6-2020

Evaluating College Access Programs: The Emerging Leaders Program Impact on College Enrollment for Hispanic and Immigrant Youth

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2020

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Abstract

The Emerging Leaders Program is a series of workshops targeted at Hispanic and immigrant first-generation students geared towards improving the likelihood they enroll in college. The present study uses different regression techniques and matching algorithms that incorporate propensity scores to estimate the average treatment effect on the treated of the Emerging Leaders Program. This study used participants from the High School Longitudinal Survey of 2009 to create a synthetic control group, Emerging Leaders Program student applications for data on participants, and the National Student Clearinghouse for college enrollment data. The findings indicate that the Emerging Leaders Program increased the probability of enrolling in college in the range of 38 to 43 percentage points for students that participated in the program.

I. Introduction

Education, specifically a college education, is seen by many as the key to improving one's life. Those with bachelor's have been shown to have better outcomes in various areas such as health (Lawerence 2017; Rosenbaum 2012; Hoult 2011) and income (Card 2001; Carneiro, Heckman, & Vytalcil 2011; Rouse 2005; Oreopolous & Petronijevic 2013). According to a recent report from the College Board (2019), individuals with a bachelor's degree will earn $400,000 more in their lifetimes than those with just a high school diploma. Unfortunately, not everyone in the United States is afforded the same opportunities to enroll in college. Historically, people from low-income households, African Americans, and Hispanics have not enrolled in college at the same rates as Whites. Significant progress has been made in the last 20 years in closing this gap. "In 1998, 29% of Black and 21% of Hispanic young adults between the ages of 18 and 24 were enrolled in college, compared to 40% of white young adults. In 2018, enrollment rates were 37% for Black and Hispanic and 42% for white young adults" (College Board 2019). Economists and sociologists alike, have tried to explain the factors that lead to these differences. Economists have viewed the decision to enroll in college more as a cost-benefit analysis. Sociologists have tried to explain the differences in college enrollment through characteristics like socioeconomic status. Both fields have made significant contributions to explaining the factors that lead a student to enroll in college. In the past decade or so, models have been created that combine the methodologies of economists and sociologists.

One factor that has been difficult to include and assess in the college enrollment literature is whether participation in a college access program increases the probability of a student enrolling in college (Perna 2006; Bowman et al. 2018). College access programs are generally run by nonprofit organizations and often collaborate with local public middle and high schools, city and state governments, and local colleges and universities.[1] These programs provide services such as financial aid counseling, college admissions counseling, mentoring, parental advising, tutoring services, and scholarships. College access programs have been created across the nation to help mitigate barriers that prevent disadvantaged students from succeeding in school and enrolling in college like College Possible. Students that typically participate in these types of programs have few (if any) family members or acquaintances who have attended college, meaning they have no one to look to as a model. When a student's family does not have firsthand experience of college, they do not have easy access to knowledge that would help them apply for college and financial aid. This lack of knowledge may even lead families to believe that college may be out of reach for their student. College access programs often fill a void in communities where the local public schools lack the resources, incentives, or knowledge to encourage or support students that want to enroll in college.

Although many of these programs use great anecdotal stories of program participants going to college and succeeding to show their impact, not enough organizations are presenting evidence of a casual effect on college enrollment or other important outcomes. Harvill et. al (2012) conducted a meta-analysis of evaluations and found there was a lack of quality analysis. They found most of the evaluations concluded that college access programs have a positive impact on college enrollment. Evaluating these programs and providing evidence of casual effects are important for many reasons. First, if these programs do not have evidence to demonstrate their impact, they will not be able to develop them further or secure funding to support or expand services. Evaluations can also be used to compare how programs affect

[1] Nonprofit programs include the Emerging Leaders Program, College Possible, and College Forward. TRIO programs build relationships between middle schools, high schools, and universities.

different populations of students and can reveal that some approaches might be better suited for different populations. Also, there is a need to conduct more evaluations on college access programs so that there is robust evidence for their impacts and to warrant inclusion in models that try to find the determinants of college enrollment. Above all else, evaluations are important because these programs have a responsibility to hold themselves accountable to improving the outcomes for low-income and disadvantaged students and their families.

