.38*
	M	1.86	1.85	0.60	93.10	0.07	0.95	1.09
Parent Communication Scale	U	0.03	0.05	-3.20		-0.53	0.60	1.25*
	M	0.03	0.03	0.20	92.40	0.03	0.98	1.17
Literary Resources Scale	U	3.46	3.97	-38.90		-6.75	0.00	1.39*
	M	3.46	3.50	-2.80	92.80	-0.30	0.76	0.93
Grade 10 enrollment size	U	342.90	350.50	-3.50		-0.60	0.55	1.00
	M	342.90	344.58	-0.80	78.00	-0.10	0.92	1.09
% 10th-graders F/R Lunch	U	38.22	25.78	51.60		9.24	0.00	1.21
	M	38.22	36.90	5.50	89.40	0.59	0.55	1.02
% 10th-graders LEP or NEP	U	6.41	3.96	26.30		5.44	0.00	1.89*
	M	6.41	6.05	3.90	85.20	0.42	0.68	1.12
% 10^{th}-graders Non-White	U	49.58	31.73	55.30		10.47	0.00	1.28*
	M	49.58	47.19	7.40	86.60	0.86	0.39	1.10
Urban	U	0.37	0.22	34.30		6.48	0.00	1.42*
	M	0.37	0.36	2.90	91.60	0.33	0.74	1.02
Suburban	U	0.45	0.53	-16.70		-2.92	0.00	1.01
	M	0.45	0.46	-1.20	93.00	-0.14	0.89	1.04
Primary sampling unit	U	1.56	1.54	3.80		0.68	0.50	0.97
	M	1.56	1.56	0.90	75.50	0.11	0.91	0.95
Stratum	U	237.97	234.98	3.80		0.64	0.52	0.79*
	M	237.97	238.20	-0.30	92.40	-0.04	0.97	0.85
F2F1 Complex Survey Weight	U	263.89	285.66	-15.10		-2.47	0.01	0.63*
	M	263.89	264.62	-0.50	96.70	-0.06	0.95	0.63*

* if 'of concern', i.e. variance ratio in [0.5, 0.8) or (1.25, 2]. ** if 'bad', i.e., variance ratio <0.5 or >2.
NOTE: U = Unmatched, M = Matched
SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), "Base Year, 2002", "First Follow-up, 2004"

Table 6
Overall Imbalance Measures of Unmatched and Matched Samples

				Bias				%	
Sample	Ps R2	LR chi2	p>chi2	Mean	Median	B	R	concern	% bad
Unmatched	0.101	167.37	0	21.3	13.6	96.2*	1.09	52	3
Matched	0.008	4.71	1	2.2	1.3	21.5	0.65	3	0

* if B > 25%, R outside [0.5; 2]

As my primary estimate of interest is the CAP population average treatment effect (PATE), I use the propensity scores to create inverse probability treatment weights as follows:

Eq. 1: $CAP\ Student\ iptw = \frac{1}{p}\ ;\ NonCAP\ Student\ iptw = \frac{\mathbf{1}}{(\mathbf{1}-\boldsymbol{p})}$

The equation creates smaller weights for CAP students as their probability of participation increases, while weights for non-CAP students become larger as the probability of participating in a CAP increases. This weighting approach ensures non-participants who are *most similar* to CAP students are more heavily weighted in outcome models, while also drawing CAP student propensities towards the mean of the overall sample (Guo & Fraser, 2015). As using the IPTW alone would result in sample average treatment effect estimand (SATE), I multiplied the inverse probability weight and the ELS complex survey weight (F2F1WT) in order to create a composite weight, capable of accounting for both the IPT weight and complex survey design. Weighting models using this composite weight allowed for population average treatment effects to be obtained. Returning to **Table 4**, columns 5-7 and 8-10 examine covariate balance between CAP and non-CAP students when the sample is weighted by the IPTW and composite weights, with no notable differences between these groups, suggesting the matching strategy was able to reduce the significant baseline differences between groups.

Analytic Models

As all three outcomes of interest are binary, I relied on logistic regression models to examine whether CAP and non-CAP students differed significantly in their match and fit outcomes. I used a sequential model building using three groupings of variables, with the first models controlling only for demographic characteristics. The second and third models add additional variables to control for academic characteristics (honors weighted GPA, the NCES standardized 10th grade test score, as a proxy for SAT or ACT scores, and the level of selectivity

to which the student is presumed to have access) as well as a set of variables identifying how important the expensiveness of the college, availability of financial aid, and desire to live away from home were in their choice of college to apply and enroll in. Models were weighted by the ELS provided survey weight and the composite weight in order to examine whether observed effects change when comparing a more similar group of students (based on differences in weighting schemes and estimands).

Results

In this section, I present descriptive rates of selectivity access and undermatch by CAP participant status across each sample broken out by the presumed level of selectivity. These rates provide additional context for similarities and differences between participants and non-participants across samples. I follow with a review of the results from the analytic models and conclude by presenting findings from the sensitivity analyses.

Presumed Levels of Selectivity, by CAP Status

To begin the review of results, **Table 7** presents descriptive statistics showing the differences in selectivity access between the CAP and non-CAP groups. The first two columns present the unweighted proportions of each group by the presumed level of selectivity each group has access to, with subsequent columns showing differences when the various weighting schemes are applied. Across both student groups and all weight specifications, between 35 to 40 percent of Non-CAP and CAP students were presumed to have access only to two-year colleges, though this proportion for CAP students was slightly lower when weighted using the IPT and composite weights. Notably, the largest proportion of CAP students with access only to two-year institutions is observed in the regular complex survey weighted sample (nearly 42 percent), though this proportion decreases in both the IPT and composite weighted samples. Across all

specifications, CAP students were predicted to have greater access to *Non-Selective* and *Less Selective*, and more limited access to the *Most Selective* institutions when compared to non-participants. While a smaller proportion of participants is deemed as having access to *Selective* institutions in the unweighted and regular weighted samples, when the IPTW and composite weights are applied the CAP proportion is slightly larger than for non-participants.

Table 7
Predicted Levels of Institutional Selectivity Access, by CAP Status and Sample

Selectivity	Unweighted		Survey Weighted		IPT Weighted		Composite Weighted	
Access	Non	CAP	Non	CAP	Non	CAP	Non	CAP
Two Years	38.8%	39.3%	38.8%	41.8%	39.2%	35.0%	39.2%	35.5%
Non-Selective	5.7%	8.3%	5.9%	7.8%	5.8%	6.9%	6.1%	6.6%
Less Selective	19.5%	22.7%	19.8%	23.8%	19.4%	25.0%	19.7%	25.7%
Selective	22.9%	21.8%	23.3%	20.4%	22.7%	24.0%	23.1%	24.7%
Most Selective	13.1%	8.0%	12.1%	6.2%	12.9%	9.1%	11.9%	7.5%
N	5,610	330	1,602,559	86,029	5,936	5,878	1,688,324	1,648,669

NOTE: Survey weighted sample is weighted by the ELS:2002 provided 1st and 2nd Follow-Up Sample Weight (F2F1WT), the Inverse Probability of Treatment Weight (IPTW) and the Composite Weight (F2F1WT * IPTW). Unweighted sample sizes are rounded to the nearest ten per IES restricted use data requirements.
SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), "First Follow-up, 2004", "Second Follow-up, 2006".

Rates of Undermatch by Presumed Levels of Selectivity and CAP Status

Table 8 presents application and enrollment undermatch rates by the level of selectivity students have access to, and by CAP participant and non-participant status. Across each weighted sample CAP students with access to selective and the most selective institutions were slightly more likely to undermatch when applying to college. At the stage of enrolling in college, within the survey weighted population, CAP students with access to less selective or selective institutions were less likely to undermatch than their peers, though they were much more likely to undermatch near the selectivity extremes (less and most selective).

Table 8

Rate of Undermatch by Level of Selectivity Access and CAP Status, Across Samples

	Application Undermatched		Enrollment Undermatched	
Level of Access	Non-CAP	CAP	Non-CAP	CAP
	Panel A: Complex Survey Weighted			
Two-Years	0.1%	0.0%	25.3%	16.2%
Non-Selective	25.9%	21.1%	43.6%	59.2%
Less Selective	30.7%	29.9%	37.1%	46.0%
Selective	39.3%	39.6%	44.0%	50.2%
Most Selective	33.7%	33.3%	35.5%	67.5%
	Panel B: Inverse Probability of Treatment Weighted			
Two-Years	0.1%	0.0%	25.1%	21.0%
Non-Selective	26.2%	40.6%	43.0%	59.3%
Less Selective	29.8%	28.3%	38.2%	40.4%
Selective	39.0%	46.6%	45.4%	44.4%
Most Selective	31.7%	33.0%	33.4%	61.9%
	Panel C: Composite Weighted			
Two-Years	0.1%	0.0%	25.2%	20.5%
Non-Selective	25.4%	41.0%	43.6%	62.5%
Less Selective	30.7%	30.5%	37.3%	44.8%
Selective	39.5%	51.0%	44.1%	49.1%
Most Selective	33.8%	35.3%	35.6%	72.2%

NOTE: M = Matched, UM = Undermatched. Separated panels represent the proportions of CAP participants and non-participants who undermatched, by their level of presumed access, across each weighted sample, during the application phase, and during the enrollment phase. Complex survey weighted sample is weighted by the ELS:2002 provided 1st - 2nd Follow-up sample weight, F2F1WT; Composite Weighted = complex survey weight multiplied against the inverse probability of treatment weight (IPTW).
SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), "First Follow-up, 2004", "Second Follow-up, 2006", NCES-Barron's Admissions Competitiveness Index File: 2004.

Similar to prior research findings, across all weighted samples CAP students were roughly twice as likely as non-participants to undermatch enrollment when having access to the most selective institutions. It is worth emphasizing the CAP sample of students is small and so these rates are quite sensitive, though it is still helpful to determine what types and under which conditions CAP students were most likely to undermatch. Though not shown, overall, I find that roughly more

than one out of every five students undermatched during the application phase[29]. Of applicants who matched during the application phase, roughly a third undermatched during the enrollment stage[30].

Access Program Participation Impact on Undermatch and Financial Fit Outcomes

Turning to the analytic models, results are presented side by side using the regular complex survey (F2F1WT) and composite (COMP) weighted samples, in a series of sequential models each adding additional control variables. The first set of models control only for demographic background characteristics during the base year, with subsequent models controlling for academic characteristics (honors weighted grade-point average, standardized exam score, and selectivity level students had access to), and a set of college preferences for living away from home, college expenses, and availability of financial aid. College preference variables remained unimputed and are coded with a "missing" category, as I opted not to impute student choices and preferences that likely heavily influence students' application choice set – additionally non-responses may identify students who are 'on-the fence' or may not have a strong preference one way or another.

Table 9, **Table 10**, and **Table 11** present results of program participation on application undermatching, financial fit, and enrollment undermatching. The coefficients were transformed into marginal effects for ease of interpretation. Across all three outcomes in both weighted samples, the results of the analyses find academic match and financial fit outcomes of CAP participants did not significantly differ when compared to non-participants. **Table 9** shows that

[29] Overall application undermatch rates across samples were: Unweighted = 20.3%, Complex Survey Weighted (F2F1WT) = 20.8%, IPT Weighted = 22.2%, Composite Weighted = 23.3%.
[30] Undermatching Rates when Enrolling in College (among students who submitted a match application), across samples were: Unweighted = 32.7%, Complex Survey Weighted (F2F1WT) = 32.8%, IPT Weighted = 33.7%, Composite Weighted = 34.5%.

in the F2F1WT sample, program participants who were potential first-generation college students were roughly 8 to 10 percent more likely to undermatch, relative to students whose parents had four-year degrees, when controlling for academic characteristics and preferences. These differences disappear when the sample is weighted by the composite weight, however. Moreover, across all models while the results were not statistically significant, program participants were found to be more likely to undermatch during the application phase. Outcomes were roughly equivalent during the enrollment phase with respect to undermatch. With respect to academically undermatching during both application and enrollment phases, the results are discouraging but consistent with the findings of Domina (2009) and Glennie et al. (2015), who observed that these programs did not seem to impact overall college-going (nor four-year college-going). Given the large proportion of CAP students who are determined to have access only to two-year colleges, undermatching would mean either not applying to *any* institutions, or opting not to enroll anywhere if they applied and were admitted.

Table 9

Marginal Effects of CAP Participation on Application Undermatch

	F2F1WT	COMP	F2F1WT	COMP	F2F1WT	COMP
CAP Student	0.04	0.06	0.00	0.04	0.01	0.03
	(0.03)	(0.05)	(0.03)	(0.04)	(0.03)	(0.04)
Female	0.03*	0.02	0.01	0.01	0.01	0.02
	(0.01)	(0.03)	(0.01)	(0.03)	(0.01)	(0.03)
First language is English	0.04	0.15*	0.03	0.12*	0.04	0.09
	(0.03)	(0.06)	(0.02)	(0.06)	(0.02)	(0.05)
Asian	-0.08**	-0.10	-0.11***	-0.12*	-0.11***	-0.14**
	(0.03)	(0.07)	(0.02)	(0.06)	(0.02)	(0.04)
Black	-0.21***	-0.21**	-0.10***	-0.11	-0.10***	-0.09
	(0.03)	(0.07)	(0.03)	(0.06)	(0.02)	(0.05)
Hispanic	-0.11***	-0.04	-0.02	0.05	-0.02	-0.02
	(0.02)	(0.07)	(0.02)	(0.06)	(0.02)	(0.04)
Other	-0.10**	-0.13*	-0.04	-0.06	-0.03	-0.05
	(0.03)	(0.06)	(0.03)	(0.05)	(0.03)	(0.05)
First Gen Student	0.02	-0.10	0.10***	-0.01	0.08***	-0.01
	(0.01)	(0.05)	(0.01)	(0.04)	(0.01)	(0.04)
Low Income Student	-0.05*	-0.04	0.01	0.01	0.01	0.03
	(0.02)	(0.05)	(0.02)	(0.05)	(0.02)	(0.05)
Access to Two-Year			-0.94***	-1.02***	-0.93***	-0.95***
			(0.10)	(0.12)	(0.10)	(0.11)
Access to Non-Selective			-0.17***	-0.07	-0.17***	-0.05
			(0.03)	(0.09)	(0.03)	(0.08)
Access to Less- Selective			-0.09***	-0.10*	-0.10***	-0.10*
			(0.02)	(0.05)	(0.02)	(0.04)
Access to Most Selective			0.03	-0.03	0.04*	0.02
			(0.02)	(0.04)	(0.02)	(0.04)
Honors Weighted GPA			-0.05**	-0.04	-0.05**	-0.05
			(0.02)	(0.04)	(0.02)	(0.04)
Standardized test score			-0.00**	0.00	-0.00**	0.00

Table 9

Marginal Effects of CAP Participation on Application Undermatch

	F2F1WT	COMP	F2F1WT	COMP	F2F1WT	COMP
			(0.00)	(0.00)	(0.00)	(0.00)
College Expenses: Missing					-0.05	0.35***
					(0.07)	(0.10)
College Expenses: Important					0.05***	0.10***
					(0.01)	(0.03)
Aid Available: Missing					0.05	-0.23***
					(0.08)	(0.07)
Aid Available: Important					0.05**	-0.02
					(0.02)	(0.05)
Living Away: Missing					0.08	-0.14
					(0.10)	(0.17)
Living Away: Important					-0.11***	-0.18***
					(0.01)	(0.03)
N (Unweighted)	5,940	5,940	5,940	5,940	5,940	5,940
N (Weighted)	1,688,588	3,336,993	1,688,588	3,336,993	1,688,588	3,336,993

* $p < .05$, ** $p < .01$, *** $p < .001$.

NOTE: Marginal effects for factor levels represents the discrete change from the base level. Reference categories in order, are: White, Access to Selective, College Expenses: Not Important, Fin. Aid Availability: Not Important, Living Away from Home: Not Important. F2F1WT = ELS:2002 1st-2nd Follow-up complex survey weight, COMP = Composite Weight Representing the Interaction Between the F2F1WT weight and IPTW. Unweighted sample sizes are rounded to the nearest 10 per IES restricted-use data requirements.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), "Base Year, 2002", "First Follow-up, 2004", "Second Follow-up, 2006", NCES-Barron's Admissions Competitiveness Index File: 2004.

Table 10

Marginal Effects of CAP Participation on Financial Match (Aid > COA; Loans are 20% or Less of COA)

	F2F1WT	COMP	F2F1WT	COMP	F2F1WT	COMP
CAP Student	0.03	0.03	0.03	0.04	0.03	0.05
	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)
Female	-0.03*	-0.00	-0.02	-0.01	-0.02	-0.02
	(0.01)	(0.04)	(0.01)	(0.04)	(0.01)	(0.04)
First language is English	-0.11***	-0.02	-0.11***	-0.02	-0.11***	-0.03
	(0.03)	(0.09)	(0.03)	(0.09)	(0.03)	(0.08)
Asian	-0.01	0.12	-0.01	0.08	-0.01	0.08
	(0.03)	(0.09)	(0.03)	(0.07)	(0.03)	(0.07)
Black	-0.13***	-0.09*	-0.14***	-0.08	-0.11***	-0.08
	(0.02)	(0.04)	(0.02)	(0.05)	(0.02)	(0.05)
Hispanic	0.07*	0.09	0.06	0.08	0.06	0.08
	(0.03)	(0.07)	(0.03)	(0.07)	(0.03)	(0.07)
Other	0.04	0.08	0.03	0.08	0.04	0.07
	(0.04)	(0.07)	(0.04)	(0.06)	(0.04)	(0.06)
First Gen Student	-0.05***	-0.02	-0.05***	-0.02	-0.05**	-0.01
	(0.02)	(0.04)	(0.02)	(0.04)	(0.02)	(0.04)
Low Income Student	0.05*	-0.05	0.05*	-0.04	0.05*	-0.04
	(0.02)	(0.05)	(0.02)	(0.05)	(0.02)	(0.05)
Access to Two-year			0.04	-0.04	0.03	-0.02
			(0.03)	(0.09)	(0.03)	(0.08)
Access to Non-Selective			0.01	-0.06	0.00	-0.04
			(0.04)	(0.09)	(0.04)	(0.09)
Access to Less- Selective			-0.02	-0.08	-0.03	-0.08
			(0.02)	(0.06)	(0.02)	(0.05)
Access to Most Selective			0.06**	0.10*	0.06*	0.09
			(0.02)	(0.05)	(0.02)	(0.05)
Honors Weighted GPA			-0.02	0.00	-0.02	0.02
			(0.02)	(0.05)	(0.02)	(0.05)
Standardized test score			0.00	-0.00	0.00	-0.00

Table 10

Marginal Effects of CAP Participation on Financial Match (Aid > COA; Loans are 20% or Less of COA)

	F2F1WT	COMP	F2F1WT	COMP	F2F1WT	COMP
			(0.00)	(0.00)	(0.00)	(0.00)
College Expenses: Missing					0.01	-0.10
					(0.14)	(0.19)
College Expenses: Important					-0.03	-0.06*
					(0.02)	(0.03)
Aid Available: Missing					-0.06	0.17***
					(0.14)	(0.04)
Aid Available: Important					-0.13***	-0.09*
					(0.02)	(0.04)
Living Away: Missing					-0.01	-0.57**
					(0.09)	(0.19)
Living Away: Important					-0.06***	-0.04
					(0.02)	(0.04)
N (Unweighted)	4,870	4,870	4,870	4,870	4,870	4,870
N (Weighted)	1,376,869	2,701,397	1,376,869	2,701,397	1,376,869	2,701,397

* p < .05, ** p < .01, *** p < .001.

NOTE: Marginal effects for factor levels represents the discrete change from the base level. Reference categories in order, are: White, Access to Selective, College Expenses: Not Important, Fin. Aid Availability: Not Important, Living Away from Home: Not Important. F2F1WT = ELS:2002 1st-2nd Follow-up complex survey weight, COMP = Composite weight representing the interaction between the F2F1WT weight and IPTW. Unweighted sample sizes are rounded to the nearest 10 per IES restricted-use data requirements.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), "Base Year, 2002", "First Follow-up, 2004", "Second Follow-up, 2006", NCES-Barron's Admissions Competitiveness Index File: 2004.