In order to add to the college enrollment literature and the literature on the effects of college access programs on college enrollment, I have conducted a program evaluation of Edu-Futuro’s Emerging Leaders Program (ELP) to measure the average casual effect the Emerging Leaders Program has on participants’ probability of enrolling in college. Using data from participant applications, the National Student Clearinghouse for college enrollment data, and the High School Longitudinal Survey of 2009 to create a synthetic control group, I implemented regression models and matching algorithms with propensity scores to account for selection into ELP and to estimate casual effects. My investigation reveals that the average casual effect of the Emerging Leaders Program on participants’ probability of enrolling in college is 38 to 43 percentage points.

The structure of this paper is as follows. The next section provides a brief description of Edu-Futuro and the Emerging Leaders Program. Section 3 provides a review of the relevant literature. Section 4 will cover my theoretical and empirical models. In section 5, I will discuss my data and in section 6 I will talk about my results and analyses. Finally, I will conclude this paper with a discussion as to why I believe this program has been successful and ways to improve this study.

II. Edu-Futuro and the Emerging Leaders Program

Edu-Futuro is a 501 (c)(3) nonprofit based in Arlington and Fairfax counties, located right outside of Washington D.C. Edu-Futuro has developed a research-based inter-generational approach that ensures immigrant youth and families attain postsecondary goals, improve long-term financial stability, and strengthen interfamily cohesiveness. Edu-Futuro's performance has been nationally recognized for its effectiveness in supporting the success of low-income, underserved youth and parents. They were recognized by the White House's Initiative on Educational Excellence for Hispanics as a Bright Spot in Hispanic Education in 2015 (U.S. Dept. of Education). Edu-Futuro offers several programs for youth and parents including the Emerging Leaders Program (ELP), which is the focus of this evaluation. ELP is a workshop series offered three times per year in the spring, summer, and fall at five high schools in Arlington and Fairfax counties (edu-futuro.org). Each seven-week series consists of twelve workshops on topics including résumé preparation, interviewing, networking, financial literacy, scholarships, public speaking, and the college application process. Students are grouped into small "Crews" of five or six, with each "Crew" lead by two young professional mentors who will help them prepare for speech and scholarship competitions, create résumés, and practice for mock interviews. Students are exposed to several types of career paths through a career panel and small group discussions with the panelists. Students are taken on college campus tours with their parents and mentors. Students and mentors also participate in at least one volunteer project together during the seven weeks. Every series there is one workshop dedicated to discussing the cost and benefits of going to college. For this workshop, students' entire family are highly encouraged to attend. Ultimately, the program's goal is to empower low-income Hispanic and other immigrant youth to overcome the systemic barriers contributing to generational poverty by going to college and becoming the next generation of leaders who will transform their communities.

III. Literature Review

College Enrollment Theory

The foundations for studying college enrollment were built on the work of Gary Becker. In his paper, "Investment in Human Capital: A Theoretical Analysis," he explains how human capital investments are made in order to develop and better individuals' "mental and physical abilities," in order to enhance their productivity. Becker's work was fundamental in understanding why studying college enrollment was an important area of study. After Becker's work, the models used to study college enrollment split between the fields of economics and sociology. Economists adhered to Becker's model of Human Capital and built on it. Sociologists on the other hand, have used a status attainment model, cultural and social capital models to guide their work. College access programs were designed with the findings of both disciplines in mind to help increase the enrollment of low-income and minority students.

The work done by economists in researching the determinants of college enrollment are based on Human Capital Investment models. Human capital theory predicts that if someone is more productive, they will be rewarded with higher earnings (Becker 1993). This theory suggests that the varying amounts of investments in the quantity and quality of one's education, makes a difference in their level of productivity (Becker 1962; Schultz 1961; Perna 2006). Out of these different types of investments in human capital, education is one of the most significant types. Human capital theory assumes that additional education raises one's productivity and this would lead to higher earnings. Education raises productivity and earnings by as Becker put it, “mainly by providing knowledge, skills, and a way of analyzing problems” (Becker 1993, p.19).

In human capital investment models, individuals are seen as rational actors. This means that individuals are assumed to act rationally in deciding how to maximize their utility, given their personal preferences, tastes, and expectations (Becker 1962, 1993). In the choice of deciding whether one will enroll in college, they will base their decision by comparing the expected lifetime benefits with the expected costs. In calculating the benefits of enrolling in college, there are monetary and nonmonetary benefits to consider. The nonmonetary long-term benefits of enrolling in college include more fulfilling work, better health, and lower probabilities of unemployment (Perna 2006). The short-term benefits of enrolling in college can include involvement in extracurricular activities and participation in social and cultural events (Perna 2006). There are also several costs associated with attending college. The direct costs are tuition, fees, room, board, books, and supplies. Indirect costs can be forgone earnings and leisure time. These indirect costs can be seen as opportunity costs. Economists have noted that by just weighing the cost and benefits of college, they cannot explain the observed differences in deciding to enroll or not (Perna 2006). They also believe that variations in enrollment can be shaped by the demand of human capital and the supply of resources for investing in human capital (2006). “Differences in the demand for higher education are expected to reflect differences across groups in academic preparation and achievement” (Perna 2006, 107). “The supply of resources available to pay the costs of higher education are expected to reflect differences in availability of student financial aid, loan limits, and parental willingness to contribute to college costs” (Perna 2006, 107). There are criticisms of the models employed by economists. Even when controlling for factors like academic ability or the availability of financial aid, these models do not totally account for the differences across groups (Perna 2000). Paulsen (2001a) believes that the way students assess the costs and benefits of college vary greatly. He explains that the reasons for different evaluations are usually “non-monetary, less tangible, and more difficult to assess or estimate” (Paulsen 2001a). Some of these factors can include varying sources of and access to information. There can be certain family, school, or community contexts that can lead to a student making a different choice even though they have similar characteristics, like academic ability, to another student.

The work done in sociology has emphasized how socioeconomic factors contribute to college enrollment decisions. The early work in sociology used status attainment models to tackle the question of college enrollment (Perna 2006). Status attainment models focus on the effects of students’ socioeconomic status on their educational goals (Perna 2006). These models predict that the better one does in school, the more encouragement they will receive from teachers, friends, and family. This encouragement will lead the student to aspire to greater goals like attending college. More recent sociological models have focused on how cultural and social capital affect college enrollment. Cultural and social capital is similar to human capital in that they are also used to increase productivity (Coleman 1988) and better one’s life. “Cultural capital refers to the system of attributes, such as language skills, cultural knowledge, and mannerisms, that is derived, in part, from one’s parents and that defines an individuals’ class status” (Perna 2006, p. 111). One potential consequence of insufficient cultural capital is lower educational aspirations such as deciding not to enroll in college. This can be due to one’s lack of knowledge associated with the cultural norms of going to college. “Social capital focuses on social networks and the ways in which social networks and connections are sustained” (Perna 2006, p.112). According to Portes (1998), social capital is accumulated through relationships with others and the greater social networks a person in connected to. Being a part of a network benefits individuals by giving them access to greater amounts of human and cultural capital. Sociological models help inform us how students gather information that can lead to a college enrollment decision, but do not really inform us on how they make decisions based on the information.

Bourdieu (Bourdieu and Wacquant, 1992) and Lin (2001b) have made an argument that a person's choices can only be fully understood when one considers the social context in which that person's choice was made. "Habitus, or an individual's internalized system of thoughts, beliefs, and perceptions that are acquired from the immediate environment, conditions an individual's college-related expectations, attitudes, and aspirations" (Perna 2006). Based on this, it has been argued that an individual's decisions about college are not based on rational analyses but are "sensible or reasonable choices" (McDonough, 1997, p. 9; Perna 2006). It is one's surroundings that influences what might be a "reasonable" choice. Someone's habitus reflects the internalization of structural boundaries and constraints and determines what is possible for an individual. So, it is important to study how structures like schools and communities can affect the decisions of students to enroll in college or not.