Table 11

Marginal Effects of CAP Participation on Enrollment Undermatch

	F2F1WT	COMP	F2F1WT	COMP	F2F1WT	COMP
CAP Student	0.03	0.03	0.01	-0.00	0.02	0.02
	(0.03)	(0.05)	(0.03)	(0.04)	(0.03)	(0.04)
Female	-0.05***	-0.09*	-0.05**	-0.07*	-0.03*	-0.07*
	(0.01)	(0.04)	(0.01)	(0.03)	(0.01)	(0.03)
First language is English	0.03	0.13*	0.04	0.13*	0.05	0.14**
	(0.03)	(0.06)	(0.03)	(0.05)	(0.03)	(0.05)
Asian	-0.02	0.11	-0.04	0.03	-0.04	0.03
	(0.04)	(0.10)	(0.03)	(0.08)	(0.03)	(0.08)
Black	-0.10***	-0.12*	-0.10***	-0.10*	-0.08**	-0.09
	(0.03)	(0.06)	(0.03)	(0.05)	(0.03)	(0.05)
Hispanic	-0.04	0.02	-0.02	0.03	-0.01	0.03
	(0.03)	(0.06)	(0.03)	(0.06)	(0.03)	(0.06)
Other	-0.04	-0.06	-0.04	-0.05	-0.03	-0.04
	(0.04)	(0.07)	(0.04)	(0.06)	(0.04)	(0.06)
First Gen Student	0.05**	0.05	0.10***	0.13**	0.08***	0.11**
	(0.02)	(0.05)	(0.02)	(0.04)	(0.02)	(0.04)
Low Income Student	0.02	0.03	0.04	0.04	0.04	0.04
	(0.02)	(0.05)	(0.02)	(0.05)	(0.02)	(0.05)
Access to Two-year			-0.42***	-0.59***	-0.45***	-0.60***
			(0.03)	(0.08)	(0.03)	(0.08)
Access to Non-Selective			-0.17***	-0.24**	-0.16***	-0.24**
			(0.04)	(0.08)	(0.04)	(0.08)
Access to Less- Selective			-0.14***	-0.18**	-0.14***	-0.18**
			(0.03)	(0.06)	(0.03)	(0.06)
Access to Most Selective			0.03	0.13*	0.03	0.13*
			(0.03)	(0.06)	(0.03)	(0.06)
Honors Weighted GPA			-0.10***	-0.10*	-0.09***	-0.10*
			(0.02)	(0.04)	(0.02)	(0.04)
Standardized test score			-0.01***	-0.01***	-0.00***	-0.01***

Table 11

Marginal Effects of CAP Participation on Enrollment Undermatch

	F2F1WT	COMP	F2F1WT	COMP	F2F1WT	COMP
			(0.00)	(0.00)	(0.00)	(0.00)
College Expenses: Missing					0.27*	0.05
					(0.12)	(0.15)
College Expenses: Important					0.05*	0.01
					(0.02)	(0.05)
Aid Available: Missing					0.06	0.44**
					(0.12)	(0.15)
Aid Available: Important					0.01	0.05
					(0.03)	(0.05)
Living Away: Missing					0.01	-0.14
					(0.10)	(0.12)
Living Away: Important					-0.07***	-0.06
					(0.02)	(0.04)
N (Unweighted)	4,720	4,720	4,720	4,720	4,720	4,720
N (Weighted)	1,334,723	2,559,351	1,334,723	2,559,351	1,334,723	2,559,351

* $p < .05$, ** $p < .01$, *** $p < .001$.

NOTE: Marginal effects for factor levels represents the discrete change from the base level. Reference categories in order, are: White, Access to Selective, College Expenses: Not Important, Fin. Aid Availability: Not Important, Living Away from Home: Not Important. F2F1WT = ELS:2002 1st-2nd Follow-up complex survey weight, COMP = Composite weight representing the interaction between the F2F1WT weight and IPTW. Unweighted sample sizes are rounded to the nearest 10 per IES restricted-use data requirements.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), "Base Year, 2002", "First Follow-up, 2004", "Second Follow-up, 2006", NCES-Barron's Admissions Competitiveness Index File: 2004.

Sensitivity Analysis

While I observe no statistically significant effects of program participation on any of the outcomes, it should be noted that propensity score methods and subsequent analyses may be significantly impact by omitted and hidden variable bias, particularly if variables related to CAP participation are omitted from the predictive equations. Though ELS provides a rich set of data on students, there may be additional factors above and beyond those included that may be Haenszel Bounds (Imbens & Rubin, 2015) to examine the strength required for an unobserved predictor to influence estimated program impacts.

Results from the sensitivity analysis are presented in **Table 12**. With an assumption that no hidden bias was present, gamma(Γ) would equal 1. The table can then be read to understand that larger values represent the extent of bias needed to influence results in either direction. For example, gamma values at 1.5 and 2.0 would represent the unobserved variable being associated with odds one and a half to two times greater of participating in a CAP compared to non-participants. Though other studies have used similar methods to study CAP students in this dataset, none have examined the extent to which omitted variable bias may influence results. I find that for outcomes under consideration, – and with particular attention to application undermatch, the potential to overstate an effect is extremely sensitive, using a conservative 10% significance level. The possibility of finding that CAP students were more likely to undermatch due to an influential unobserved covariate could occur at Γ = 1.05. Said another way, significantly different results might be obtained if an unobserved predictor caused the odds of being in the treatment group to differ by a factor of 1.05 between CAP and non-CAP students. Similar highly sensitive effects are observed for the other outcomes as well. Though this test does not formally determine whether there *is* hidden bias, the finding that bias could strongly

influence results with even the slightest change due to unobserved characteristics suggests there are likely many additional factors and conditions contributing to student participation in these programs. This may be the case due to individual programs implementing criteria or requirements for participation above and beyond the ones noted in this study. The lack of uniformity among programs likely also represents a lack in uniformity among requirements for participation in a given program at a particular school site.

Table 12

Rosenbaum Bounds Sensitivity Analysis (Mantel-Haenszel Statistic)

	Application Undermatch				Financial Match				Enrollment Undermatch			
	Q_mh		P_mh		Q_mh		P_mh		Q_mh		P_mh	
Gamma	+	-	+	-	+	-	+	-	+	-	+	-
1.00	1.17	1.17	0.12	0.12	0.90	0.90	0.18	0.18	0.06	0.06	0.48	0.48
1.05	1.50	0.84	0.07	0.20	0.57	1.24	0.28	0.11	0.42	0.17	0.34	0.43
1.10	1.82	0.53	0.03	0.30	0.26	1.55	0.40	0.06	0.77	0.51	0.22	0.30
1.15	2.12	0.23	0.02	0.41	-0.04	1.85	0.52	0.03	1.10	0.84	0.14	0.20
1.20	2.41	-0.06	0.01	0.52	0.18	2.14	0.43	0.02	1.42	1.16	0.08	0.12
1.25	2.69	0.18	0.00	0.43	0.45	2.42	0.33	0.01	1.72	1.46	0.04	0.07
1.30	2.96	0.45	0.00	0.33	0.72	2.69	0.24	0.00	2.02	1.76	0.02	0.04
1.35	3.22	0.70	0.00	0.24	0.97	2.95	0.17	0.00	2.30	2.04	0.01	0.02
1.40	3.48	0.94	0.00	0.17	1.22	3.20	0.11	0.00	2.57	2.31	0.01	0.01
1.45	3.72	1.18	0.00	0.12	1.45	3.45	0.07	0.00	2.84	2.57	0.00	0.01
1.50	3.96	1.41	0.00	0.08	1.68	3.68	0.05	0.00	3.09	2.83	0.00	0.00
1.55	4.19	1.63	0.00	0.05	1.90	3.91	0.03	0.00	3.34	3.08	0.00	0.00
1.60	4.41	1.84	0.00	0.03	2.12	4.13	0.02	0.00	3.58	3.32	0.00	0.00
1.65	4.63	2.05	0.00	0.02	2.33	4.35	0.01	0.00	3.82	3.55	0.00	0.00
1.70	4.84	2.25	0.00	0.01	2.53	4.56	0.01	0.00	4.05	3.78	0.00	0.00
1.75	5.05	2.45	0.00	0.01	2.73	4.77	0.00	0.00	4.27	4.00	0.00	0.00
1.80	5.25	2.64	0.00	0.00	2.92	4.97	0.00	0.00	4.49	4.22	0.00	0.00
1.85	5.44	2.83	0.00	0.00	3.11	5.16	0.00	0.00	4.70	4.43	0.00	0.00
1.90	5.64	3.01	0.00	0.00	3.30	5.35	0.00	0.00	4.91	4.63	0.00	0.00
1.95	5.82	3.19	0.00	0.00	3.48	5.54	0.00	0.00	5.11	4.83	0.00	0.00
2.00	6.01	3.36	0.00	0.00	3.65	5.72	0.00	0.00	5.31	5.03	0.00	0.00

NOTE: Gamma : odds of differential assignment due to unobserved factors. Q_mh+ and Q_mh- : Mantel-Haenszel statistic (assumption: over and underestimation of the treatment effect, respectively). P_mh+ and p_mh- : significance level (assumption: over and underestimation of treatment effect, respectively).
SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), "Base Year, 2002", "First Follow-up, 2004", "Second Follow-up, 2006", NCES-Barron's Admissions Competitiveness Index File: 2004.

Limitations

While several methods were employed to help address issues of validity and concern, there are a number of limitations readers should keep in mind. First, though methods were employed to present population level findings of CAP participation, this analysis does not attempt nor try to make causal claims. The methods employed in this study attempt only to ensure associated outcomes of program participation are estimated using an appropriate comparison group (e.g., a group of students most similar to program participants). Second, and in agreement with concerns raised by both Domina (2009) and Glennie et al. (2015) the program participation data appear inconsistent – for example, though Gear Up programs are by design, open to an entire cohort, the vast majority of Gear Up participants were the only student at their school who reported participating. To the extent that students with higher levels of participation are predisposed to identify themselves as participants compared to same school peers who may participate less, the data may more accurately reflect active and engaged participants. As Glennie et al. (2015) aptly note in discussing potential causes for such concerns, "some schools create their own names for their program (e.g. "Ready, Set, College!"). When students do not know the proper name of that program (e.g., "Upward Bound"), they may misidentify the program" (p. 968). I attempted to use a set of questions from the base year administrator survey which asked staff to identify the proportions of students attending the school who participate in Upward Bound, Talent Search, and/or Upward Bound, but there are inconsistencies between student and administrator responses (e.g. a student reports participating in a program but the administrator reports no students in the school participating in said program). To the extent student participants answered "no" for any of these reasons, this study could underestimate program effects on the outcomes studied. Moreover, though the dataset used is rich in information, the information it

contains reports on students who were in tenth grade twenty years ago, and thus does not account for changes in todays college-going landscape (e.g., FAFSA being moved and now completed and submitted completely online for example).

Discussion

Limitations notwithstanding, the analysis presented here provides additional context, and information to the literature with respect to federal pre-college programs and undermatching outcomes. First, the results suggest college access program participants experience rates of undermatch and financial fit that are similar to non-participants. Given the ability of students to participate in these programs for extended periods of time often over multiple academic years, such results may be disappointing to some. It is worth emphasizing that while this outcome may be disappointing to proponents of access programs and others, the *results do not suggest* students who participated in these programs undermatched at greater rates – said differently, CAP students were not more likely than similar peers to undermatch neither while applying nor during the enrollment decision.

Moreover, the analysis demonstrates the potential of using nationally representative datasets to investigate whether students who participate in a specific information rich targeted intervention could improve match and fit outcomes. As researchers have limited available student-level data for participants in the federal programs, longitudinal studies like ELS are a potential route to examine similar enrollment outcomes. Still, the analyses here offer a number of contributions to the existing literature. By adopting methodologically similar techniques including the use of most popular method to estimate undermatch (the predicted probability method), these results provide additional information on undermatching outcomes of ELS cohort members, and particularly for students participating in a targeted information centered

intervention. Specifically, the theoretical transmission of social capital through CAPs to students does not appear to necessarily translate to influencing match and fit measures.

Though not the specific focus of the current analysis, preliminary results from *Find the Fit* (Martinez et al., 2018) may provide an important clue as to why these programs do not appear to influence match outcomes either way: while the package of enhanced advising strategies was found to positively (and significantly) impact the number of colleges a student applied to overall, as well as those with greater levels of selectivity, and FAFSA submission rates for some subgroups, these represent concrete changes in behavior, rather than a change in disposition, priorities, or valuation of importance. With respect to the latter, when asked whether participating in UB influenced the importance of academic quality with respect to the institutions they wanted to attend, students who received the treatment did not significantly differ in their responses than control students. It may be the case this finding is driven by the vast majority of both treated (77.3 percent) and control (78.1 percent) groups already highly valuing academic quality when choosing a college, leaving only a small portion of students to 'move'. Despite this, the inability of the intervention to impact this measure may be the most consequential as undermatching efforts require students to be able to effectively differentiate and benchmark their ability against colleges, and believe undertaking such assessments are in service of increasing their probability of successfully graduating. Alternatively, it may also be the case that other practical considerations take precedence over considerations of attending a match college. For example, as the matching and weighting strategy employed in the current study did not include a measure of proximity to the nearest match institution, it could be the case that location restricted students or those needing/wanting to live close to family, undermatched if they did not live reasonably close to a match institution, as found by Ovink et al. (2018).

Perhaps a subtle, though equally consequential finding of the current analysis is the low predictive propensity scores of both, students who did and those who did not participate in a program. The scores obtained here are similar to those reported by Domina (2009) who notes "considerable propensity-score overlap between the program participants and non-participants in the ELS sample… [indicating] a large number of likely outreach program participants in American public high schools who have not yet been recruited into college outreach programs" (p. 137). This point is important in that it supports demonstrates the need for programs beyond the federal projects examined here, in helping students through the application process. Beyond the overlap noted, however, the low propensity scores suggest there may be substantial variation in the characteristics of students who participate. This is all the more important to recognize given that programs may recruit students or may require students formally apply to participate. In the latter case, with potential for requirements to vary from site to site (e.g., academic ability such as a minimum GPA, aspirations to attend college/graduate school, recommendations from teachers and counselors, family engagement commitment, etc.) the 'typical' student profile may look quite different, and perhaps less 'targeted.'

CHAPTER 4:

NEAR-PEER ADVISING PRORAMS AND COLLEGE ENROLLMENT OUTCOMES

The second analysis of this dissertation adopts a difference-in-difference strategy to empirically investigate the extent to which near-peer college access programs improve college-going metrics within schools; specifically: (1) raising graduation rates, increasing college-going rates, investigating whether changes in college-going rates (if any) are due to more students enrolling or whether enrollment patterns between four and two-year institutions shift, and by examining whether the programs improve the rate of students who persist once in college. Additionally, though some prior literature examining near-peer college access programs have focused on identifying impacts for lower-income or low-socioeconomic status students specifically, the current study focuses on examining impacts on college-going outcomes by student subgroups.

The decision to focus on rates between subgroups was motivated in part, by recent declines in college-going rates which vary by race and ethnicity according to data published by NCES (National Center for Education Statistics, 2021). Though the extent of the declines have varied across overall subgroups, an emerging commonality in these trends has led to wide gaps in college-going rates of female and male students within subgroups. When compared to ten years prior, by the 2019-20 academic year the gap in college-going rates between males and females has barely budged for Black students (from 8.7 in 2009-10 to 8.4), increased slightly for White students (from 5.4 to 8.9) and nearly doubled for Hispanic or Latino students (from 6.8 to

12.5)[31]. Given these increasing gaps between male and female student outcomes that appear to vary by subgroup, investigating interventions designed to improve college-going should also consider whether effects vary not only between subgroups along race and ethnicity, but also by sex within said groups.

This is particularly relevant, as the near-peer programs under investigation in the second analysis operate under a schoolwide rather than targeted service model (e.g., working with a specific group of participating students). Given this approach, it is conceivable that certain groups of students may be more inclined to take-up the offer of college advising with a near-peer adviser than others. As observed in the first analysis for example, students who Black or African-American students and native English-speaking students were significantly more likely to participate in one of the federal access programs. Similarly, female students were also much more likely to participate in these programs, constituting nearly two out of every three participants (64 percent). Therefore, given both recent diverging trends in college-going patterns by race and ethnicity and sex, and the near-peer model approach to offering schoolwide college advising services, the current investigation examines the effect of near-peer programs on outcomes disaggregated by race and ethnicity and sex.

[31] According to Digest of Education Table 302.60 from the National Center for Education Statistics 2021 report in the 2009-10 year college-going rates within subgroups were: 42.3% (White males) compared to 47.7% (White females), 33.2% (Black males) compared to 41.9 % (Black females), and 24.2% (Hispanic or Latino) compared to 31.0% (Hispanic or Latina). In the 2019-20 year, the same groups in the same order were: 36.5% to 45.4%, 31.4% to 39.8%, and 29.9% to 41.7%.

Background Context

One of the newest College Advising Corps (CAC) programs established in the 2010-11 school year was the Michigan College Advising Corps (MCAC) hosted by the University of Michigan's Center for Educational Outreach (University of Michigan Center for Educational Outreach, 2011). In the preceding 2009-10 school year, Michigan had one of the largest student to counselor ratios in the nation – at 660 students to every 1 school counselor, Michigan was approaching a ratio nearly three times the size recommended by the American School Counselor Association (American School Counselor Association, 2022b). Thus, the introduction of MCAC served as a strategy with which to combat these large counseling ratios and provide access to and guidance from a near-peer adviser capable of helping students navigate their own college application processes. The Michigan expansion continued into the 2011-12 school year seeing both MCAC nearly double in size from eight to fifteen advisers, and the creation of a new chapter at Michigan State University (MSUCAC) which started off serving 24 schools that year. While the MCAC chapters size remained relatively stable, serving between 12 to 15 schools each school year through the 2020-21 year, MSUCAC began by serving 24 schools, and by 2020-21 was serving in 41 schools.

Similar to the national CAC, both chapters developed and expanded due to funding from private organizations, as well as a partnership with a broader statewide initiative in Michigan, the Michigan College Access Network (MCAN). Acting as a larger consortium, organizing college access resources across the state, MCAN sought to create a broader infrastructure to support college-going initiatives (Daun-Barnett & Holohan-Moyer, 2013) and boost economic prosperity for Michiganders, by supporting the Michigan state goal of doubling (to 60 percent) the proportion of Michigan residents who earned a degree or high-quality credential by the year 2025 MCAN, 2009; NCAC, 2011). Though the two CAC programs served a large number of

students, Michigan's investment and commitment to the effort increased significantly during the 2015-16 school year when MCAN itself developed and scaled a near-peer advising program. MCAN explained,

> *"In Michigan, the student-to-counselor ratio exceeds 700:1 and high schools have inadequate capacity to provide one-to-one college advising…[p]reviously, MCAN partnered with the National College Advising Corps to establish college advising programs at University of Michigan and Michigan State University. Despite growth, the UM and MSU College Advising Corps only serve a fraction of Michigan high schools that need the program. MCAN launched the AdviseMI program in Fall of 2015 to help serve that need"* (MCAN, 2016, para. 3). Serving forty-seven schools in the first year, the new Advise-Michigan (AdviseMI) program had roughly as many advisers as the two CAC programs. Since this time, all three programs have continued to serve schools across the state.

Research Questions

To investigate whether near-peer college access programs impact school-level college-going outcomes, this study employs a DID design to answer the following set of research questions: Does near-peer college access program implementation:

1. Lead to increased high school graduation rates?
2. Lead to increased college-going rates? If so, are effects equitably distributed across racial groups or are they concentrated in specific racial/ethnic groups?
3. Induce more students to enroll in college, or are there shifts in enrollment patterns? Are changes/shifts concentrated among student subgroups?
4. Lead to an increase in the percentage of college enrollees who earn at least one year of college credits (24 credits) in their first year of college?

As near-peer advising programs aim increase the number of low-income and underrepresented students attending and persisting in college, I hypothesize graduation, college-going, and credit attainment rates should increase, though less clear is the effect on proportional attendance patterns (four-year versus two-years). Intuitively, the introduction of the near-peer adviser should provide additional assistance to students applying to four-year colleges as these application processes often require many additional components and steps. However, the adviser may also encourage students not thinking about attending college at all to consider starting at a two-year institution, and thus the increase in students attending two-year schools may proportionally outweigh any gains in four-year attendance.

Methodology

The subsections that follow outline my analytic strategy in conducting the second analysis. I first discuss the data and schools included in the analytic sample. Thereafter I discuss the difference-in-difference design, specifically describing the two-way fixed effects modeling strategy and supplementary event study analyses, as well as the comparison groups used compare the treated near-peer schools. This is followed by a discussion of the placebo and falsification tests used to test the sensitivity and robustness of the results. Finally, I conclude with a discussion of the limitations of this second analysis, before presenting the results.

Data and School Sample

To answer the research questions above, I constructed a school-level panel dataset of all public high schools in Michigan spanning the 2007-08 through 2020-21 school years. The panel was constructed with publicly available school level data from the Michigan Center for

Educational Performance and Information (CEPI)[32], the NCES Common Core of Data, and data provided by CAC, MCAN, and their websites. Postsecondary outcome data were provided to CEPI by the National Student Clearinghouse (NSC), thus this study examines college-going outcomes of Michigan high school graduates nationally and not only those who attended institutions located in the state of Michigan. Due to differences in applied data suppression rules, the number of observations per dependent variable (by subgroup) ranges, though the analytic sample contains roughly 630 to 640 high schools per school year, spanning the 13-year panel. Fluctuations of the sample size are due to mergers, closures, and openings of schools. Of the 640 high schools, 180 schools were treated in at least one school year between the 2007 through 2020 school years. **Table 13** presents the number of schools in the sample served by each program over time.

Outcome Variables

This study examined four school-level outcomes: graduation rates, college-going rates, proportion of enrollees who enrolled in a four-year institution, and the percentage of enrollees who earned twenty-four credits within one year of enrolling. I briefly detail each outcome below:

Graduation Rates. Michigan reports the adjusted cohort graduation rate formula standard set by the National Governor's Association Graduation Counts Compact (Michigan Center for Educational & Performance and Information, 2022). The method assigns students to a graduation cohort based on the year they entered high school as ninth graders – so students who were ninth graders in the 2017-18 year are members of the 2021 cohort as an 'on-time' four-year graduation would see them graduate during the 2020-21 year. The graduation rate variable is a percentage

[32] Data for outcomes were gathered from the following reports: College Readiness, College Enrollment by High School, and Historical Graduation/Dropout Files, Historical Economically Disadvantaged Student Counts, Dual Enrollment Counts, School and Staffing Summary Full-Time Equivalencies Data (Michigan Center for Educational Performance and Information, 2021b, 2021a, 2021e, 2021d, 2021f, 2021c).

derived from the total number of students considered to be 'on-track' graduates divided by the number of students in the cohort four-years earlier. Students are considered as on-track graduates if they "completed high school with a regular diploma in four years or less; or if enrolled in an early/middle college, completed high school with a regular diploma AND an associate degree or other advanced certificate in five years or less" (p. 13)[33].

Table 13

Schools Served Over Time, By Program

	Near-Peer College Advising Program			
School Year	Not served	MCAC	MSUCAC	Advise-MI
2007 – 2008	621			
2008 – 2009	621			
2009 – 2010	631			
2010 – 2011	629	8		
2011 – 2012	602	15	24	
2012 – 2013	595	13	26	
2013 – 2014	600	13	29	
2014 – 2015	595	15	33	
2015 – 2016	546	12	35	47
2016 – 2017	536	12	35	55
2017 – 2018	530	12	34	64
2018 – 2019	522	13	37	61
2019 – 2020	517	15	35	65
2020 – 2021	522	14	41	64

NOTE: Table shows the number of Michigan high schools (according to the NCES Common Core of Data Definition), by advising program partner school status. MCAC = University of Michigan College Advising Corps; MSUCAC = Michigan State University College Advising Corps; Advise-MI = Advise Michigan.
SOURCE: School Level Files: School Years: 2007-08 through 2020-21, Common Core of Data (CCD), U.S. Department of Education, National Center for Education Statistics; College Advising Corps; Michigan College Access Network

[33] Students are considered 'off-track' if they earned a GED, received a special education certificate of completion or other certificate (or aged-out of special education), did not finish high school in four-years and are continuing, or left high school permanently at any point during their fourth year in high school.

Immediate Overall College-Going Rate. This outcome is derived by CEPI and identifies the percentage of a high schools graduates who enrolled in college within six months of high school graduation. College enrollment data is provided to CEPI by the National Student Clearinghouse which tracks college enrollment nationally and is currently the most robust and widely utilized data collection to track enrollment patterns.

Percent Four-Year College-Going. This measure represents the *proportion of a high schools' overall college-going population* that enrolled in a four-year institution within the six months following their graduation. As this rate is not provided by CEPI, I derived this measure by taking the percent of four-year college going overall, divided by the immediate overall college-going rate.

Percent of Enrollees Who Earned 24 Credits in One Year. This variable is a proxy measure of persistence, measuring the proportion of a high school's college-enrollees (both four and two year enrollees) who earn one year of credit within the first twelve months of their enrolling. I derived this measure by taking the overall number of students earning 24 credits, over the total number of college enrollees.

Of note, CEPI apply data disclosure rules before publishing public facing data, with such techniques suppressing student counts for student subgroups when subgroups are fewer than ten students, and suppressing percentages at the extremes. For example, high schools with college-going rates lower than five percent or larger than 95 percent are bottom ("< 5%") and top-coded (" > 95%) – for the current study these values were recoded with values of 4 and 96 in order to distinguish them from actual observed rates of "5.0" and "95.0", respectively. Each outcome is disaggregated, by race/ethnicity and sex to investigate whether effects differ between groups. These distinctions allow the analytic models to distinguish and estimate separately the effects of

the advising program on college-going rates of all students, all female students, all male students, black students, black female students, and so forth.

Analytic Strategy

Under ideal conditions, in order to reliably estimate a causal treatment effect of near-peer advising programs on partner school outcomes, I would observe treated schools under conditions where they received the treatment and separately did not receive the treatment (the counterfactual), and compare the outcomes between the two outcomes. Of course, it is impossible to observe a true counterfactual outcome, in order to identify what *would have* happened if schools did not receive the treatment, and thus researchers utilize a variety of techniques to attempt to estimate causal effects. Many researchers turn to randomized control trials in order to control for self-selection into a treatment condition. Under such conditions where randomization procedures are sufficient and assignment conditions are implemented with fidelity, the control schools would approximate a counterfactual condition – what *would have* occurred if the treated schools had not received the treatment. This was the case with Bettinger and Evans (2019) who also studied the near-peer advising program treatment on student outcomes, though even this design they faced a number of challenges due to attrition, and schools crossing over into a treatment condition not initially assigned.

A primary challenge in any causal research design is the identification strategy utilized, as it is unlikely that the raw data available will alone reliably tell the story of the impact of a given intervention or event. In the current analysis for example, the near-peer advising program treatment was not randomly assigned to schools, as advising programs conduct outreach to community partners and schools in order to place advisers in schools with higher need student populations. As this is the case, the variation of outcomes in the raw data is likely confounded by

the factors which lead some schools to be treated and others to remain untreated. Identification is understood as the process of figuring out how to extract and isolate the variation in the data which answers the research question(s) at hand, so as to ensure results obtained are reliant on the specific theoretical mechanism of interest (Huntington-Klein, 2022). To identify the effect of near-peer program implementation on partner school outcomes, I use a difference-in-differences (DID) approach to compare outcomes before and after program implementation. The first difference examines the outcomes before and after treatment of the group that is treated while the second difference is taken by comparing before and after outcomes over the same period, for schools that were not treated. Representing the within group variation of the treated and control groups respectively, the second difference is then subtracted from the first, with the remaining difference (hence 'difference'-in-differences) serving as the estimated treatment effect, since the in group variation has already been accounted for (Gertler et al., 2010; Huntington-Klein, 2022).

Difference-In-Differences

In a traditional 2x2 DID design, a researcher observes two groups in both a pre- and post-treatment period, with one group never experiencing the treatment, and the other exposed to the treatment condition at the same time. However, in practice, as is true in the current study, many interventions feature different groups being treated at different times and for varied treatment periods and durations. Treated schools in the current study are an example of such an arrangement as MCAC was the first active program in the state and served eight schools. The following year MCAC nearly doubled in size to serve fifteen schools, while MSUCAC began by serving twenty-four schools. Even within programs the number of schools served changes from year to year making the traditional DID design impractical.

Thus, the traditional DID analysis, is slightly modified from equation 2 to equation 3:

Eq. 2: $Y_{it} = \alpha + \beta Treated + \beta PostPeriod + \delta_{DID} Treated_{it} \, X \, PostPeriod_{it} + \varepsilon_{it}$

Eq. 3: $Y_{it} = \alpha + \beta TreatActive_{it} + Y_i + \delta_t + \varepsilon_{it}$

In both equations Y represents the outcomes of interest (e.g. graduation rates, college-going rates, proportion of enrollees enrolled in a four-year institution, and the proportion of enrollees who earn twenty-four credits within twelve months) for school *i* in school year *t.* In equation 3, the traditional indicators for schools in the treated group (*Treated*) and the post-implementation period (*PostPeriod*) are substituted for unit (Y_i) and time (δ_t) fixed effects, and the interacted treatment and post treatment period is replaced by a single treatment indicator equal to 1 in years when a treated school is actively receiving the treatment, and 0 otherwise (Furquim et al., 2020). As Furquim et al. (2020) explain,

> "though the parametrization is different, the intuition remains the same: treatment effects are identified off of "switchers": those units that become treated during the observation period….[t]his means that some units will serve as comparison observations sometimes and treated units at later times, while other units will always serve as comparisons" (p. 30).

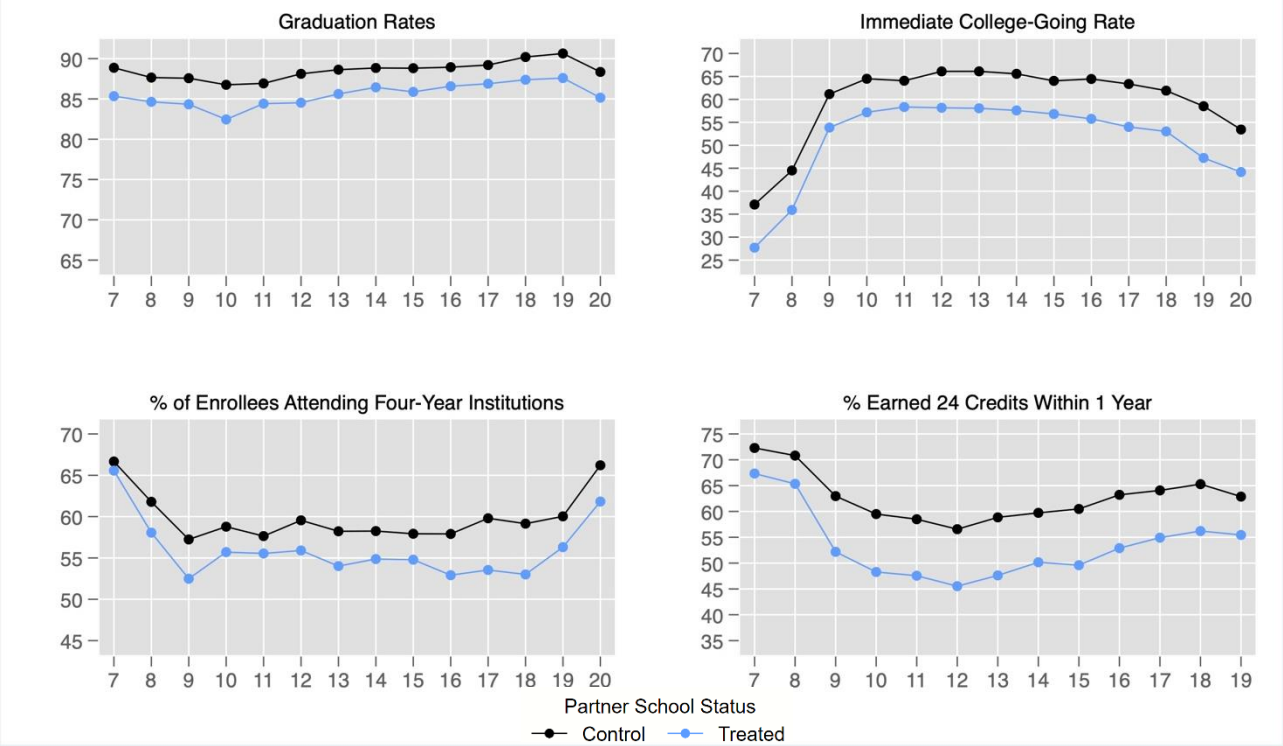
As there was no uniform implementation year nor treatment period, this modification appropriately replaces both the partner school and post-treatment indicators with the *Treated* indicator set equal to 1 in years where partner schools had near-peer college advisers, and 0 otherwise, allowing both the initial timing of treatment and treatment duration to vary by school over time.

In essence, the DID design allows causal inferences to be made, conditional upon a few assumptions. Most notably, DID assumes that treated and control units observed a parallel trend in the pre-treatment period. This is to say, that outcomes for treated and control units would have

remained on the same trajectory or similar trajectories as they were in the absence of being treated. While this assumption is formally untestable as there is no way to definitively know what outcomes would have been like for treated units without the treatment, researchers rely on a few strategies to examine pre-treatment trends. The first strategy is perhaps the simplest, conceptually, with a visual inspection of the data. **Figure 4** graphically presents trends of the four outcomes over time between schools that ever received the treatment, and pure controls (schools that were never treated).

Figure 4

Outcome Trends of the Overall Student Subgroup, by Treatment Status



Notes:
- Graphs show outcomes for the overall student population. X-axis values represent school year beginning, for example: 7 = school year 2007-08, 8 = school year 2008 - 09, etc. Data trends according to author's calculations.
- Partner Schools are schools that were served by an advising program in at least 1 school year between 2007-2020.

Source: Data from the Michigan Center for Educational Performance and Information (CEPI), https://www.mischooldata.org

A visual inspection of such trends shows that schools that ever-received treatment and those that did not largely maintained similar trends in average outcomes though the trend in graduation rates in the year preceding the establishment of MCAC shows a slightly sharper

decline for treated schools. I do not invoke the ‘parallel trend’ terminology when describing such trends using these figures. In addition to there being no consensus on formal tests capable of testing the parallel trends assumption, the time varying nature and differences in initial treatment year and duration of treatment, make these graphs difficult to interpret as they cannot distinguish when treatment ‘turns on’ for partner schools, compared to control schools which never receive the treatment (thus the lines represent partner schools’ status and not treatment status per se).

While many of the outcomes observe similar trajectories and patterns over time between the two groups, graduation rates at control schools observe a slightly sharper decline in the 2009-10 year, then do advising program partner schools. Additionally, the proportion of a schools graduates who enroll in a four-year school are fairly close between the two groups in the first year of the panel, and diverge slightly in the years preceding the introduction of the first advising program. Still, while these figures provide a bit of additional information and context, relying on these alone does not allow for a true visual cue as to whether outcomes changed trajectories at the time of treatment as schools received treatments in different school years.

Comparison Groups

To the latter point, as with most quasi-experimental designs, in order for the DID strategy to accurately estimate a causal effect, care must be taken to ensure that appropriate comparison groups are constructed in order to reduce effects of self-selection.

All Schools. For reference, in order to obtain baseline estimates among all schools in the sample the first comparison group I use are all schools that did not receive the treatment. However, as previously noted, because some schools in this group may be significantly different than treated schools, I adopted a ‘local and focal’ strategy (Furquim et al., 2020)

to develop two additional comparison groups that are more similar to treated schools and as such, more likely to have received the treatment.

Local Education Agency/Same District Schools ("LEA Schools"). The second comparison group is constituted of schools located in the same school districts as partner schools, as these proximally located schools may enroll students from similar backgrounds, neighborhoods, and in general reflect more similar characteristics to treated schools. Schools were assigned to this comparison group if they had the same NCES Local Education Area (LEA) identification number as a treated school.

Coarsened Exact Matched Schools ("CEM Schools"). The third group was created using Coarsened Exact Matching (CEM), which identified a comparable set of control schools similar to treated schools in terms of demographic composition and staffing levels in order to estimate models among schools most likely to receive the treatment in a given year. The use of CEM as a strategy in identifying comparison groups within a DID analytic strategy has also been employed in prior research studies of postsecondary institutions (Furquim & Glasener, 2017; Gándara & Li, 2020; Hillman et al., 2014; Spencer, 2019). The CEM technique is nonparametric matching method requiring units to match on all covariates included in the matching model, though it avoids the curse of dimensionality by coarsening variables in the matching procedure, binning them into smaller groups and allowing schools to 'match' so long as they fall within the coarsened group/category. Unmatched observations are pruned from the analysis, while matched observations are sorted into strata based on having similar baseline characteristics (Iacus et al., 2012).

Table 14

Summary Statistics for Treated and Comparison Groups

	Treated		All Controls		LEA		CEM	
	M	SD	M	SD	M	SD	M	SD
Panel A: Dependent Variables								
All Students								
- Graduation Rate	85.6	9.5	88.0	10.2	84.8	11.1	87.7	9.3
- Six Month College-Going Rate	52.3	12.8	58.0	17.7	51.5	16.9	56.1	16.1
- % Enrolled in a Four-Year	54.8	15.1	59.3	18.2	56.8	17.6	57.4	18.0
- % Earned 24 Credits in 1 Year	51.2	14.8	60.9	15.4	53.3	17.1	58.4	15.0
Black Students								
- Graduation Rate	81.3	11.1	82.9	13.0	79.7	13.4	82.4	12.1
- Six Month College-Going Rate	49.2	16.8	55.3	20.3	49.8	19.0	53.0	19.4
- % Enrolled in a Four-Year	51.5	18.0	56.4	20.4	54.4	19.1	53.5	19.6
- % Earned 24 Credits in 1 Year	37.0	10.9	45.7	16.9	39.7	14.7	41.2	14.1
White Students								
- Graduation Rate	86.5	9.9	89.0	9.8	86.3	10.8	88.7	8.9
- Six Month College-Going Rate	53.8	13.3	59.5	17.8	52.8	17.4	57.4	16.4
- % Enrolled in a Four-Year	56.6	17.3	60.2	18.8	58.5	18.8	58.4	18.8
- % Earned 24 Credits in 1 Year	58.2	12.2	64.4	13.3	60.3	14.5	62.1	13.1
Panel B: Independent Variables								
Student Enrollment	687	435	737	537	714	469	664	477
% Free/Reduced Lunch	59.4	17.1	43.8	21.6	55.5	18.2	48.9	17.9
% Black	27.0	35.0	14.4	27.0	30.4	37.7	13.9	27.0
% Hispanic	8.8	11.9	5.3	8.9	7.1	13.6	5.4	8.8
% White	59.1	35.7	75.0	28.6	58.5	38.1	75.6	28.5
FTE # of Counselors	1.9	1.4	1.9	1.6	1.9	1.6	1.7	1.5
FTE # of Teachers	34.8	21.3	37.3	25.6	35.9	22.2	33.8	22.5
Title-I Eligible School	0.7	0.5	0.7	0.5	0.8	0.4	0.7	0.5
Schoolwide Title-I Program[1]	0.3	0.5	0.2	0.4	0.4	0.5	0.2	0.4
Charter[2]	0.1	0.2	0.1	0.3	0.1	0.2	0.0	0.2
Magnet[2]	0.2	0.4	0.1	0.3	0.1	0.4	0.1	0.2
Locale: City[2]	0.3	0.4	0.2	0.4	0.3	0.5	0.1	0.3
Locale: Suburb[2]	0.2	0.4	0.3	0.5	0.2	0.4	0.2	0.4
Locale: Town[2]	0.1	0.3	0.1	0.3	0.1	0.3	0.2	0.4
Locale: Rural[2]	0.4	0.5	0.4	0.5	0.3	0.5	0.5	0.5
Total # Schools Ever Treated	180		180[3]		180		180	
Total # Schools Never Treated	0		532		37		440	
Total # of Schools	180		712		217		620	
Total # of Observations	827		8,064		2,693		5,704	

[1] This measure is derived using CCD Data and represents schools indicating full school participation in the Title-I program in a given year (variable stub name *STITLI)*. Though the CCD now includes data distinguishing what type of Title-I program a school is eligible for and participating in (e.g., eligible for schoolwide but providing only targeted assistance, etc. (variable stub name *TITLEISTAT*)) this data was not included in the CCD prior to 2009, after the start of the timeframe of this analysis.

[2] Variables not included in analytic models given their time invariant nature. They are displayed to provide additional context for comparing and contrasting treated and control schools.

[3] Advising program partner schools (schools that received the treatment in at least one year) are included in the 'control' group though they contribute to presented statistics only when they are not actively receiving the treatment.

NOTE: LEA = Local Education Agency/Same District Schools, CEM = Coarsened Exact Matched, M = Mean, SD = Standard Deviation.

SOURCE: School Level Files: School Years: 2007-08 through 2020-21, Common Core of Data (CCD), U.S. Department of Education, National Center for Education Statistics; Michigan Center for Educational Performance: Graduation Rates by Student Subgroup Files, College Enrollment by High School Report; College Advising Corps; Michigan College Access Network.

Panels A and B of **Table 14** presents summary statistics for outcome variables and select independent variables, respectively across the treated and comparison groups. As the DID regression framework allows for additional controls above and beyond the noted fixed effects, I include variables controlling for: a schools' Title-I eligibility, had a schoolwide Title-I program in operation, the number of full-time equivalent teachers and school counselors, school enrollment size, and the percentage of the school receiving free or reduced-price lunch. The summary statistics of independent variables presented in **Table 14** are helpful in identifying similarities and differences between control and treated schools. Though treated schools enroll 50 fewer students on average, the two groups employ roughly the same number of full-time equivalent school counselors (1.86 at treated schools and 1.85 at control schools), and similar counts of full-time equivalent teachers (34.8 and 37.3 respectively). Compared to control schools, half as many treated schools were charters (ten and five percent, respectively) slightly more were magnet schools (thirteen compared to 16 percent), and nearly three out of every ten were located in cities (compared to sixteen percent of control schools).

Additional Diagnostics and Robustness Checks

To test the validity of findings, several falsification tests and robustness checks were undertaken, which include testing for placebo effects using a false treatment period, in addition to false outcomes, and employing event study models to both serve as a check on the two-way fixed effects models and check for potential violations of parallel trends in the pre-treatment period (Angrist & Pischke, 2008; Cunningham, 2021; Lechner, 2011). I briefly discuss each in the subsections below.

Placebo Treatment Assignment

First, I used a placebo treatment assignment to examine whether estimated treatment effects were spurious. To do so, schools with near-peer advisers were coded as being treated two years before they actually received the adviser treatment. These models also omit all observations of treated units once they receive the actual treatment. Significant estimates yielded from these models would be concerning in that since advisers had not yet begun working at the school, the treatment effects may be due to other spurious pre-treatment confounding influences.

Placebo Outcomes

Beyond the fake treatment assignment, I extended placebo testing by conducting a series of falsification tests utilizing eighteen false outcomes, including: the schools mobility rate (transfer in/out) [34]; the number of students eleventh and twelfth grade students eligible for, as well as participating in in dual enrollment (DE); the expenditures high schools reported for covering tuition of DE students; the average number of DE credits completed by a high schools DE students; and finally the average composite ACT scores of students[35]. These outcomes were chosen as they represent measures which should not be impacted by the near-peer adviser treatment. Beyond mobility rates which measure the transfer in/out rate of schools and which are simplest to understand in that they are not directly tied/related to college-going outcomes, context is helpful in understanding why dual enrollment outcomes and ACT scores should not be affected by the near-peer adviser treatment.