In 2006, Laura Perna put forth a new conceptual model for studying college enrollment. Perna says, "When considered separately, neither rational human capital investment models nor sociological approaches are sufficient for understanding differences across groups in student college choice." Economic models help understand decision making, but do not take into consideration the ways that people are able to gather information. Sociological models help inform the ways students gather information but does not do a good job of explaining the decisions that are made with this information. Perna's model combines the economic and sociological models. The model assumes that an individual's college-choice decisions are shaped by four contextual layers: the individual's habitus, school and community context, the higher education context, and the broader social, economic, and policy context. The habitus layer considers areas like demographic characteristics, cultural capital, social capital, academic achievement, and family income. The school community context includes availability of resources, types of resources, and structural supports and barriers. The higher education context includes factors like locations of universities and their characteristics. Finally, the social, economic, and policy context includes demographic characteristics and public policy.

College Enrollment Indicators for Hispanic and Immigrant Students

Given the Emerging Leaders Program serves mostly Hispanic and immigrant students it is important to understand what specifically affects their enrollment. Demographics has played a key role in whether someone of Hispanic descent enrolls in college. Hispanic males have been found to be less likely to enroll in college (Nunez 2012; Sanchez et., 2015). Hispanic females were found to be three times more likely to enroll in college than their male counterparts (Nunez 2012). More recently, according to the National Center for Education Statistics, 32 % of Male Hispanics (18-24) were enrolled in college compared to 41% of Female Hispanics in 2016. This 9% gap is the largest Male-Female gap amongst all races/ethnicities. Several factors have contributed to this trend. From an early age, Hispanic and Black boys were more likely to be diagnosed with a learning disability, serious emotional and behavioral disorder, and labeled "at-risk" (Saenz 2009). These things stigmatize boys and lower their confidence, making them not like going to school and learning. These attitudes can be carried through their whole schooling experience and impact their college choice. There are also cultural expectations for Hispanic males that might decrease their likelihood of enrolling in college. Males in Hispanic families are expected to sacrifice their needs over the needs of the family (Saenz 2009). They are expected to contribute to the family, especially economically, if the family is struggling to pay their rent or for other necessities. This might mean working after school and not having time to complete homework or study for tests. This could also mean that Hispanic males might have to drop out of high school so they could work full-time to help their families. This cultural pressure is even greater for young Hispanic male immigrants. High school GPA has been found in many studies to be a significant predictor of college enrollment (Berkner & Chavez 1997; Cabrera & La Nasa 2001; Kuh et al., 2008; Radunzel & Noble 2012). But Hispanic students' GPA remain lower on average that their White counterparts (Miller & Garcia 2004). Income has been shown to have a positive relationship with college enrollment across many studies (Ellwood and Kane 2000; Hossler, Schmit, and Vesper 1999; Kane 1999). With regards to Hispanic students, Nunez and Kim (2012) found that students from families that make between $25,000 and $75,000 were less likely to enroll in college than a student from a Hispanic family that makes more than $75,000. Interestingly, they also found that the odds of a student from a family that makes less than $25,000 enrolling in college is not statistically significantly different from the odds of a student from a family that makes more than $75,000 enrolling in college. Parental involvement and expectations play significant roles in if a Hispanic student enrolls in college. A parent that is involved in their student's education makes it about twice as more likely that their student will enroll in college (Nunez 2012). Parents that have high educational expectations for their student can be significant factor in their student enrolling in college (Walpole 2007; Nunez 2012). Finally, immigration status' and parent immigration status of Hispanic students are important variables to consider. Lauderdale and Heckman (2017) found that children with at least one immigrant parent had a higher likelihood of enrolling in college. In a model of only Hispanic students, having at least one immigrant parent was found to be positively related to college enrollment. Siahaan et al., (2014) found that U.S.-born children with both foreign-born parents were the most likely to enroll in college followed by foreign-born children with both foreign-parents.