Dual Enrollment Outcomes. First, while the college adviser may be able to encourage students to consider DE, their presence alone would not change the number of students

[34] Mobility Rate Outcome data were first reported by CEPI in 2009, thus the 2007 and 2008 years were excluded from the school mobility models.
[35] School mobility rate and ACT score outcomes include testing within-subgroup rates as well (e.g., “All Students”, “Black Students”, etc.).

eligible for DE participation. For those students who are eligible, beyond redirecting students to school or district staff who handle DE requests (as schools/districts cover tuition costs of DE students, etc.), it is unlikely advisers could provide additional assistance or substantive guidance, given constraints on their time and priorities in focusing on other steps of the application process. As such, since neither eligibility nor participation would be likely to significantly change with an adviser present, the costs to the school and number of credits completed by enrolled students should also not be related to the treatment.

ACT Composite Score Outcomes. In 2005, Michigan Governor Jennifer Granholm signed legislation requiring all 11th graders attending Michigan high schools take the ACT exam (replacing the Michigan Educational Assessment Program) each Spring (Braciszeski, 2005; Keller, 2005). The ACT requirement for eleventh graders was substituted in 2015 with the SAT. Though college advisers typically encourage students to sit for these exams, because all 11th graders have been required to take an exam since before any of the near-peer programs were implemented, advisers should not impact changes in these outcomes as they are unable to induce additional students to take the exam. Because the exam administered changed between the 2015-16 school year, I used the concordance tables[36] to convert the SAT scores to ACT scores for comparability with earlier years. Thus, while the college adviser treatment may be new to a school, an adviser would be unlikely to induce significant changes in composite average scores as the vast majority of students already should have been undergoing testing each year.

[36] The 2016-18 version of the score concordance tables were used to convert average scores (ACT & The College Board, 2018).

Event Study Models

Finally, in recent years researchers have begun re-examining the appropriateness of using two-way fixed effects modeling strategies to deal with DID designs. Specifically, recent methodological developments in DID analyses suggest that this modeling strategy can be biased in analyses of interventions with staggered treatment timing and durations[37]. The broad argument contends that the modeling strategy is problematic, in that it assumes there is a single average treatment effect over the entirety of the post-treatment period, and additionally the models utilize already treated schools as controls as schools treated later in the panel receive the treatment. This ultimately erodes the estimated treatment effects as units treated at later points in the panel utilize already treated schools (and their treatment effect coefficients) as controls . While there is an active debate as to the best approaches to deal with such challenges (Callaway & Sant'Anna, 2021; Cunningham, 2021; Goodman-Bacon, 2019, 2021; Huntington-Klein, 2022; Sun & Abraham, 2021) one method researchers have employed to both complement their DID analyses and conduct additional robustness checks of their two-way fixed effects estimates has been to use event study (ES) models.

The event study model substitutes the treatment term of the DID equation with a set of indicators leading up to and after program implementation. Technically, this is achieved by coding all treated schools with 0 for the first-year a school had a near peer adviser and thereafter each year before and after is consecutively coded (e.g., three years prior = -3, two years prior = -2, three years after = 3, etc.). In addition to allowing for an examination of the extent to which

[37] Goodman-Bacon's (2019) decomposition of the two-way fixed effects model demonstrated the TWFE model may be biased in circumstances when treatment timing and length varies between units as the weight of estimated treatment effects each unit contributes depends upon its place in the panel (e.g., a unit being treated in the early period while another is treated in the later period). Currently, the *bacondecomp* Stata routine (Goodman-Bacon et al., 2019) to identify the weights each comparison (treated v. never treated, or early treated v. late treated) contributes to the average treatment effect is only available for strongly balanced panel datasets where each observation is observed at all time-periods in the panel.

treatment effects may vary during the post-treatment period, the ES models also have added benefit of providing a check of whether the assumption of parallel trends is violated in the observed in pre-treatment periods (Furquim et al., 2020; Gándara & Li, 2020; Hu et al., 2022; Zhu et al., 2021), which would be identified through estimates of significance in the years preceding program implementation. Any significant effects observed in the years preceding program implementation, would be an indication that parallel trends were not observed between treated and control schools in the pre-treatment period, thus calling into question any estimates of significance identified in the TWFE models. Still, it should be noted that while these models may help identify violations of parallel trends, null findings of parallel trends are not necessarily indicative that none exist. I relied on the Stata *eventdd* routine (Clarke & Tapia-Schythe, 2021) in order to estimate the event study models.

The event study models focus on a seven-year window: three years before ($t = -3$, $t = -2$, $t = -1$*[serves as the reference year])*, the program implementation year ($t=0$), and three years after implementation ($t = 1$, $t = 2$, $t = 3$). The constricted window is used for primarily two reasons: first, researchers have noted that event study models typically focus on shorter treatment windows (or *short horizons*) as the further out one gets from the initial treatment period, the more likely time will begin confounding estimates (even after controlling for it) (Huntington-Klein, 2022).. Thus, identification is threatened as the variation from the treatment effect begins to become more confounded by other factors. Second, from a practical standpoint the panel dataset in this analysis begins in the 2007-08 and the first treated schools were treated by MCAC during the 2010-11 year, thus the three-year window would capture even schools during the earliest treatment period. As most advisers typically serve on a two-year service cycle, in most

instances this window would represent only two near-peer advisers, and not several out after a school has been served for an extended period.

Results

In this section I present results first from the TWFE OLS models, then those from placebo testing, and conclude with a review of estimates from the event studies. The TWFE model results of all outcomes for the full panel are presented in **Table 15**. To assess whether effects are greater shortly in the first few years of program implementation **Table 16** presents the same outcomes but constrains the analytic window to three years pre and post implementation for schools that receive the treatment. Each coefficient presented represents the estimated average treatment effect of the near-peer adviser obtained using TWFE OLS models. Given evidence of autocorrelation using a series of Wooldridge (2002) tests, all models employed robust standard errors.

Two-Way Fixed Effect Results

Starting with **Table 15**, across all outcomes for all subgroups, the TWFE model results suggest that with a single exception, schools that received the near-peer adviser treatment did not significantly differ in their outcomes compared to schools that did not receive the treatment. The only case where the treatment appears to have impacted a school level outcome is observed as a 1.7 percent increase ($p < .05$) in the six-month college-going rate (Panel B) of female students compared to untreated schools located in same district schools. Constricting the models to the three year pre and post-treatment window, the same effect for this subgroup and school sample is observed (**Table 16**, panel B), though the effect is slightly larger (1.9 percent increase). The only other effect detected within the constrained three-year window suggests programs increased college-going rates of Black students overall by roughly 2.4 percent ($p < .05$) compared to rates

at similar (untreated) schools in the CEM sample. For each of the three effects noted here, no placebo treatment effects were detected when assigning treated schools as "treated" two-years before actual treatment began.

Falsification Tests Using Placebo Outcomes

Table 17 presents results of a series of falsification tests conducted using false outcomes. Models were tested using all three comparison groups, and for all models except two, the treatment had no effect on the false outcome. These models detected two placebo effects which suggest that in the advising program treatment resulted in a slight decrease in ACT scores of Black students, overall (-0.15 points ; $p < .05$), and a 1.3 percent decrease in the mobility rate of White students. Importantly, these effects while slightly concerning, are only detected in the full sample and not in same district or CEM schools[38].

Event Studies

Results from the main event study models are presented in **Table 18**, **Table *19***, and **Table 20** for all, Black, and White students, respectively. These models include all the controls included in the TWFE models, except for the treatment indicator, which is substituted for a fully saturated set of lags and leads representing years until and since program implementation. The year right before a program is implemented ($t = -1$) is used as the reference year and as such is omitted from the models. Though the tables are presented separately, I review results across subgroups by outcome.

[38] For these two detected placebo effects, I estimated two event study models. While the model for average ACT scores of Black students found no effect, the model for mobility rates of White students detected a significant and slightly larger impact during the second year of advising program implementation (- 2.3 percent). The effects may identify spurious impacts though it may also be due to data on this outcome being unavailable for the 2007-08 and 2008-09 years, thus shortening the pre-treatment period.

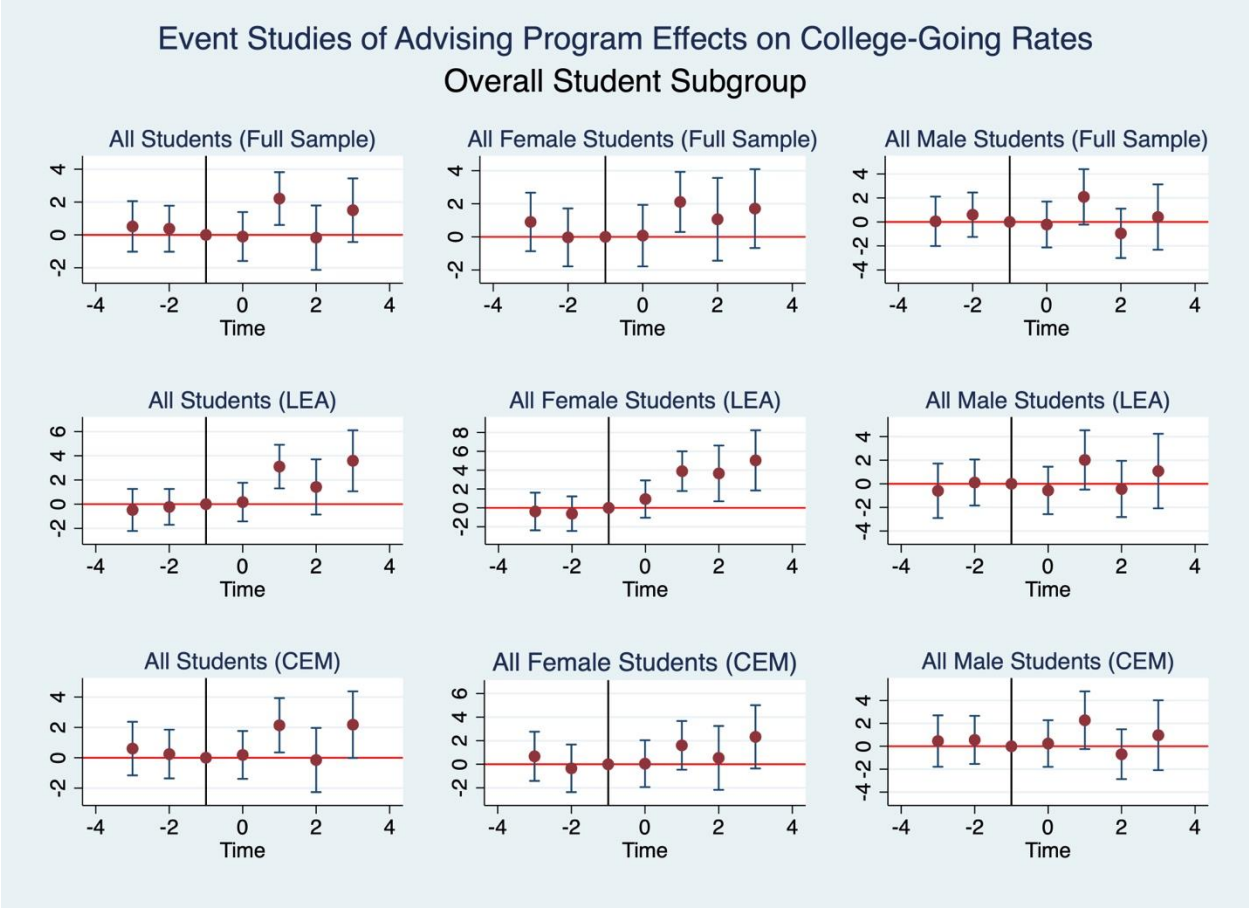
Graduation Rates

Similar to the main results, with the exception of a few estimates the event study results suggest largely no significant effects of advising programs on student graduation rates (Panel A of **Table 18**, **Table *19***, and **Table 20**). Notably, the models detect several pre-treatment trends for White students indicating violations of parallel trends in rates of students overall and for male students (across all three comparison groups), and White female students attending schools in LEA sample. The ES models do suggest program effects for White female student graduation rates during the first year of a program in both the full sample and the CEM sample of roughly 1.8-1.9 percent. The models do, however, find a roughly 2.9 percent decrease in graduation rates of White students overall in the LEA sample, during a programs fourth year ($t = 3$). These decreases occur concurrent with continued declines of greater magnitude in graduation rates of White male students, thus to the extent that the rates for these students are driving the overall rates, they are likely biased due to the pre-treatment trends already present for these male students. The only other effect detected by the models suggest a 2.5 percent decrease in graduation rates of Black Female students at LEA schools during the initial year of the program. This is the only group for which no pre-treatment violations of parallel trends are detected.

Six-Month College-Going Rates

Perhaps most appropriately, the ES models for immediate college-going rates are the only outcomes across all subgroups and school samples that do not detect any pre-treatment trends. Moreover, unlike the TWFE models, the event studies detect several significant effects on college-going rates (Panel B of **Table 18**, **Table *19***, and **Table 20**) across subgroups and samples. Because of the number of significant effects detected I discuss each subgroup separately below.

Figure 5
Event Studies of Advising Program Effects on College-Going Rates Overall

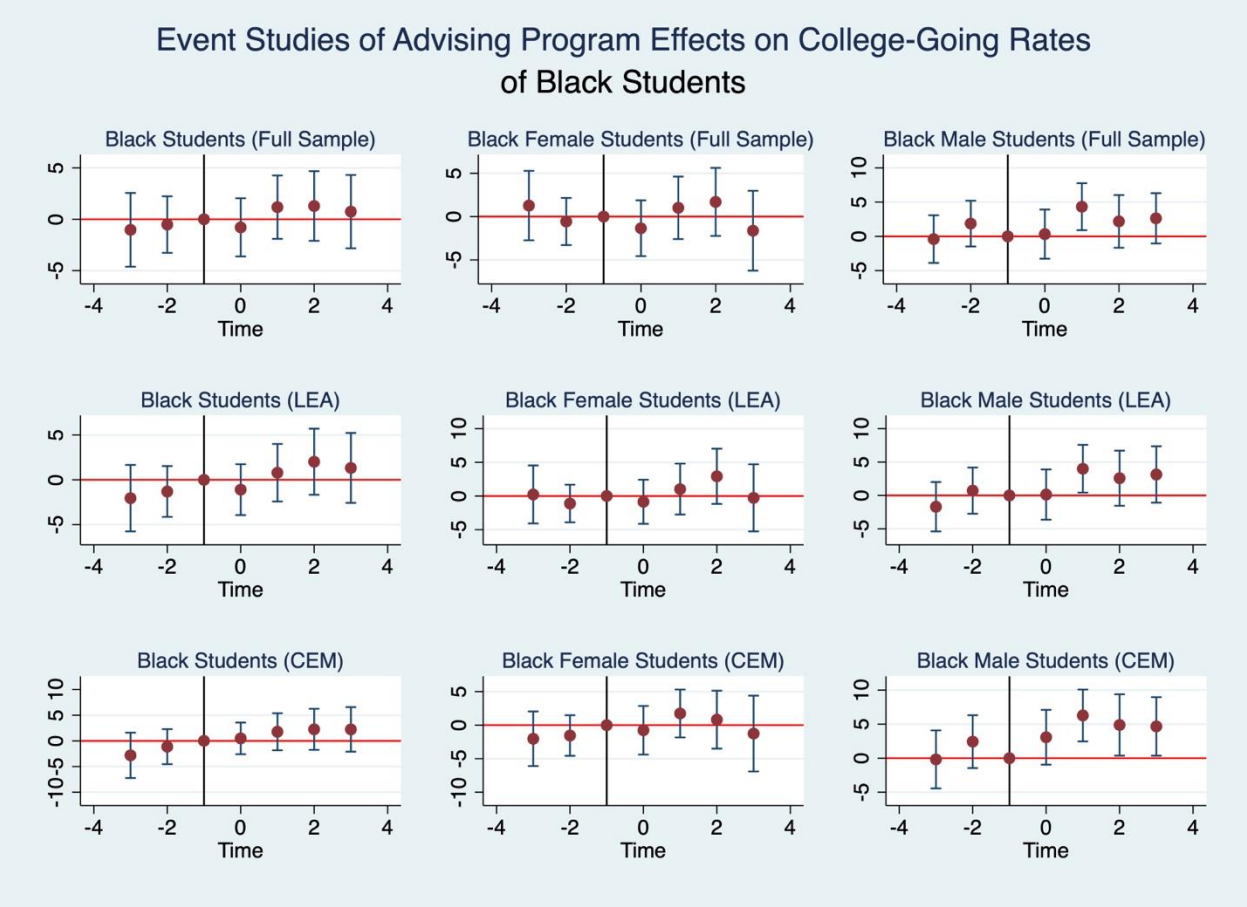


All Students. Starting with the overall student group, results suggest that the college-going rate for all students increases significantly during the program's second year (*t* = *1*), by around 2.1 to 2.2 in the full sample of schools (p < .01) and in the CEM sample (p < .05), and by 3.9 percent in the LEA sample (p < .001). For the LEA sample, a significant increase of 3.6 percent is also detected in the fourth year of the program (p < .01). For overall female rates, the college-going rate is estimated to increase by 2.1 percent in the programs second year in the full sample, and for the full post-treatment period in the LEA sample, specifically: by 3.9 percent in year two (p < .001), 3.7 percent in year three (p < .05), and 5.0 percent in year four (p < .01). These effects are the

strongest observed for female rates across samples and comport with estimates obtained in the DID model which detected significant treatment effects (1.7 and 1.9 percent in the full panel and constrained to a three-year window, both p <.05) for this group within the same sample though the event study effects are more than twice as large (and nearly three times in the case of the coefficient on the fourth year).

Figure 6

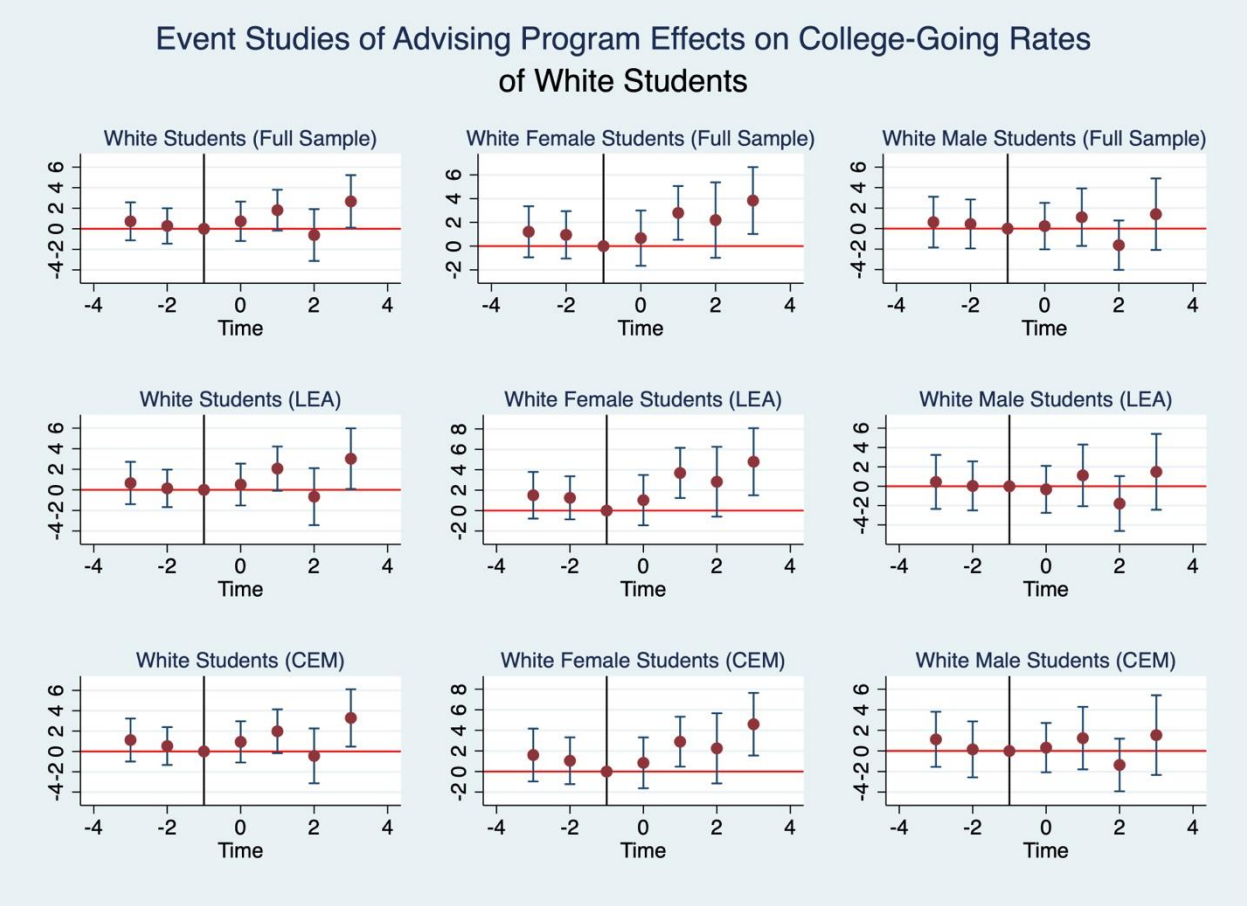
Event Studies of Advising Program Effects on College-Going Rates of Black Students



Black or African American Students. The only subgroup for which effects on college-going rates are detected in the Black student population are for Black male students. The year after program implementation, the college-going rate for this group increases across all three samples: 4.3 percent in the full school sample ($p < .05$), 4.0 percent in the LEA

sample ($p < .05$), and 6.3 percent in the CEM sample ($p < .01$). The effects in the CEM sample for this group are sustained with significant increases following in year three and year four (increasing 4.9 and 4.7 percent respectively, both $p < .05$).