Social and cultural capital also play very important roles in increasing the likelihood a Hispanic student will enroll in college. Perna (2000) found that Hispanics and Whites have comparable rates of college enrollment when controlling for differences in costs, benefits, ability, and social and cultural capital. In other words, the lower observed enrollment rate for Hispanics is attributable to their lower levels of social and cultural capital. Also, social and cultural capital were just as significant a factor as academic ability. Information about college can be considered a form of social capital and many Hispanic families have a lot less of it. In a telephone survey of 1,054 Hispanic parents in Chicago, New York, and LA conducted by Tornatzky et al. (2002), 65.7% of parents missed at least half of the straightforward information items about college. They found that socioeconomic status shaped how they got information about college. For people with low socioeconomic status, it was important to have a family member or relative that had college knowledge. For those of middle socioeconomic status internet and union membership were important sources of information. For all levels of socioeconomic status, school counselors were important sources of information. Even with more information, Hispanic students still needed guidance in the college application process. Venegas (2006) did a qualitative study on availability and accessibility of financial aid information and found that Hispanic students had access to information via the internet, but still needed help of an advisor to complete financial aid paperwork.

Evaluations of Other College Access Programs

College access programs were created to help increase the number of low-income and minority high school students enroll in college. The federal government established three major educational initiatives called TRIO: Upward Bound, Talent Search, and Student Support Services. Upward Bound and Talent Search were geared towards increasing enrollment of low-income and minority students in postsecondary education. Student Support Services was created to increase the college retention and graduation rates of first-generation college students from low-income families. The evaluations of Upward Bound and Talent Search have produced mixed results in their effectiveness of increasing college enrollment (Harvill, Maynard, Nguyen, Robertson-Kraft, & Tagnatta 2012). This meta-analysis of college access program evaluations from Harvill et. al looked at 12 different college access programs and found that their average impact on enrollment in a 2-year or 4-year college was a 12-percentage point increase. For evaluations that were evaluated by a randomized controlled trial, enrollment increased on average by four percentage points. The al Seftor, Mamun, and Schirm (2009) found that Upward Bound did not affect college enrollment. However, they did find that Upward Bound had a positive and significant effect on college enrollment for a subgroup of students who had lower educational expectations at the onset of the program. Constatine et al., (2006) found that Talent Search participants in Florida, Indiana, and Texas were more likely to enroll in college than nonparticipants via a propensity score analysis. Another college access program was established by the Clinton Administration in 1998 called GEAR UP. GEAR UP also was created to help increase college enrollment like the TRIO programs but was distinct from the TRIO programs. GEAR UP is different in that it pushes for more of a systemic change in public schools by providing a cohort or priority model in which a group of students participate in the interventions each year from seventh grade through at least high school graduation (Ward 2006). GEAR UP also requires collaborative partnerships among states, a local educational agency, local universities, middle and high schools, and community organizations. There have been limited evaluations of GEAR UP on college enrollment. Sondergald et al., (2013) and Knaggs et al., (2015) compared students who enrolled at a high school before GEAR UP was implemented with those who enrolled later and received GEAR UP services. The students that received services had college enrollment rates that were 7 to 11 percentage points higher than those that did not receive services. Using propensity score methods, Fogg and Harrington (2015) found that GEAR UP participants in a Rhode Island program were 15 percentage points more likely to attend college than nonparticipants. Bowman et al., (2018) conducted a difference-in-differences analysis of GEAR UP in Iowa and found that program participants were 3 to 4 percentage points more likely to enroll in college than nonparticipants. Along with these federal programs, nonprofit organizations have also created college access programs that help the communities in which they are located. One of these nonprofits is College Possible. They have college access programs in cities like Chicago, Philadelphia, and Omaha. In a working paper, Avery (2013) conducted a randomized controlled trial of the College Possible program in Minneapolis and St. Paul. The evaluation found that initial enrollment at four-year colleges increased by more than 15 percentage points for program participants.