Figure 7
Event Studies of Advising Program Effects on College-Going Rates of White Students



White Students. The college-going rates of White students increase in a pattern across all three samples, for the group overall, and for White females specifically. In the year following the program start, college-going rates of White female students increase across samples, by 2.8 to 2.9 percent in the full and CEM samples (both $p < .05$), and by 3.7 percent in the LEA sample ($p < .01$). In the fourth year of the program, the rates for this group increases significantly again, with increases that are significantly stronger: 3.8

percent in the full sample, and roughly 4.6 to 4.8 percent in the LEA and CEM samples (all significant at the one percent level). The fourth year also picks up a significant effect for the college-going rates for White students overall (2.6, 3.0, and 3.3 percent in the full, LEA, and CEM samples, all $p < .05$).

Overall, across samples and subgroups, increases in college-going rates were most often observed during the second and fourth years of the program. Such patterns may be indicative of the experience advisers accrue during their first year of service working with students that may then serve to benefit the cohort they work with in their second and final year of service. The most consistent effects observed were for rates for female students in the LEA sample, and of rates of Black male students in the CEM sample, both of which consistently increased in each period after program implementation, and the latter of which experienced large though over time diminishing increases in college-going rates.

Proportion of Enrollees who Enrolled in a Four-Year Institution

Similar to graduation rates, several pre-treatment effects are detected within rates for the White student population – specifically, White female students. Despite this, the models indicate that the proportion of female students overall who enrolled in a four-year increased by 2.9 percentage points during the program implementation year (Event = 0) compared to CEM control schools.

The remaining effects detected occur during the third year of the program (*t=2*), with significant decreases in the proportion of enrollees attending four-year schools. Three significant estimates are detected in the full sample for the overall male student subgroup (-3.73), Black students overall (-5.85) and Black Female student rates in the full sample (-5.80). The final estimate also concerns the latter group, as models detect a seven-percentage point drop in the

proportion of Black Female students enrolling in four-year institutions compared to CEM control schools ($p < .05$).

Given the prior noted placebo effect detected for average composite scores of Black students overall, it could be argued declining scores impacted decreases in these overall rates more so than the advising program itself. While this is plausible, the detected decrease in average overall scores of Black students was quite small (-0.14). Moreover, this effect was only detected for schools in the full sample – thus while the drops noted in the full sample may be in part explained due to such a decrease in scores the drop noted in the CEM sample may be indicative that the detected decrease may be driven more by the near-peer adviser. As no effects have been detected for rates of Black Female college-going between either the DID and ES models, such proportional shifts are more likely to be driven by inducing more Black Female students to enroll in two-year institutions rather than in four-years, as opposed to increase Black Female enrollment rates overall.

Proportion of Enrollees Earning 24 Credits Within 12 Months of Enrolling

The only subgroup for which an effect is detected on rates of credit attainment for the fourth outcome, are for Black male students. Models for the LEA sample detect a 6.4 percentage point decrease in the proportion of enrollees who earned twenty-four credits in their first year of college, during the second year of the program, while a nearly 8 percent decrease is detected in year four, among the full school sample. The decreases detected present an interesting situation. If the estimates obtained by earlier ES models are reliable, the decreases observed here may be attributable to more Black male students having enrolled in college in the first place, though such drops in rates of persistence (or momentum in credit acquisition) may also be indicative of a poor match and fit between students and the institutions where they enroll.

Table 15

Effect of Near-Peer Advising Programs on School Outcomes, by Student Subgroup

	All Schools				LEA Schools				CEM Schools			
	β	SE	R^2	N	β	SE	R^2	N	β	SE	R^2	N
Panel A: Graduation Rates												
All Students	0.23	(0.46)	.041	8,881	0.25	(0.52)	.048	2,686	0.12	(0.46)	.051	5,704
- Black	0.57	(0.90)	.086	3,168	1.07	(0.90)	.085	1,339	0.43	(0.88)	.104	1,875
- White	-0.42	(0.48)	.032	8,019	-0.13	(0.53)	.038	2,132	-0.38	(0.50)	.036	5,169
Female Students	0.46	(0.41)	.025	8,555	0.31	(0.48)	.034	2,634	0.33	(0.43)	.034	5,471
- Black	-0.62	(0.75)	.051	2,470	-0.45	(0.82)	.047	1,197	-1.02	(0.83)	.065	1,493
- White	0.26	(0.41)	.020	7,475	0.42	(0.51)	.025	1,996	0.26	(0.43)	.028	4,797
Male Students	0.07	(0.57)	.041	8,555	0.21	(0.62)	.048	2,641	-0.06	(0.58)	.051	5,505
- Black	1.03	(1.14)	.078	2,442	1.20	(1.21)	.099	1,192	1.43	(1.22)	.113	1,507
- White	-1.03	(0.59)	.037	7,490	-0.48	(0.62)	.039	2,025	-1.12	(0.64)	.041	4,845
Panel B: Six-Month College-Going Rates												
All Students	0.00	(0.63)	.556	8,881	0.89	(0.63)	.582	2,686	0.07	(0.68)	.500	5,704
- Black	0.00	(0.92)	.428	3,245	0.53	(0.96)	.496	1,355	1.05	(1.02)	.363	1,927
- White	0.20	(0.72)	.539	8,019	0.62	(0.71)	.553	2,134	0.43	(0.76)	.479	5,168
Female Students	0.68	(0.74)	.528	8,552	1.72*	(0.77)	.539	2,634	0.42	(0.80)	.475	5,469
- Black	-0.42	(1.18)	.386	2,592	0.76	(1.22)	.413	1,228	0.33	(1.14)	.365	1,555
- White	0.87	(0.86)	.530	7,477	1.26	(0.86)	.551	2,003	0.97	(0.89)	.467	4,798
Male Students	-0.66	(0.62)	.433	8,548	0.09	(0.63)	.451	2,641	-0.38	(0.68)	.376	5,501
- Black	-0.63	(1.04)	.386	2,535	-0.03	(1.16)	.400	1,217	0.60	(1.20)	.331	1,564
- White	-0.45	(0.74)	.415	7,489	-0.10	(0.75)	.405	2,027	-0.19	(0.80)	.357	4,844
Panel C: % of Enrollees Who Enrolled in a Four-Year												
All Students	-0.78	(0.81)	.067	8,874	0.14	(0.78)	.091	2,683	0.27	(0.86)	.070	5,697
- Black	-2.64	(1.44)	.075	3,224	-0.68	(1.41)	.114	1,349	-1.60	(1.52)	.082	1,908

Table 15

Effect of Near-Peer Advising Programs on School Outcomes, by Student Subgroup

	All Schools				LEA Schools				CEM Schools			
	β	SE	R^2	N	*β*	SE	R^2	N	*β*	SE	R^2	N
- White	-1.16	(1.03)	.052	8,007	-0.12	(0.93)	.070	2,129	-0.43	(1.05)	.061	5,158
Female Students	-0.75	(0.94)	.057	8,545	0.33	(0.96)	.075	2,631	0.21	(0.98)	.060	5,463
- Black	-0.12	(1.43)	.063	2,577	0.34	(1.60)	.105	1,223	0.50	(1.67)	.073	1,544
- White	-1.59	(1.12)	.047	7,472	-0.08	(1.11)	.056	2,001	-0.89	(1.14)	.052	4,794
Male Students	-0.84	(0.91)	.038	8,539	-0.22	(0.95)	.061	2,636	0.27	(1.01)	.040	5,494
- Black	-2.85	(1.63)	.049	2,520	-0.66	(1.56)	.071	1,212	-1.41	(1.93)	.063	1,553
- White	0.14	(1.14)	.034	7,478	0.68	(1.21)	.061	2,023	0.82	(1.23)	.038	4,834
Panel D: % of Enrollees Who Earned 24 Credits Within 1 Year												
All Students	-0.42	(0.63)	.281	7,012	0.45	(0.72)	.372	2,176	0.31	(0.71)	.279	4,462
- Black	0.39	(1.00)	.290	1,320	0.69	(1.02)	.364	710	0.97	(1.17)	.298	778
- White	0.21	(0.77)	.260	6,198	0.67	(0.89)	.359	1,643	0.58	(0.84)	.270	3,914
Female Students	-0.29	(0.69)	.244	6,047	0.44	(0.80)	.337	1,836	0.32	(0.78)	.233	3,749
- Black	-0.51	(1.12)	.246	844	-0.68	(1.24)	.303	496	0.13	(1.49)	.227	510
- White	0.34	(0.87)	.229	5,245	0.89	(1.04)	.333	1,334	0.27	(0.95)	.231	3,200
Male Students	-0.16	(0.81)	.263	5,154	0.70	(0.93)	.332	1,437	0.30	(0.84)	.240	3,111
- Black	0.13	(1.53)	.328	429	-0.40	(1.53)	.429	258	0.65	(1.77)	.350	270
- White	0.72	(1.07)	.241	4,478	1.18	(1.34)	.285	1,034	1.48	(1.08)	.226	2,633

* p < .05, ** p < .01, *** p < .001

NOTE: Coefficients represent two-way fixed effect OLS estimates of program treatment on the respective outcome. Robust standard errors in parentheses. All models include unit and school year fixed effects, and control for: Title-I Eligibility, if the school has an active Schoolwide Title-I Program, the percentage of students on free and reduced-price lunch (and a logged version), full-time equivalent number of school counselors, full-time equivalent number of school-teachers, and the number of students enrolled at the school. LEA = Same District/Local Education Agency; CEM = Coarsened Exact Matched.

SOURCE: School Level Files: School Years: 2007-08 through 2020-21, Common Core of Data (CCD), U.S. Department of Education, National Center for Education Statistics; Michigan Center for Educational Performance: Graduation Rates by Student Subgroup Files, College Enrollment by High School Report; College Advising Corps; Michigan College Access Network

Table 16

Effect of Near-Peer Advising Programs on School Outcomes, by Student Subgroup (Three-Year Window)

	All Schools				LEA Schools				CEM Schools			
	β	SE	R^2	N	β	SE	R^2	N	β	SE	R^2	N
Panel A: Graduation Rates												
All Students	0.12	(0.46)	.040	7,647	0.17	(0.54)	.046	1,452	0.03	(0.48)	.047	4,716
- Black	0.44	(0.86)	.085	2,646	0.86	(0.87)	.082	817	0.33	(0.95)	.104	1,486
- White	-0.44	(0.52)	.031	6,957	-0.27	(0.63)	.031	1,070	-0.46	(0.55)	.034	4,290
Female Students	0.21	(0.44)	.023	7,342	0.15	(0.53)	.040	1,421	0.12	(0.48)	.032	4,503
- Black	-0.78	(0.82)	.052	2,013	-0.62	(0.88)	.055	740	-1.13	(0.94)	.075	1,153
- White	0.56	(0.45)	.021	6,472	0.88	(0.63)	.045	993	0.63	(0.49)	.032	3,965
Male Students	0.16	(0.57)	.041	7,341	0.15	(0.65)	.042	1,427	0.06	(0.60)	.049	4,535
- Black	0.67	(1.15)	.070	1,982	0.85	(1.16)	.097	732	1.08	(1.31)	.115	1,162
- White	-0.92	(0.64)	.037	6,475	-0.77	(0.78)	.028	1,010	-0.98	(0.68)	.042	4,001
Panel B: Six-Month College-Going Rates												
All Students	0.31	(0.59)	.532	7,647	0.97	(0.68)	.434	1,452	0.38	(0.63)	.469	4,716
- Black	1.00	(1.00)	.387	2,715	0.95	(1.04)	.375	825	2.38*	(1.15)	.307	1,530
- White	0.36	(0.72)	.519	6,954	0.16	(0.77)	.408	1,069	0.43	(0.76)	.450	4,287
Female Students	0.60	(0.73)	.501	7,339	1.93*	(0.89)	.354	1,421	0.44	(0.78)	.439	4,501
- Black	-0.27	(1.33)	.360	2,113	0.12	(1.47)	.341	749	0.95	(1.29)	.344	1,198
- White	1.17	(0.91)	.504	6,468	0.94	(1.03)	.365	994	1.12	(0.95)	.430	3,962
Male Students	-0.14	(0.65)	.415	7,333	-0.17	(0.76)	.346	1,426	-0.03	(0.72)	.352	4,530
- Black	1.31	(1.14)	.375	2,053	1.58	(1.27)	.361	735	2.61	(1.36)	.317	1,199
- White	-0.62	(0.76)	.406	6,470	-0.96	(0.86)	.304	1,008	-0.55	(0.83)	.343	3,996
Panel C: % of Enrollees Who Enrolled in a Four-Year												
All Students	-0.61	(0.73)	.059	7,640	-0.43	(0.98)	.068	1,449	-0.08	(0.85)	.068	4,709

Table 16

Effect of Near-Peer Advising Programs on School Outcomes, by Student Subgroup (Three-Year Window)

	All Schools				LEA Schools				CEM Schools			
	β	SE	R^2	N	*β*	SE	R^2	N	*β*	SE	R^2	N
- Black	-1.52	(1.40)	.063	2,696	0.04	(1.70)	.082	821	-1.68	(1.68)	.071	1,513
- White	-1.31	(0.99)	.049	6,943	-1.53	(1.07)	.071	1,065	-0.74	(1.03)	.063	4,278
Female Students	-0.43	(0.91)	.052	7,332	-0.20	(1.14)	.068	1,418	0.09	(1.03)	.064	4,495
- Black	0.21	(1.67)	.047	2,099	0.43	(1.97)	.074	745	0.20	(2.15)	.060	1,188
- White	-1.15	(1.04)	.047	6,463	-0.78	(1.25)	.077	992	-0.51	(1.11)	.058	3,958
Male Students	-0.69	(0.98)	.029	7,325	-0.34	(1.22)	.033	1,422	-0.05	(1.11)	.032	4,523
- Black	-0.20	(1.76)	.042	2,041	2.15	(1.89)	.078	733	1.25	(2.15)	.049	1,190
- White	-0.61	(1.23)	.027	6,460	-0.89	(1.50)	.037	1,005	-0.31	(1.37)	.031	3,987
Panel D: % of Enrollees Who Earned 24 Credits Within 1 Year												
All Students	0.45	(0.71)	.242	6,009	1.00	(0.84)	.274	1,173	0.94	(0.78)	.236	3,671
- Black	0.43	(1.09)	.226	1,066	1.16	(1.23)	.286	456	1.26	(1.22)	.240	593
- White	1.02	(0.87)	.228	5,386	1.00	(1.07)	.283	831	1.24	(0.93)	.231	3,253
Female Students	0.14	(0.80)	.209	5,223	0.52	(0.98)	.253	1,012	0.70	(0.88)	.186	3,102
- Black	-0.82	(1.44)	.217	669	-0.20	(1.70)	.289	321	0.76	(1.75)	.214	386
- White	0.88	(1.00)	.200	4,598	0.56	(1.24)	.270	687	0.81	(1.08)	.188	2,679
Male Students	0.63	(0.97)	.235	4,496	1.56	(1.01)	.240	779	1.10	(0.97)	.206	2,594
- Black	-0.79	(1.81)	.245	341	-0.68	(1.84)	.307	170	-0.28	(2.15)	.285	206
- White	0.49	(1.23)	.227	3,968	1.71	(1.50)	.234	524	1.16	(1.22)	.207	2,228

* p < .05, ** p < .01, *** p < .001

NOTE: Coefficients represent two-way fixed effect OLS estimates of program treatment on the respective outcome, with the window of analysis restricted to three years prior to and post implementation for treated schools. Robust standard errors in parentheses. The same unit and school year fixed effects, and control variables are included, similar to those presented in Table 15. LEA = Same District/Local Education Agency; CEM = Coarsened Exact Matched.

SOURCE: School Level Files: School Years: 2007-08 through 2020-21, Common Core of Data (CCD), U.S. Department of Education, National Center for Education Statistics; Michigan Center for Educational Performance: Graduation Rates by Student Subgroup Files, College Enrollment by High School Report; College Advising Corps; Michigan College Access Network

Table 17
Falsification Tests Using Placebo Outcomes

	All Schools				LEA Schools				CEM Schools			
	β	SE	R^2	N	*β*	SE	R^2	N	*β*	SE	R^2	N
DE Eligible 11th Graders[1]	0.28	3.24	.161	8,069	-3.86	3.46	.192	2,419	2.07	3.59	.153	5,168
DE Enrolled 11th Graders[1]	-0.95	1.38	.044	8,069	0.69	1.32	.020	2,419	-0.37	1.51	.035	5,168
DE Eligible 12th Graders[1]	4.33	3.54	.137	8,069	-1.21	3.58	.158	2,419	3.77	4.02	.134	5,168
DE Enrolled 12th Graders[1]	-1.80	1.92	.055	8,069	0.41	1.97	.037	2,419	-0.92	2.12	.049	5,168
HS DE Expenditures[1]	44.45	27.89	.015	8,069	61.95	37.39	.037	2,419	36.82	20.18	.021	5,168
# of DE Credits Completed[1]	0.81	10.13	.113	8,069	13.44	8.82	.154	2,419	5.18	11.01	.157	5,168
Mobility Rate - All[2]	-0.37	0.33	.383	7,623	-0.03	0.33	.465	2,291	-0.22	0.35	.402	5,071
Mobility Rate - Black[2]	0.50	1.04	.094	7,037	1.54	1.09	.126	2,170	0.02	1.15	.091	4,617
Mobility Rate - White[2]	-1.29*	0.53	.225	7,510	-0.88	0.66	.185	2,219	-0.66	0.55	.243	5,012
Avg ACT Score - All	-0.10	0.05	.320	8,135	-0.03	0.06	.363	2,443	-0.03	0.06	.345	5,198
Avg ACT Score - Female	-0.08	0.05	.280	7,790	-0.02	0.06	.321	2,383	-0.03	0.06	.293	4,957
Avg ACT Score - Male	-0.09	0.06	.248	7,763	-0.01	0.07	.283	2,389	0.00	0.07	.271	4,974
Avg ACT Score - Black	-0.14*	0.07	.221	2,835	-0.02	0.08	.318	1,189	-0.02	0.08	.252	1,673
Avg ACT Score - White	-0.12	0.06	.300	7,333	-0.09	0.07	.312	1,940	-0.07	0.07	.315	4,692

* p < .05, ** p < .01, *** p < .001

[1] Schools that did not offer DE (dual enrollment) are excluded from models where outcomes relate to DE.

[2] Data for Mobility Rate outcomes is limited to 2009-2020.

[3] ACT/SAT concordance tables were used to convert school average SAT scores for 2007-2015 years into ACT scores. ACT score results are reported based on the school where a student was tested. As a result, it is possible for a student attending a treated school to take the exam at a control school and have their score reported their (or vice versa), thus scores may not be entirely representative of the school test-taking population.

NOTE: Coefficients represent two-way fixed effect OLS estimates of treatment effect near-peer advising programs on respective placebo outcomes. Sample sizes fluctuate due to data limitations. Robust standard errors in parentheses. All models include unit and school year fixed effects, and control for: Title-I Eligibility, if the school has an active Schoolwide Title-I Program, the percentage of students on free and reduced-price lunch (and a logged version), full-time equivalent counts of school counselors and school teachers, and the number of students enrolled at the school. LEA = Same District/Local Education Agency. CEM = Coarsened Exact Matched. DE = Dual Enrollment.

Table 18

Event Studies of Advising Program Effects Across Outcomes of All Students

	All Schools			LEA Schools			CEM Schools		
	A	F	M	A	F	M	A	F	M
Panel A: Graduation Rates									
T = -3	-0.57	0.52	-1.49	-0.81	0.46	-1.77*	-1.24	-0.19	-2.18*
	(0.61)	(0.60)	(0.79)	(0.67)	(0.65)	(0.85)	(0.72)	(0.70)	(0.92)
T = -2	-0.56	0.50	-1.63*	-0.68	0.46	-1.78*	-0.75	0.29	-1.98**
	(0.53)	(0.50)	(0.72)	(0.56)	(0.52)	(0.74)	(0.54)	(0.54)	(0.76)
T = 0	-0.10	0.56	-0.62	-0.02	0.71	-0.74	-0.32	0.29	-0.85
	(0.47)	(0.54)	(0.65)	(0.50)	(0.57)	(0.69)	(0.50)	(0.57)	(0.71)
T = 1	-0.05	0.91	-1.02	-0.04	1.00	-1.27	-0.39	0.39	-1.25
	(0.54)	(0.60)	(0.69)	(0.58)	(0.63)	(0.74)	(0.59)	(0.62)	(0.77)
T = 2	-0.18	0.46	-0.69	-0.15	0.57	-0.92	-0.56	0.20	-1.29
	(0.75)	(0.76)	(0.90)	(0.86)	(0.85)	(1.04)	(0.80)	(0.77)	(0.99)
T = 3	-0.82	0.27	-1.53	-0.82	0.37	-1.87	-0.87	0.13	-1.61
	(0.90)	(0.90)	(1.03)	(1.03)	(1.01)	(1.19)	(0.92)	(0.91)	(1.09)
R^2	0.04	0.03	0.04	0.06	0.04	0.06	0.06	0.04	0.06
N	8,881	8,555	8,555	2,686	2,634	2,641	5,704	5,471	5,505
Panel B: Six-Month College-Going Rates									
T = -3	0.52	0.91	0.04	-0.48	-0.36	-0.61	0.61	0.69	0.43
	(0.78)	(0.90)	(1.05)	(0.88)	(1.01)	(1.17)	(0.90)	(1.06)	(1.14)
T = -2	0.38	-0.02	0.60	-0.23	-0.61	0.11	0.24	-0.34	0.56
	(0.71)	(0.89)	(0.94)	(0.75)	(0.93)	(0.99)	(0.82)	(1.03)	(1.07)
T = 0	-0.10	0.08	-0.22	0.17	0.97	-0.56	0.18	0.07	0.23
	(0.76)	(0.95)	(0.97)	(0.81)	(1.01)	(1.02)	(0.80)	(1.01)	(1.04)
T = 1	2.21**	2.11*	2.08	3.11***	3.93***	2.02	2.14*	1.61	2.27
	(0.82)	(0.93)	(1.18)	(0.91)	(1.07)	(1.28)	(0.91)	(1.05)	(1.28)
T = 2	-0.17	1.05	-0.96	1.42	3.68*	-0.45	-0.15	0.52	-0.70
	(1.00)	(1.28)	(1.05)	(1.16)	(1.50)	(1.21)	(1.08)	(1.38)	(1.11)
T = 3	1.50	1.70	0.39	3.59**	5.08**	1.05	2.18	2.31	0.96
	(0.99)	(1.22)	(1.39)	(1.28)	(1.62)	(1.61)	(1.12)	(1.37)	(1.55)
R^2	0.56	0.53	0.44	0.59	0.55	0.46	0.50	0.48	0.38
N	8,881	8,552	8,548	2,686	2,634	2,641	5,704	5,469	5,501
Panel C: % of Enrollees Who Enrolled in a Four-Year									
Event = -3	0.01	1.91	-2.22	-0.45	2.06	-3.39	0.07	1.84	-2.73
	(1.25)	(1.36)	(1.78)	(1.45)	(1.58)	(1.94)	(1.45)	(1.53)	(2.03)

Table 18

Event Studies of Advising Program Effects Across Outcomes of All Students

	All Schools			LEA Schools			CEM Schools		
	A	F	M	A	F	M	A	F	M
Event = -2	0.26	1.36	-1.58	0.26	1.71	-2.18	0.08	1.25	-1.75
	(1.02)	(1.35)	(1.46)	(1.08)	(1.40)	(1.53)	(1.18)	(1.56)	(1.65)
T = -3	1.03	2.21	0.20	0.85	2.14	0.03	1.52	2.93*	0.57
	(0.98)	(1.31)	(1.43)	(1.09)	(1.41)	(1.52)	(1.07)	(1.42)	(1.54)
T = -2	-1.27	-0.59	-2.26	-1.25	-0.64	-2.08	-0.71	0.03	-1.81
	(1.21)	(1.35)	(1.62)	(1.39)	(1.54)	(1.77)	(1.35)	(1.49)	(1.77)
T = 0	-2.38	-1.60	-3.73*	-2.11	-1.83	-3.38	-1.21	-0.75	-2.19
	(1.29)	(1.38)	(1.82)	(1.67)	(1.77)	(2.18)	(1.44)	(1.52)	(2.02)
T = 1	0.73	1.82	-1.62	1.38	1.88	-0.93	2.09	2.87	0.31
	(1.36)	(1.55)	(1.90)	(1.90)	(2.12)	(2.37)	(1.56)	(1.79)	(2.16)
R^2	0.07	0.06	0.04	0.10	0.09	0.07	0.08	0.07	0.05
N	8,874	8,545	8,539	2,683	2,631	2,636	5,697	5,463	5,494
Panel D: % of Enrollees Who Earned 24 Credits Within 1 Year									
T = -3	0.68	1.32	-0.65	-0.78	0.01	-1.43	-0.34	-0.06	-1.62
	(1.07)	(1.15)	(1.37)	(1.14)	(1.25)	(1.44)	(1.13)	(1.21)	(1.49)
T = -2	0.41	0.51	-1.47	0.05	0.07	-1.18	0.92	-0.13	0.06
	(0.89)	(1.09)	(1.26)	(0.95)	(1.14)	(1.34)	(0.97)	(1.25)	(1.42)
T = 0	0.75	0.57	0.10	1.26	0.96	0.88	1.03	0.64	0.35
	(0.94)	(1.12)	(1.45)	(1.00)	(1.20)	(1.54)	(1.03)	(1.18)	(1.54)
T = 1	0.64	0.42	-0.29	1.37	0.88	0.45	0.98	0.39	0.15
	(1.00)	(1.15)	(1.35)	(1.13)	(1.33)	(1.44)	(1.07)	(1.24)	(1.41)
T = 2	0.58	0.51	-0.17	1.90	1.43	0.93	0.66	0.07	0.20
	(0.94)	(1.13)	(1.42)	(1.22)	(1.45)	(1.66)	(1.04)	(1.28)	(1.54)
T = 3	0.19	-0.05	-0.95	1.64	0.93	-0.09	0.15	-0.86	-0.91
	(1.26)	(1.52)	(1.58)	(1.58)	(1.85)	(1.86)	(1.40)	(1.66)	(1.76)
R^2	0.28	0.25	0.27	0.38	0.35	0.34	0.29	0.24	0.25
N	7,012	6,047	5,154	2,176	1,836	1,437	4,462	3,749	3,111

* p < .05, ** p < .01, *** p < .001

NOTE: Robust standard errors in parentheses. Event studies are conditional on school Title I eligibility and schoolwide program status, percent of students receiving free/reduced price lunch (and a logged version), school enrollment size, and the full-time equivalent number of school counselors and teachers.

LEA = Same District/Local Education Agency. CEM = Coarsened Exact Matched. A = All Students. F = Female Students. M = Male Students.

SOURCE: School Level Files: School Years: 2007-08 through 2020-21, Common Core of Data (CCD), U.S. Department of Education, National Center for Education Statistics; Michigan Center for Educational Performance: Graduation Rates by Student Subgroup Files, College Enrollment by High School Report; College Advising Corps; Michigan College Access Network.

Table 19

Event Studies of Advising Program Effects Across Outcomes of Black Students

	All Schools			LEA Schools			CEM Schools		
	A	F	M	A	F	M	A	F	M
Panel A: Graduation Rates									
T = -3	0.27	0.77	-0.93	-1.01	-0.24	-1.76	-1.00	-0.09	-1.45
	(1.35)	(1.43)	(1.77)	(1.35)	(1.41)	(1.79)	(1.65)	(1.69)	(2.09)
T = -2	0.23	1.09	-0.93	-0.53	0.57	-1.50	-0.55	0.14	-1.37
	(1.09)	(1.05)	(1.38)	(1.10)	(1.06)	(1.40)	(1.11)	(1.25)	(1.46)
T = 0	0.32	-1.31	0.56	0.41	-1.25	0.72	-0.57	-2.45*	0.67
	(0.97)	(1.22)	(1.26)	(1.00)	(1.26)	(1.36)	(1.05)	(1.23)	(1.46)
T = 1	1.68	1.12	0.42	1.77	1.14	0.42	1.01	-0.04	0.73
	(1.10)	(1.17)	(1.44)	(1.10)	(1.16)	(1.50)	(1.22)	(1.31)	(1.70)
T = 2	1.03	0.64	-0.30	1.06	0.75	-0.89	-0.16	-0.64	-1.01
	(1.41)	(1.33)	(1.72)	(1.48)	(1.40)	(1.88)	(1.49)	(1.44)	(1.92)
T = 3	0.40	-0.13	-0.11	1.02	0.66	-0.26	-0.36	-1.36	-0.20
	(1.54)	(1.43)	(1.93)	(1.61)	(1.48)	(2.02)	(1.64)	(1.59)	(2.24)
R^2	0.09	0.06	0.08	0.11	0.07	0.12	0.12	0.08	0.13
N	3168	2470	2442	1339	1197	1192	1875	1493	1507
Panel B: Six-Month College-Going Rates									
T = -3	-0.96	1.36	-0.38	-1.98	0.25	-1.56	-2.70	-1.95	-0.12
	(1.82)	(2.03)	(1.76)	(1.86)	(2.17)	(1.85)	(2.23)	(2.07)	(2.14)
T = -2	-0.48	-0.50	1.88	-1.25	-1.04	0.81	-1.07	-1.47	2.41
	(1.39)	(1.38)	(1.69)	(1.42)	(1.41)	(1.74)	(1.73)	(1.54)	(1.95)
T = 0	-0.79	-1.34	0.34	-1.04	-0.82	0.17	0.53	-0.77	3.11
	(1.43)	(1.63)	(1.83)	(1.43)	(1.66)	(1.89)	(1.55)	(1.85)	(2.04)
T = 1	1.18	0.98	4.35*	0.85	1.01	4.04*	1.83	1.69	6.35**
	(1.57)	(1.84)	(1.75)	(1.62)	(1.92)	(1.81)	(1.83)	(1.82)	(1.95)
T = 2	1.28	1.62	2.19	2.06	2.88	2.50	2.26	0.74	4.86*
	(1.72)	(2.00)	(1.95)	(1.86)	(2.08)	(2.07)	(2.02)	(2.19)	(2.29)
T = 3	0.72	-1.71	2.65	1.38	-0.30	3.09	2.28	-1.32	4.72*
	(1.81)	(2.35)	(1.87)	(1.97)	(2.54)	(2.12)	(2.21)	(2.88)	(2.20)
R^2	0.43	0.39	0.39	0.50	0.42	0.42	0.37	0.37	0.35
N	3245	2592	2535	1355	1228	1217	1927	1555	1564
Panel C: % of Enrollees Who Enrolled in a Four-Year									
T = -3	-2.99	-1.68	-0.21	-4.78	-2.30	-3.35	-2.86	-2.31	-2.00
	(2.68)	(3.09)	(3.02)	(2.82)	(3.31)	(3.10)	(3.26)	(3.65)	(3.37)

Table 19

Event Studies of Advising Program Effects Across Outcomes of Black Students

	All Schools			LEA Schools			CEM Schools		
	A	F	M	A	F	M	A	F	M
T = -2	-2.37	-0.64	-0.75	-3.18	-0.27	-2.20	-1.94	-0.32	-0.55
	(2.15)	(2.61)	(2.57)	(2.22)	(2.70)	(2.70)	(2.41)	(2.78)	(2.94)
T = 0	-2.99	0.60	-0.07	-2.95	-0.22	0.81	-3.16	-0.12	0.95
	(2.35)	(2.71)	(2.92)	(2.49)	(2.91)	(3.00)	(2.45)	(3.06)	(3.16)
T = 1	-2.35	-2.11	0.06	-0.95	-2.54	2.60	-1.92	-2.37	1.27
	(2.30)	(2.52)	(2.81)	(2.50)	(2.75)	(2.87)	(2.59)	(2.97)	(3.12)
T = 2	-5.85*	-5.85*	-2.72	-3.85	-6.02	2.60	-5.22	-7.02*	0.76
	(2.47)	(2.88)	(3.13)	(2.80)	(3.25)	(3.41)	(2.87)	(3.44)	(3.56)
T = 3	-4.48	-2.26	-2.00	-1.27	-1.24	2.95	-2.38	-2.10	1.91
	(2.37)	(2.53)	(3.38)	(2.93)	(3.21)	(3.82)	(2.58)	(3.20)	(3.81)
R^2	0.08	0.07	0.05	0.13	0.12	0.09	0.09	0.09	0.07
N	3224	2577	2520	1349	1223	1212	1908	1544	1553
Panel D: % of Enrollees Who Earned 24 Credits Within 1 Year									
T = -3	-4.21*	-3.09	-2.93	-4.44*	-3.45	-2.56	-5.09**	-3.87	-7.11*
	(1.83)	(2.96)	(3.98)	(1.94)	(3.21)	(4.26)	(1.86)	(3.80)	(3.33)
T = -2	-0.17	0.27	-4.02	-0.02	0.43	-3.94	0.57	0.98	-0.93
	(1.60)	(2.17)	(2.28)	(1.63)	(2.29)	(2.24)	(1.65)	(2.67)	(2.91)
T = 0	-0.67	-2.05	-3.33	-0.40	-2.00	-3.51	0.44	-0.76	-1.55
	(1.52)	(2.40)	(2.69)	(1.58)	(2.63)	(2.87)	(1.53)	(2.66)	(3.14)
T = 1	-0.68	-1.79	-4.90	-0.22	-1.96	-6.41*	0.04	-1.56	-2.56
	(1.67)	(2.18)	(2.80)	(1.74)	(2.41)	(3.05)	(1.60)	(2.56)	(2.89)
T = 2	-0.46	-0.72	-1.27	0.63	0.14	-2.28	-0.42	-1.55	-2.65
	(1.81)	(2.34)	(3.62)	(2.08)	(2.58)	(3.83)	(1.92)	(2.75)	(2.57)
T = 3	-2.92	-0.65	-8.31*	-1.90	-0.52	-8.76*	-2.29	-2.78	-3.37
	(2.11)	(3.42)	(3.49)	(2.37)	(3.72)	(4.33)	(2.35)	(3.63)	(4.33)
R^2	0.31	0.26	0.39	0.40	0.34	0.52	0.34	0.26	0.44
N	1320	844	429	710	496	258	778	510	270

* p < .05, ** p < .01, *** p < .001

NOTE: Robust standard errors in parentheses. Event studies are conditional on school Title I eligibility and schoolwide program status, percent of students receiving free/reduced price lunch (and a logged version), school enrollment size, and the full-time equivalent number of school counselors and teachers. LEA = Same District/Local Education Agency. CEM = Coarsened Exact Matched. A = All Students. F = Female Students. M = Male Students.

SOURCE: School Level Files: School Years: 2007-08 through 2020-21, Common Core of Data (CCD), U.S. Department of Education, National Center for Education Statistics; Michigan Center for Educational Performance: Graduation Rates by Student Subgroup Files, College Enrollment by High School Report; College Advising Corps; Michigan College Access Network.

Table 20

Event Studies of Advising Program Effects Across Outcomes of White Students

	All Schools			LEA Schools			CEM Schools		
	A	F	M	A	F	M	A	F	M
Panel A: Graduation Rates									
T = -3	-0.57	1.17	-1.00	-0.42	1.62*	-1.39	-1.17	0.97	-1.84*
	(0.62)	(0.72)	(0.76)	(0.67)	(0.75)	(0.85)	(0.72)	(0.83)	(0.88)
T = -2	-0.71	1.14	-1.90*	-0.71	1.39*	-2.18*	-0.72	1.19	-2.16*
	(0.62)	(0.69)	(0.80)	(0.65)	(0.70)	(0.84)	(0.66)	(0.76)	(0.88)
T = 0	-0.28	1.83*	-1.86*	-0.43	1.87*	-2.03*	-0.34	1.91*	-2.10*
	(0.56)	(0.73)	(0.83)	(0.60)	(0.72)	(0.87)	(0.60)	(0.78)	(0.90)
T = 1	-1.11	0.86	-2.27*	-1.27	0.83	-2.31*	-1.31	0.86	-2.63**
	(0.68)	(0.72)	(0.91)	(0.76)	(0.78)	(1.01)	(0.74)	(0.78)	(0.99)
T = 2	-1.11	0.57	-1.71	-1.47	0.43	-1.68	-1.26	0.79	-2.17*
	(0.82)	(0.88)	(0.95)	(0.98)	(1.01)	(1.12)	(0.85)	(0.90)	(1.03)
T = 3	-2.14	0.65	-3.10**	-2.86*	0.15	-3.12*	-1.92	1.13	-3.25**
	(1.11)	(0.93)	(1.20)	(1.31)	(1.15)	(1.42)	(1.13)	(0.83)	(1.25)
R^2	0.04	0.02	0.04	0.05	0.04	0.05	0.04	0.03	0.05
N	8019	7475	7490	2132	1996	2025	5169	4797	4845
Panel B: Six-Month College-Going Rates									
T = -3	0.77	1.26	0.68	0.70	1.51	0.52	1.16	1.63	1.19
	(0.94)	(1.09)	(1.26)	(1.04)	(1.14)	(1.41)	(1.07)	(1.30)	(1.37)
T = -2	0.27	0.95	0.43	0.13	1.20	0.03	0.52	1.02	0.14
	(0.87)	(1.01)	(1.21)	(0.92)	(1.06)	(1.28)	(0.94)	(1.15)	(1.38)
T = 0	0.74	0.71	0.24	0.53	1.09	-0.34	0.94	0.88	0.30
	(0.98)	(1.18)	(1.15)	(1.03)	(1.25)	(1.23)	(1.04)	(1.26)	(1.22)
T = 1	1.83	2.80*	1.12	2.10	3.74**	1.10	1.98	2.87*	1.23
	(1.01)	(1.15)	(1.43)	(1.09)	(1.24)	(1.60)	(1.09)	(1.23)	(1.54)
T = 2	-0.60	2.20	-1.60	-0.67	2.86	-1.83	-0.45	2.22	-1.35
	(1.28)	(1.62)	(1.23)	(1.41)	(1.74)	(1.42)	(1.37)	(1.74)	(1.30)
T = 3	2.67*	3.85**	1.42	3.02*	4.89**	1.42	3.28*	4.58**	1.53
	(1.31)	(1.44)	(1.78)	(1.49)	(1.65)	(1.96)	(1.44)	(1.56)	(1.97)
R^2	0.54	0.53	0.42	0.57	0.56	0.41	0.48	0.47	0.36
N	8019	7477	7489	2134	2003	2027	5168	4798	4844
Panel C: % of Enrollees Who Enrolled in a Four-Year									
T = -3	1.73	4.61**	-0.79	1.67	4.80*	-0.98	2.21	5.12**	0.10
	(1.54)	(1.59)	(2.13)	(1.82)	(2.00)	(2.38)	(1.78)	(1.76)	(2.40)

Table 20

Event Studies of Advising Program Effects Across Outcomes of White Students

	All Schools			LEA Schools			CEM Schools		
	A	F	M	A	F	M	A	F	M
T = -2	0.98	4.26*	-1.44	1.15	4.86**	-1.44	0.59	4.26*	-1.70
	(1.48)	(1.66)	(1.98)	(1.58)	(1.76)	(2.08)	(1.61)	(1.88)	(2.17)
T = 0	1.77	4.18**	0.72	1.60	4.26**	0.20	2.36	4.93**	1.35
	(1.25)	(1.47)	(1.83)	(1.38)	(1.55)	(1.94)	(1.36)	(1.58)	(1.97)
T = 1	-1.68	0.45	-2.50	-2.18	0.70	-3.07	-1.13	1.31	-1.93
	(1.48)	(1.65)	(2.09)	(1.73)	(1.97)	(2.37)	(1.62)	(1.77)	(2.27)
T = 2	-1.22	1.11	-2.07	-1.96	0.95	-3.64	-0.41	1.89	-1.07
	(1.59)	(1.61)	(2.31)	(2.07)	(2.33)	(2.82)	(1.72)	(1.72)	(2.54)
T = 3	1.17	3.40	-0.57	0.69	3.45	-1.71	2.30	4.21	1.35
	(1.66)	(1.92)	(2.54)	(2.39)	(2.93)	(3.30)	(1.86)	(2.16)	(2.83)
R^2	0.06	0.05	0.04	0.08	0.07	0.07	0.06	0.06	0.04
N	8007	7472	7478	2129	2001	2023	5158	4794	4834
Panel D: % of Enrollees Who Earned 24 Credits Within 1 Year									
T = -3	1.24	2.28	-1.95	-0.57	0.78	-2.88	0.34	1.18	-2.83
	(1.20)	(1.36)	(1.69)	(1.28)	(1.45)	(1.83)	(1.26)	(1.49)	(1.80)
T = -2	0.62	1.05	-2.25	-0.08	0.30	-2.25	1.34	0.44	-1.18
	(1.04)	(1.22)	(1.65)	(1.15)	(1.30)	(1.79)	(1.15)	(1.38)	(1.85)
T = 0	1.40	1.44	0.19	1.90	1.51	1.56	1.14	0.51	0.46
	(1.17)	(1.36)	(1.92)	(1.28)	(1.51)	(2.08)	(1.29)	(1.46)	(2.01)
T = 1	0.87	1.27	-1.51	1.23	1.19	-0.08	0.69	0.62	-0.91
	(1.24)	(1.49)	(1.75)	(1.43)	(1.72)	(1.85)	(1.34)	(1.57)	(1.79)
T = 2	1.09	2.06	-0.09	2.38	2.41	2.23	1.14	1.12	0.23
	(1.15)	(1.36)	(1.81)	(1.49)	(1.70)	(2.18)	(1.27)	(1.51)	(1.95)
T = 3	1.27	1.33	-0.53	2.77	1.96	1.69	0.78	0.29	-0.80
	(1.38)	(1.64)	(1.99)	(1.80)	(1.95)	(2.42)	(1.55)	(1.86)	(2.17)
R^2	0.27	0.23	0.25	0.37	0.35	0.31	0.28	0.24	0.24
N	6198	5245	4478	1643	1334	1034	3914	3200	2633

* p < .05, ** p < .01, *** p < .001

NOTE: Robust standard errors in parentheses. Event studies are conditional on school Title I eligibility and schoolwide program status, percent of students receiving free/reduced price lunch (and a logged version), school enrollment size, and the full-time equivalent number of school counselors and teachers. LEA = Same District/Local Education Agency. CEM = Coarsened Exact Matched. A = All Students. F = Female Students. M = Male Students.

SOURCE: School Level Files: School Years: 2007-08 through 2020-21, Common Core of Data (CCD), U.S. Department of Education, National Center for Education Statistics; Michigan Center for Educational Performance: Graduation Rates by Student Subgroup Files, College Enrollment by High School Report; College Advising Corps; Michigan College Access Network.

Limitations

Though this analysis suggests near-peer advising programs may impact college-going differentially between student subgroups, there are some important limitations which readers should note. First, though data sources for the study provide rich data on school level outcomes, the censored nature of the outcomes may mask the extent to which programs effect certain outcomes. For example, data suppression rules of the Michigan CEPI prevent disclosure of sample sizes smaller than ten students or of college-going rates in excess of 95 percent, thus schools that are at the extremes in either case will have censored college-going rate values (bottom- and top-coded at 5 and 95 percent respectively) which may bias estimates in either direction. For schools where there are significant decreases from graduates to college enrollees, or from enrollees to those earning 24 credits within their first year, the estimates may be upwardly biased, as available data excludes from models those schools where smaller and smaller proportions of students make it to and are accounted for within the next outcome, as a result of omitted subgroups.

Second, and of a related nature, this analysis is limited in its ability to examine program effects across student groups as a result of limitations of data. Though the analysis was originally intended to examine additional student subgroups beyond Black or African-American and White students (e.g., Asian, Hispanic or Latino), small sample sizes, data limitations[39], and a retraction in the publication of college-going data for certain subgroups of students by the Michigan CEPI[40]

[39] Analysis was originally intended to include an 'economically-disadvantaged' subgroup. Analysis for this subgroup was not conducted as this student subgroup category was first included in the college-going data reports during the 2010-11 school year, the first year an advising program became active (thus eliminating the entirety of the pre-treatment period).

[40] A personal communication with staff at the Michigan Center for Educational Performance Information revealed the center had republished college-going data by high school with "Hispanic" and "Two or More Race" student subgroups missing from the 2010-11 through 2013-14 school years. Staff at CEPI indicated the removal of these groups of students from these years was due to a "data quality concern" (Michigan Center for Educational Performance and Information, personal communication, March 25, 2022) .

ultimately led to the decision to exclude these groups from the main analyses. Supplemental analyses with noted limitations for the Hispanic or Latino student subgroup are included in appendices **Appendix** B, **Appendix** C, and **Appendix** D.

Finally, though this analysis employed multiple robustness checks and measures to assess the reliability of findings, DID is a quasi-experimental design, which limits the certainty with which causal estimates can be identified. This is particularly true as new strategies continue to be developed and advanced in order to correct for limitations of two-way fixed effect modeling when dealing with a staggered treatment. To the extent that the event study modeling can compensate for shortcomings of the TWFE models, the TWFE models may understate program effects. Though newer estimators have recently become available in order to model dynamic treatment effects, the data in the current study do not meet certain assumptions underlying such estimators[41] and thus were not utilized here. As methodological advances continue, the current study may be improved by applying such advances to provide more precise effect estimates.

Discussion

This analysis is one of the few that examines the impact of near-peer programs on outcomes of partner schools the programs serve. While earlier school-level quantitative research has examined proximal college enrollment outcomes (Clayton, 2016), and limited in-state

[41] For example, the Callaway and Sant'Anna (2021) estimator which allows for modeling of dynamic treatment effects assumes once a treatment has begun a unit cannot be untreated. Such an assumption may be reasonable under other circumstances but in the current sample schools are served by advisers for some periods and thereafter the partnership between the program and school ends. While school personnel such as counselors may be able to continue implementation of programs and strategies implemented by previous near-peer advisers, they themselves are not near-peers and thus the impact felt by 'turning off' the treatment may be reversible for the classes of students which follow, as they no longer have access to a near-peer adviser. As an example: a school was treated by an advising program for a two-year period early in the panel and not served by an advising program for the remainder of the panel. Such an estimator would still conflate this treatment effect.

college enrollment outcomes[42] (Belasco, 2014), the current analysis benefits from improvements in state data collection efforts and examines direct college enrollment outcomes for by subgroup, and level, using the most robust data clearinghouse on college enrollment in the nation (provided by the National Student Clearinghouse). Near-peer programs have continued to expand in reach and scope, with the National College Advising Corps serving 747 high schools in 17 states across the nation during the 2020-21 school year (College Advising Corps, 2022). Given the growth of near-peer advising programs, investigating how these programs impact the schools and students they serve is valuable in continuing to improve outcomes for *all* students served by programs.

The results from this analysis provide some suggestive evidence of effectiveness for college-going metrics for some student subgroups. Foremost, the event study analyses supported the effect observed in the TWFE models, suggesting that near-peer advising programs increase college-going rates of female students, at least when compared to other schools in the district[43]. The consistency of the effects for this group within the LEA sample across both TWFE and ES models reinforces the reliability of this finding.

When the TWFE model is constrained to a three-year window pre/post-treatment, results also suggest the near-peer adviser increased college-going rates of Black students overall by roughly 2.4 percent, compared to rates at similar CEM control schools. This effect is not detected in the ES models, though the ES models do detect significant increases in rates of Black male college-going across samples, and particularly in the CEM during the entirety of the three-year

[42] Belasco, (2014) examined outcomes of high schools served by the College Advising Corps in three states, though the data varied with respect to coverage and quality – Pennsylvania's college enrollment data were self-reported by high schools, North Carolina represented enrollments in the NC system and did not capture two-year nor private enrollment, and Missouri did not track outcomes outside of Missouri.

[43] This was the only sample for which the TWFE models found an effect for this group. Effects were observed across all three comparison groups in the event study models.

post-treatment period. Conversely, the three-year window TWFE model for college-going rates of Black males is near though slightly beyond the five percent significance level in the current study (at p = .057) which may lend credibility to the ES estimates obtained. Finally, rates of college-going for White students most consistently improved for White female students, though the White student rate overall experienced increases across samples. Representing the only group for which the TWFE models detected a significant increase in college-going for all three samples, rates for Hispanic or Latino students were estimated to significantly increase between 3.5-4.8 percent (**Appendix B**), and by 4.4 – 6.5 percent for the full and CEM samples when constrained to the three year pre/post window (**Appendix C**). Despite this, no effect was detected in any post-treatment period in the ES models (**Appendix D)** for this subgroup, challenging the reliability of these estimates as we might expect to detect at least a single significant effect in one of the three samples. That coupled with the data limitations previously noted for this group make it challenging to accept these estimates at face value as evidence of efficacy of the intervention.

More broadly, while the estimated increases across groups and samples overall is encouraging for establishing some evidence of program effectiveness across lines of race/ethnicity and sex, other outcomes warrant attention. Surprisingly, across all models only three effects were detected for Black Female students – all of which highlight potential outcomes of concern. Most notably, significant decreases in the proportion of Black Female students attending four-year institutions detected in the full and CEM samples (-5.8 and -7.0 pp, respectively). Given no detected increases in college-going for this group, the near-peer intervention may be guiding more Black Female students into the two-year sector. The current analysis does not incorporate student preferences or characteristics which might help distinguish the extent to which the treatment is driving this outcome – still, with no significant increases in

enrollment for this group overall, and with students having a better chance at persistence and completion at four-year institutions, such patterns *may* result in slowed momentum in credit accumulation (though such an outcome is not observed in the current data). Similarly, rates of credit accumulation for Black male students drops substantially in the full and LEA samples, which may indicate a poor match/fit between enrollees and the institutions they attend. Importantly this may also be a consequence of reduced sample sizes due to data limitations (in which case the large reduction in observations may itself be indicative of fewer students accumulating one year of college credit). Conversely, as the largest gains for Black male students occurs in the CEM sample for the full post-treatment three-year window, the null findings with respect to credit attainment may indicate that *at the very least* Black male students are earning credits at least as fast as they were previously – not necessarily an improvement in match and fit, but also not illustrative of poorer rates of persistence.

CHAPTER 5: DISCUSSION

This dissertation has sought to understand and investigate the impacts of pre-college access programs on postsecondary enrollment and persistence outcomes for students served by these programs. Through two analyses at both the student level and school level, the investigation focused on the extent to which programs addressed match and fit outcomes for students as they made decisions on where to apply and enroll in for college, as well as whether a near-peer college adviser could improve postsecondary outcome rates of partner schools - and if so, for which groups of students. Taken together the results from these analyses are mixed at best.

Results from the first analysis suggest students who participated in a TRIO or GEAR UP program did not ultimately experience significantly different match and fit outcomes compared to similar non-participants, neither when applying to nor enrolling in college. These results though disappointing may not be surprising given that prior research using the same data has found no effect of program participation on college enrollment overall, nor the level of institution students enrolled in (Domina, 2009; Glennie et al., 2015), though decreased persistence of participants was observed in the second semester of the second year (Glennie et al., 2015). The findings here support the conclusion obtained from the *Find the Fit* results which also found differences in rates of undermatching between UB students receiving an advised package of advisement and resources and regular UB students were indistinguishable. As the federal programs investigated often require students to participate over a number of years during high school, the extended duration of exposure to the program appears not to tighten the

selectivity match of participants. This occurs at both the application and enrollment phases for participants across samples and nearly all levels of selectivity undermatched at rates higher than their non-participating peers. Similar to the patterns observed by Hoxby and Avery (2013), undermatch rates among participants is highest among those with access to institutions of the greatest levels of selectivity. Related to the results from the first analysis it is worth reiterating here that the results do not suggest participation in a program is associated with adverse college enrollment outcomes, nor do they suggest students did not individually experience any benefit with respect to match and fit. What they do suggest is that on average these students experienced outcomes that were not substantially different from their peers who did not participate.

Results from the second analysis yield new insight into the impact of near-peer advising programs, which are observed to be modest though significant increases in college-going rates for female students, relative to other schools located in the same districts. Beyond this finding, results are mixed with respect to outcomes between subgroups. Under specific conditions and circumstances college-going rates for overall subgroups of students of color appear to increase as a result of the treatment. Specifically, college-going rates for Black or African American and Hispanic or Latino students at treated schools experience modest to sizeable increases of 2.4 and 6.5 percent respectively, compared to the matched (CEM) sample of schools. While no effects are observed in the event study models for either of these subgroups overall, there do appear to be significant positive increases in Black male college-going rates following implementation. These results suggest near-peer programs may help close the widening gap in college enrollment rates of Black female and male students, particularly when targeted to schools which are similar in demographics and characteristics to treated schools. The most consistent effects on school

outcome rates accrue to White students overall and White female student college-going rates specifically.

Somewhat surprisingly, there were largely no detected effects observed on rates of credit accumulation of enrollees, suggesting that while programs may help students get in the door, the emphasis placed on match and fit during the advisement period may not necessarily translate to increased rates of persistence once students begin their college journeys. Importantly, the only effects on persistence that were observed suggest significant decreases in rates of credit accumulation for Black male students. Previously described data limitations notwithstanding, if the effect on these credit accumulation rates holds in other contexts and programs, the implications are quite serious. Students who do not maintain a timely path to degree completion, are at higher risk of leaving college without a degree – and for those students who have had to take out student loans to finance their education, leaving with debt and no degree from which to receive an economic premium has consequences on ones' life and well-being.

The findings of both analyses are best contextualized within the social capital theory which guided this research. Though programs did not appear to impact match and fit outcomes, Coleman (1988) would likely contend the transmission of social capital was successful if students were moved to postsecondary enrollment in general - particularly in contemporary society where opportunity is severely limited if the individual does not seek to continue their education beyond secondary school. The Bourdieuan perspective would likely suggest that though these programs provide exposure and access to others who can transmit important information and guidance, they were not intended to address academic undermatching – if this is

in fact the case it may arguably be more in line with his broader theories of social reproduction and stratification[44].

It is unlikely the architects of the original federal access programs could anticipate that nearly 70 years later, these programs would not only still be in operation, but also be so prevalent across the nation (and buttressed by a large number of other organizations in the advising space). Initially intended to serve as a short-term tool in waging the 'war on poverty', college access programs like TRIO, GEAR UP, and more recently developed programs like the College Advising Corps have become enduring programmatic interventions that continue to work with new generations of students to help facilitate their entry to postsecondary education. Increasingly if access program proponents are set on continuing to work towards gap closing measures, it now appears the most urgent challenge is to ensure their students are able not only to enroll in college, but also to stay and receive the quality education they deserve.

[44] His theory, unlike that of Coleman's, focuses on the societal level, and specifically how dominant forces and values of those who wield greater volumes of *capital* shape the experiences – educational and otherwise, of individuals in society. At the core, the ruling or dominant class in a society uses education and any other perceived means of social mobility, not to enact or support change that will benefit those with less-capital, but instead to propagate and reproduce societal conditions from one generation to another. This in turn creates distinct societal classes capable of distinguishing the haves from the have-nots. In essence, his theory is not focused on the explanation of social capital as a tool of liberation or mobility, but instead as a tool used to reproduce and propagate conditions of inequity. Though attaining a college education has been historically advanced by many as a way for individuals to achieve social mobility and economic security in the United States through furthering their education, higher education as an institution has arguably been transformed into *the* primary method in the reproduction of social inequality (Musoba & Baez, 2009; Weis, 2016).

Implications and Recommendations

The final sections of this dissertation identify implications, considerations, and recommendations for policymakers, program administrators, and practitioners in the access program space. The recommendations discussed include data availability on pre-college programs, a commitment to ongoing research and evaluation efforts, and the potential benefits of refocusing the limited resources available to programs, in order to maximize effects for students at a resource disadvantage.

Improving Data Availability for Empirical Inquiry

At the outset it is important to recognize that the findings, implications, and recommendations presented here are only possible given the availability of data on programs and participants (whether individual students or partner schools). For the federal programs reviewed here, however, the reliance on national longitudinal datasets should be only a start. Though these datasets are rich in their ability to contextualize students, families, and school experiences, the information related to access program availability and participation provides minimal detail. Such constraints prohibit a more comprehensive understanding of how students experience these programs. Consider that these programs, as grant funded projects, allow for great variability in outreach and offerings, based upon the approach taken by program and school staff:

- Does the program coordinate with school staff to identify students who would benefit from participating in a program?
- What factors for inclusion are considered above and beyond status as a low-income or potential first-generation college student? (e.g., a minimum GPA requirement, a commitment to participate over a multi-year period, aspirations to earn a degree, etc.). Are such factors informally or formally considered?

- Do students need to apply to a given program? If so, does the application require letters of recommendation from teachers or counselors? Are provisions such as agreements for participation from parents/guardians/family required?
- What kinds of activities does a specific project site offer? (e.g., one-on-one individual assistance in college searches/applications, consultation with families, coordinating visits from college admissions representatives, etc.).

In fairness, staff designing longitudinal surveys (e.g., ELS:02, HSLS:09, etc.) are rightly wary of placing additional administrative burden on school and staff respondents who participate in these surveys, fearing unnecessarily long survey instruments might dampen response rates.

If this (or similar) information is available through annual reports submitted by TRIO/GEAR UP staff to ED elsewhere, one way to potentially avoid such design choices is in would be to allow for the integration of said data with these longitudinal surveys post-collection. This would provide a great deal of contextual information on how students are selected (or self-select) into participation in addition to identifying experiences of students within these programs. Such data would allow for empirical investigations to move from studies of "if students participate then…" towards those focused on investigating what *specific* mechanisms, practices, and program elements are most impactful to student outcomes. Certainly, these ideas will require the support of policymakers and practitioners, and will require striking a balance between collecting data which provide breadth and depth, while respecting individual data privacy concerns of participants, programs, and schools.

Commitment to Ongoing Research and Evaluation

Though unrealistic in the short-term, ideally these evaluation efforts would remain ongoing rather than as an addendum to a given longitudinal survey, in an effort to identify the most effective implementation and service strategies in improving match and fit outcomes of participants. Given the substantial investment of funding provided to programs in the federal portfolio each year, supporting a commitment to improving support for students year over year through empirical inquiry should be uncontroversial. Importantly, such efforts *should not serve as a punitive instrument* designed to seek out and eliminate funding for programs with 'poor' outcomes. Rather, this inquiry should focus on identifying which strategies and approaches work in different contexts and under various conditions and aid programs in adjusting support with strategies validated by empirical inquiry, according to the needs of their students. To the latter point, as evinced by findings from the second analysis, the value in such efforts is in the recognition that blanket or uniform practices or approaches may have varied effects for different student populations. What is effective for some may not be effective for all, and in the most harmful cases what is effective for some may be harmful for others. Accordingly, policymakers, practitioners, and researchers must make greater efforts to concern themselves with investigating differential effects, even beyond differences by race and ethnicity and sex as presented in this research. The second analysis of this dissertation is such an example of the willingness of programs like the College Advising Corps and Michigan College Access Network in pursuit of continuous improvement through ongoing evaluation and inquiry. Staff from both organizations were willing and able to provide information on program service sites throughout their histories.

Such information minimally requires programs to catalogue and maintain data on their service over time, and ideally be willing to share such information so as to identify potential areas for improvement and examine effects program are having, both intentional or otherwise.

In describing the importance in identifying such impacts, CAC and researchers recognize: “all evaluation results are welcome. Positive, negative, and inconclusive results are all valuable for understanding the nature of programmatic impact. Scholarly research is welcome. A successful intervention is guided by both data-driven programmatic assessment as well as research-focused scholarly inquiry” (Horng et al., 2013, p. 62).

Focusing Limited Resources

A common challenge for all college access programs is the limited (and sometimes scarce) resources available to the program staff. While funding concerns are typically among the first practitioners think of given the grant funded nature of the federal programs and non-profits like CAC and MCAN, the resource of time and attention is as important for program staff. As such, a recommendation for programs, particularly the near-peer programs to consider is where to focus attention and effort given constraints of time, and perhaps revisit the full school model open-door model (Horng et al., 2013) . Under such a policy, the advisers time may be entirely consumed by motivated students seeking to maximize every resource available to them – this would be consistent with findings of Roksa and Deutschlander (2018) who note “students who seek college information from counselors also consult many more sources of information” (p. 19). This is not to say advisers should turn students away, but rather conducting proactive outreach to engage students who do not actively seek help or to engage in the college-going process may help ensure students who do not seek information of their own volition are not overlooked in the process. These actions are critical in focusing effort on students may not have

the social capital to seek guidance, or the know-how of what questions to ask, or where to get started.

Such considerations for practitioners are not possible for the federal programs as a subset of students are already served, rather than the whole school. Instead, policymakers and those working in the college access space should regularly engage with examining the federal portfolio of programs and assessing whether the landscape of programs and resources offered are optimally organized and distributed. As it stands these programs receive varied support from site to site due to differences between schools and per student funding. For example, on average GEAR UP, Talent Search, and Upward Bound had per student spending of $660[45], $543, and $4,979 during FY 2021. Similar variability in the number of students served per project site ranging from 642 served by Talent Search and 73 students per Upward Bound project. (NCCEP, n.d.; U.S. ED, 2022a, 2022b). Such spending variation ultimately translate to differences in student experience and availability and access to program staff. If programs might benefit from reorganization or consolidation due to duplicative efforts or goals, with such actions focused on improving students' experiences and creating a more coherent federal strategy related to access it is worth at least a discussion. For students without support to navigate the college-going process, college access programs can play a pivotal role in assisting these students through the application and admissions process. The opportunity to do so, however, should ensure services are targeted towards students with the greatest need. As observed in the ELS undermatching analysis, eligibility requirements and student, family, and school characteristics yield low estimates in explaining the probability of student participation in these programs both in this study and in

[45] GEAR UP estimate is based on FY 2022 and is calculated from the aggregated figures of 572,000 GEAR UP students and a $378 million dollar budget, as presented by National Council for Community and Education Partnerships (n.d.).

prior research of the same nationally representative sample (Domina, 2009). While the predictive models utilized may suffer from omitted variable bias which may explain these low estimates, such findings may also stem from the wide variation in program outreach efforts, requirements, and activities provided to students. If student eligibility and participation requires students to apply to participate or relies on the recommendations of teachers, counselors or others, for example, students with the greatest need and who might most benefit from access program services may miss out – if for example they are not inclined or encouraged to apply, or they may be overlooked in favor of students who benefit from and are able to employ greater amounts of social capital with school adults. While the near-peer programs avoid such possibilities by serving all students in a school, the strategy employed in conducting outreach is critical to avoiding similar outcomes. For example, a schoolwide approach which requires students to engage with or seek out the adviser first, as opposed to the adviser conducting proactive outreach may result in an advisers time being largely consumed by highly motivated or academically engaged students who are intent on maximizing their advising resources available to them. Certainly, while this behavior ought not be discouraged as the adviser aims to assist all students and provide equitable and comprehensive advising guidance, the outreach approach utilized may have a significant impact on *which* students decide to take up the advising service offered. Though the empirical strategies employed by the current, and similar research aim to minimize these self-selection effects, these are practical challenges facing access programs which impact outcomes for students and families each day. Delays in engaging in college advising meetings and activities earlier during the senior year can be costly and may result in missed deadlines for submitting college or financial aid applications.

Results from these analyses suggest that at best programs do not appear to negatively impact college enrollment for students served, though enrollment outcomes are not optimal in some cases (e.g., no reduction in undermatch in the first analysis, shifting enrollment of Black female students to two-year institutions in the second analysis). While the push to increase college enrollment continues at high schools across the nation, these increases alone are insufficient, particularly if students persist at lower rates, as appears to be the case for Black male students in the second analysis.

Potential Directions for Future Research

Still, there is more work to be done in this space to continue investigating how these programs impact these outcomes in other contexts and conditions. With respect to near-peer programs, as state data reporting requirements have improved significantly, particularly with respect to college-enrollment outcomes at both the student and school levels, conducting evaluations of these programs has become much more feasible in recent years[46]. Still, future research of these programs and the near-peer model are valuable in understanding the impact across groups in college-going interventions, and the outcomes for cohorts served by these programs. Future research on these programs should examine whether specific *adviser* characteristics, demographic or otherwise, influence the effects experienced by partner schools and their student populations. In other words, beyond a closeness in age, are there other characteristics which influence what exactly constitutes a *near-peer* adviser, and in what ways do these characteristics impact these dynamics?

[46] Researchers should note that unlike the IPEDS data collection where institutions typically retain their Unit Identifier, evaluations of these programs at the secondary school level are slightly more challenging given the specific state/city context being investigated and the need to ensure correct mapping of identifiers over time. This is especially the case in situations where schools frequently undergo mergers/closures and/or are remapped to new districts.

The Enduring Condition of Stratification

Though access to postsecondary education has expanded, the increases in enrollment have largely occurred at non-selective four-year institutions, as well as at community colleges (Carnevale & Strohl, 2014). Seemingly supporting Bourdieu's contention that society and the educational system in particular will inevitably continue to propagate and reproduce stratification, Carnevale and Strohl (2014) argue,

> *"In our postsecondary system, increasing access tends to benefit all groups; however, it also tends to sustain or even increase the distances between them. For example, data comparing postsecondary participation between 1982 and 2004 show a drop from 65 percent to 39 percent between 1983 and 2004 in the share of high school students from the bottom SES quintile who did not go on to postsecondary education. Nonetheless, over the same period, the differences between the postsecondary access between the top and bottom SES quintiles actually increases from a little more than two-to-one to more than five-to-one"* (p. 116).

Indeed, between 1994 and 2006, the number of schools ranked *Most Selective* and *Selective* grew from 146 to 193 and from 253 to 279 respectively[47]. Over the same period, the number of *Less Selective* colleges remained relatively stable (578 to 572) though the number of *Non-Selective* institutions dropped precipitously, from 429 to 299[48]. Such stratification among institutions has been observed over time, with an extensive literature documenting behaviors and strategies employed by universities, faculty, academic journals, and other actors within

[47] Utilizing Barron's Selectivity Index.

[48] Their analysis used the original Barron's assigned category titles of, respectively: Most Competitive and Highly Competitive, Very Competitive, Competitive, Less and Noncompetitive (Carnevale & Strohl, 2014).

postsecondary institutions in pursuit of prestige-linked metrics (U.S. news and World Report rankings, H-Index metrics, etc.) in an effort to differentiate and distinguish ones' group/institution/rank from others (Blackmore & Kandiko, 2011; Bound et al., 2009; D. J. Brewer et al., 2002; Carnevale et al., 2020; Melguizo & Strober, 2007; Meredith, 2004; Rosinger et al., 2019). Though these specific postsecondary prestige metrics are beyond the scope of the current study, literature has documented how their pursuit between institutions impact prospective students seeking admission and increase disparities in enrollment and persistence.

The last fifteen years have demonstrated the increasing importance of postsecondary education for individuals in being able to participate in the U.S. labor market. The two analyses in this study cover students from a period between 2002 – 2021. Most students in the ELS cohort graduated from high school in 2004, and for those who would go on to graduate from college in four years, found themselves confronting a labor market contending with a recession. Research on the recession years has found that nearly all jobs created in the aftermath of the Great Recession went to individuals with at least some postsecondary education (Carnevale et al., 2016), increasingly highlighting postsecondary education as a necessity. Concurrently, during the onset of the recession, the rate of college-going in Michigan was quite low – 37 percent of high school graduates from the class of 2008 went on to enroll in college within 6 months[49]. College-going rates increased in the following years in part due to greater coverage in NSC data and in-part as individuals responded to labor market conditions by returning to school. Despite these increases, student cohorts captured at the end of the timeframe of this study were faced with challenges presented by the Coronavirus pandemic, which disproportionately resulted in loss of jobs and income among lower-income workers, notably those without college degrees.

[49] According to the *College Enrollment by High School Report* produced by Michigan CEPI.

Concerningly, a recent report from the National Student Clearinghouse identified Michigan as being among the top five states (in both percentage change and raw enrollment numbers) with the steepest declines in college enrollment during the pandemic (Sedmak, 2021). Though many initially reasoned four-year college-going would bear the brunt of the losses in the wake of the pandemic, that impact has fallen largely onto the community college sector which has sustained the heaviest losses in terms of college enrollment relative to other sectors (NSC Student Research Center, 2022). During the current crisis, community colleges which have traditionally served larger proportions of underserved students, low-income students, and students of color, have borne the brunt of the pandemic's adverse effects.

While access programs including those studied in this dissertation might benefit from focusing resources on those students who are at greatest disadvantage, these events have also made clear with the evolving landscape of these programs that inequities in access as to *where* students go will continue, absent systemic changes at colleges and universities themselves. Arguably, higher education institutions themselves are the main driver of postsecondary enrollment stratification. The expectation (or perhaps assumption) that these access programs will continue to receive funding and expand their availability to all schools throughout the nation, is precarious given their dependency on grant funding.

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APPENDICES

Appendix A:
Descriptive Statistics of Four-Year Institutions, by Selectivity Index Categorization Status

	Not Categorized by Barron's					In Barron's				
	Mean	SD	Min	Max	N	Mean	SD	Min	Max	N
Admission Rate:										
- All Students	72.8	21.9	18.1	100	464	78.7	22.4	15.3	100	50
- Men	72.9	22.3	0.0	100	459	78.5	22.0	17.2	100	50
- Women	68.7	22.8	0	100	401	79.1	22.8	14.0	100	50
Total cohort graduation rate	44.3	26.2	0	100	623	34.9	16.3	6.0	100	105
Required for Admission:										
- Secondary school GPA	.173	.378	0	1	1,233	.327	.471	0	1	107
- Secondary school rank	.028	.166	0	1	1,233	.178	.384	0	1	107
- Secondary school record	.297	.457	0	1	1,233	.393	.491	0	1	107
- Complete college-prep program	.05	.219	0	1	1,233	.131	.339	0	1	107
- Recommendations	.124	.330	0	1	1,233	.093	.292	0	1	107
- Admission test scores	.146	.353	0	1	1,233	.402	.493	0	1	107
- TOEFL	.229	.420	0	1	1,233	.421	.496	0	1	107

NOTE: Table shows differences between four-year institutions not categorized by Barron's Selectivity Index, and those classified as "Non-competitive", as the former is recategorized into the "Non-Selective" group for the current analysis.
SOURCE: U.S. Department of Education, National Center for Education Statistics, Education Longitudinal Study of 2002 (ELS:2002), "NCES Barron's Selectivity Index, 2004".

Appendix B:
Effect of Near-Peer Advising Programs on School Outcomes of Hispanic or Latino Students

	All Schools				LEA Schools				CEM Schools			
	β	SE	R^2	N	β	SE	R^2	N	β	SE	R^2	N
Panel A: TWFE Models – Outcome: Graduation Rates												
All Latino Students	0.01	(1.16)	.149	2,082	-2.33	(1.30)	.273	704	-0.84	(1.15)	.179	1,334
- Female	-0.47	(1.40)	.161	905	-1.15	(1.21)	.275	430	-1.03	(1.34)	.221	623
- Male	0.36	(1.85)	.184	921	-0.30	(2.04)	.268	443	-1.16	(1.89)	.215	648
Panel B: TWFE Models – Outcome: Six-Month College-Going Rates												
All Latino Students	3.51*	(1.58)	.204	2,066	4.34*	(1.73)	.263	698	4.81**	(1.68)	.205	1,328
- Female	1.06	(2.21)	.193	1,007	0.14	(2.14)	.279	455	0.46	(2.32)	.198	673
- Male	-0.30	(2.19)	.180	975	-0.14	(2.48)	.198	459	0.54	(2.25)	.184	670
Panel C: TWFE Models – Outcome: % of Enrollees Who Enrolled in a Four-Year												
All Latino Students	-0.53	(2.70)	.041	2,039	0.22	(3.16)	.061	689	-0.28	(2.90)	.060	1,310
- Female	0.25	(3.60)	.075	991	3.93	(3.99)	.111	449	-0.86	(4.13)	.099	659
- Male	2.47	(3.47)	.038	946	1.62	(3.97)	.087	448	0.93	(3.94)	.069	652
Panel D: Falsification Tests With False Outcomes												
All Latino Students												
- Mobility Rate	0.04	(0.11)	.183	1,805	-0.05	(0.11)	.277	610	0.11	(0.12)	.218	1,136
- Avg ACT Score	-0.56	(0.76)	.091	7,306	0.06	(0.87)	.124	2,175	0.07	(0.81)	.098	4,858

* p < .05, ** p < .01, *** p < .001

NOTE: Robust standard errors in parentheses. Coefficients represent two-way fixed effect OLS estimates of treatment effect of near-peer advising programs on respective outcomes. All models include unit and school year fixed effects, and control for: Title-I Eligibility, if the school has an active Schoolwide Title-I Program, the percentage of students on free and reduced-price lunch (and a logged version), full-time equivalent number of school counselors, full-time equivalent number of school teachers, and the number of students enrolled at the school (and a logged version). Sample sizes are too small to reliably estimate treatment effects for the fourth outcome. Data for Mobility Rate outcomes is limited to 2009-2020. LEA = Same District/Local Education Agency. CEM = Coarsened Exact Matched.

SOURCE: School Level Files: School Years: 2007-08 through 2020-21, Common Core of Data (CCD), U.S. Department of Education, National Center for Education Statistics; Michigan Center for Educational Performance: Graduation Rates by Student Subgroup Files, College Enrollment by High School Report; College Advising Corps; Michigan College Access Network.

Appendix C:
Effect of Near-Peer Advising Programs on School Outcomes of Hispanic or Latino Students (Three-Year Window)

	All Schools				LEA Schools				CEM Schools			
	β	SE	R^2	N	β	SE	R^2	N	β	SE	R^2	N
Panel A: TWFE Models – Outcome: Graduation Rates												
All Latino Students	0.11	(1.33)	.116	1,771	-1.11	(1.41)	.230	393	-0.36	(1.37)	.137	1,078
- Female	-0.47	(1.60)	.122	715	-2.20	(1.56)	.264	240	-1.39	(1.77)	.217	471
- Male	0.99	(1.82)	.148	727	-0.27	(1.95)	.236	249	0.42	(1.93)	.194	491
Panel B: TWFE Models – Outcome: Six-Month College-Going Rates												
All Latino Students	4.41*	(1.94)	.188	1,751	3.36	(2.34)	.256	383	6.51**	(2.05)	.202	1,069
- Female	3.34	(2.54)	.157	790	2.36	(2.66)	.283	238	3.24	(2.75)	.192	504
- Male	1.22	(2.27)	.203	760	0.65	(2.98)	.269	244	2.49	(2.63)	.201	501
Panel C: TWFE Models – Outcome: % of Enrollees Who Enrolled in a Four-Year												
All Latino Students	-1.10	(2.98)	.043	1,726	-5.22	(3.34)	.103	376	-1.91	(3.29)	.064	1,052
- Female	-1.66	(3.54)	.093	774	6.67	(4.55)	.238	232	-4.47	(4.15)	.155	490
- Male	-2.84	(3.77)	.037	733	-8.81	(5.38)	.162	235	-4.28	(4.02)	.083	484
Panel D: Falsification Tests with False Outcomes												
All Latino Students												
- Mobility Rate	-0.10	(0.12)	.182	1,550	-0.11	(0.14)	.316	355	0.07	(0.11)	.223	932
- Avg ACT Score	-0.40	(0.86)	.076	6,392	-0.38	(1.05)	.080	1,261	-0.29	(0.96)	.083	4,083

* p < .05, ** p < .01, *** p < .001

NOTE: Robust standard errors in parentheses. Coefficients represent two-way fixed effect OLS estimates of treatment effect of near-peer advising programs on respective outcomes. All models include unit and school year fixed effects, and control for: Title-I Eligibility, if the school has an active Schoolwide Title-I Program, the percentage of students on free and reduced-price lunch (and a logged version), full-time equivalent number of school counselors, full-time equivalent number of school teachers, and the number of students enrolled at the school (and a logged version). Sample sizes are too small to reliably estimate treatment effects for the fourth outcome. Data for Mobility Rate outcomes is limited to 2009-2020. LEA = Same District/Local Education Agency. CEM = Coarsened Exact Matched.

Appendix D:
Event Studies of Program Effects for Outcomes of Hispanic or Latino Students

	All Schools			LEA Schools			CEM Schools		
	A	F	M	A	F	M	A	F	M
Panel A: Graduation Rates									
Event = -3	1.32	-1.42	-1.52	2.39	-0.27	-0.89	-0.24	-2.94	-2.69
	(1.88)	(2.87)	(3.07)	(2.01)	(2.97)	(3.18)	(2.19)	(3.27)	(3.44)
Event = -2	2.77	0.91	4.35	3.95	1.73	5.31	2.85	0.75	4.74
	(1.90)	(1.62)	(2.77)	(2.00)	(2.05)	(2.96)	(2.10)	(1.99)	(2.84)
Event = 0	-1.51	-2.02	1.63	-2.47	-2.14	0.61	-2.49	-2.77	0.30
	(1.86)	(1.98)	(2.26)	(1.94)	(1.91)	(2.32)	(1.96)	(2.17)	(2.36)
Event = 1	0.08	-1.04	-2.45	-1.49	-1.00	-4.15	-0.87	-1.74	-3.53
	(1.68)	(2.23)	(2.81)	(1.81)	(2.21)	(2.81)	(1.77)	(2.36)	(2.87)
Event = 2	3.84*	0.57	4.43*	1.80	-0.03	2.93	3.43	-0.06	3.74
	(1.56)	(2.40)	(2.04)	(1.85)	(2.35)	(2.14)	(1.83)	(2.56)	(2.43)
Event = 3	2.67	2.59	-0.62	-1.00	1.04	-3.76	1.82	1.61	-2.47
	(1.80)	(1.80)	(2.84)	(1.86)	(1.83)	(2.82)	(1.96)	(1.90)	(2.93)
R^2	0.164	0.207	0.226	0.299	0.347	0.342	0.199	0.270	0.277
N	2,082	905	921	704	430	443	1,334	623	648
Panel B: Six-Month College-Going Rates									
Event = -3	-2.81	6.98	-8.89	-1.17	10.91*	-7.65	-1.70	7.05	-7.19
	(2.93)	(4.47)	(4.69)	(3.34)	(4.98)	(5.77)	(3.15)	(5.11)	(5.04)
Event = -2	-3.86	1.96	-4.66	-3.90	3.54	-3.83	-2.76	-0.26	-1.60
	(2.77)	(4.02)	(4.20)	(2.84)	(4.25)	(4.43)	(3.27)	(4.13)	(4.83)
Event = 0	1.46	6.10	-4.76	2.45	6.30	-1.77	5.28	5.61	-0.88
	(3.42)	(4.41)	(4.13)	(3.63)	(4.37)	(4.38)	(3.37)	(4.49)	(4.41)
Event = 1	3.82	8.13	-1.54	4.63	8.20	0.20	5.48	5.98	1.21
	(3.26)	(5.00)	(4.33)	(3.30)	(5.51)	(4.70)	(3.47)	(5.39)	(4.84)
Event = 2	2.54	1.90	-3.40	2.65	0.26	-2.41	4.70	-2.93	-0.53
	(3.43)	(4.09)	(4.93)	(3.66)	(4.63)	(5.60)	(3.60)	(4.26)	(5.21)
Event = 3	1.47	8.92	-2.33	0.57	6.07	-2.36	3.83	5.75	-0.56
	(3.29)	(5.01)	(4.34)	(3.89)	(5.35)	(5.63)	(3.44)	(5.11)	(4.72)
R^2	0.214	0.215	0.200	0.291	0.324	0.233	0.222	0.240	0.204
N	2,066	1,007	975	698	455	459	1,328	673	670
Panel C: % of Enrollees Who Enrolled in a Four-Year									
Event = -3	4.19	8.31	18.85*	5.34	4.57	21.36*	5.73	13.63	15.98
	(5.20)	(7.02)	(7.69)	(5.59)	(7.13)	(8.43)	(5.99)	(7.80)	(9.20)

Appendix D:
Event Studies of Program Effects for Outcomes of Hispanic or Latino Students

	All Schools			LEA Schools			CEM Schools		
	A	F	M	A	F	M	A	F	M
Event = -2	-1.80	13.16*	-0.91	-2.06	9.10	1.29	-3.87	14.52*	1.05
	(3.61)	(5.22)	(5.75)	(3.86)	(4.63)	(6.06)	(4.09)	(5.74)	(5.65)
Event = 0	-0.10	6.55	12.07	-2.57	7.97	7.72	-0.49	5.60	9.82
	(5.39)	(6.16)	(6.40)	(5.62)	(6.19)	(6.66)	(6.05)	(6.88)	(6.79)
Event = 1	1.88	3.71	2.82	-2.64	6.04	-3.04	1.98	4.18	-0.55
	(4.10)	(4.85)	(6.07)	(4.45)	(6.16)	(7.03)	(4.20)	(4.89)	(6.71)
Event = 2	-7.14	-4.81	-0.92	-15.17***	-2.35	-7.87	-8.41*	-4.09	-3.37
	(3.71)	(4.40)	(6.23)	(4.36)	(5.54)	(8.09)	(4.01)	(4.88)	(6.56)
Event = 3	4.05	1.36	8.12	-5.29	3.60	-2.68	1.82	3.75	0.05
	(4.17)	(5.54)	(7.18)	(4.86)	(7.01)	(9.31)	(4.38)	(5.69)	(6.68)
R^2	0.052	0.109	0.078	0.108	0.179	0.185	0.082	0.155	0.124
N	2,039	991	946	689	449	448	1,310	659	652

* p < .05, ** p < .01, *** p < .001
NOTE: Robust standard errors in parentheses. Event studies are conditional on school Title I eligibility and schoolwide program status, percent of students receiving free/reduced price lunch (and a logged version), school enrollment size, and the full-time equivalent number of school counselors and teachers.
LEA = Same District/Local Education Agency. CEM = Coarsened Exact Matched. A = "All Students". F = "Female Students". M = "Male Students".

Appendix B presents results from the two-way fixed effect analytic models of the Hispanic or Latino student subgroup for the first three outcomes[50]: graduation rates, college-going rates, and proportion of four-year college-going, and placebo outcomes. These models utilize the full-time window and find no detected effects for outcomes one and three presented in the main analysis (**Table 15**). However, unlike the main analysis the TWFE models detect significant increases in college-going rates across samples for this group overall, with increases of 3.5 percent in the full sample ($p < .05$), 4.3 percent in the LEA sample ($p < .05$), and 4.8 percent in the CEM sample ($p < .01$). No placebo treatment effects were detected for any of these estimates, nor for placebo

[50] Due to small sample sizes, estimates could not be reliably obtained for the fourth outcome for this subgroup.

outcomes of the average composite ACT scores of Hispanic or Latino students nor the mobility rate of this group.

When the models are constrained to the three-year window pre/post (**Appendix C**) the only significance detected is again in college-going rates, though only in the full sample (4.4 percent, $p < .05$) and the CEM sample (6.51 percent, $p < .01$).

Event study models presented for the three outcomes (**Appendix D**) provide mixed findings. Significant increases in graduation rates for Hispanic students overall and Hispanic males in particular are detected in the third year of the program ($t=2$) though only in the full sample of schools. No impact on college-going rates are detected, though the proportion enrolling in four-years may provide some insight into the noted increases picked up by the TWFE models. It should be noted that pre-trend violations are picked up for female and male rates of four-year college-going in the full sample, and separately for females in the LEA sample and males in the CEM sample. Despite this, significant decreases in the proportion of Hispanic students overall who attend four-years are detected in both the CEM and LEA samples during the programs third year ($t=2$) of 15.2pp ($p < .001$) and 8.4pp ($p <. 05$) respectively. The sharp decreases in these proportions may signal that near-peer advisers were able to induce many additional students to enroll in two-year colleges – a shift which may help explain the results across samples picked up by the TWFE models. Still, the data quality concerns noted earlier in addition to null results of the event study models of overall college-going rates for this group make such a conclusion difficult to be certain of.