IV. Theoretical Framework and Empirical Model

Given that this study is not randomized, but observational, different econometric techniques must be implemented to arrive at a casual estimation of treatment effects. Under a randomized designed study, one can control for biases like endogeneity of treatment assignment, which is of much concern for economists, by randomizing who receives treatment. Endogeneity and self-selection bias are major concerns in observational studies that want to assess the casual effect of a program, like the Emerging Leaders Program, because the individuals that choose to participate in the program are by definition different than those who choose not to participate. One key difference that can be hard to measure is level of conscientiousness. Those that have high levels of conscientiousness usually have high level of self-discipline, are good at setting long-range goals and planning how to meet those goals. In the context of this study, conscientiousness can play a big role in how students plan to go to college and ultimately, enroll. These differences may lead to an inability to draw causal comparisons of the outcomes between the treated and non-treated subjects. What is needed are econometric techniques that mimic random assignment so casual estimations can be made without concern of bias.

Donald Rubin (1983) formulated the most commonly used framework to estimate casual effects in observational and experimental studies. In Rubin's Potential Outcomes Framework, one starts by defining a casual effect at the unit level in terms of potential outcomes. Potential outcomes are "pairs of outcomes defined for the same unit given different levels of exposure to the treatment, with the researcher only observing the potential outcome corresponding to the level of treatment received," (Imbens and Wooldridge 2009). In the case of the Emerging Leaders Program, one potential outcome is the potential college enrollment status if student i participated in the program and the other is the potential of college enrollment status if student i did not participate in program. Formally, let E_i be a binary indicator that equals to 1 if individual i receives the treatment of participating in the Emerging Leaders Program, and E_i equals to 0 if individual i does not participate in the Emerging Leaders Program. Then, let $E_i = 1$ and $E_i = 0$ denote the potential enrollment in college indicators under participation and non-participation in the Emerging Leaders Program, respectively. The causal effect for any individual *i* is defined in Equation 1 below:

$$E_i = Y_i(1) - Y_i(0) \qquad (1)$$

Where $Y_i(1)$ and $Y_i(0)$ represent potential college enrollment statuses for individual *i* when *i* participates in the Emerging Leaders Program ($E_i = 1$) and does not participate ($E_i = 0$).

The main point of interest when conducting a casual study is to compare two outcomes for the same unit when it receives treatment and when it does not receive treatment. Equation 1 represents this scenario, but we cannot observe someone that simultaneously participates in the Emerging Leaders Program and does not participate. We can only observe one potential outcome, participation or non-participation, which depends on the actual value that E_i takes. Paul W. Holland (1986) refers to this as the fundamental problem of casual inference. Let Y_i denote the realized or observed outcome, the observable outcome. If individual i participates in the Emerging Leaders Program, the realized $Y_i = Y_i(1)$ and $Y_i(0)$ will be the ex post counterfactual outcome. This same logic applies for the reverse situation. From this we can say the following,

$$Y_i = Yi(E_i) = Y_i(0)\,(1-E_i) + Y_i(1)\,E_i$$

$$= \begin{cases} Y_i(0) \text{ if } E_i = 0, \\ Y_i(1) \text{if } E_i = 1, \end{cases}$$

The potential outcomes framework offers many advantages including that, "…it separates the modeling of the potential outcomes from that of the assignment mechanism," (Imbens and Wooldridge, 2009) and "…allows us to formulate probabilistic assumptions in terms of potentially observable variables, rather than in terms of unobserved components," (Imbens and

Wooldridge, 2009). These benefits allow researchers to employ different econometric and statistical strategies to essentially re-create a randomized experiment with observational data.

In a randomized experiment with repeated sampling, the mechanism of randomly assigning the treatment ensures the equal distribution of the treated and control groups on all observed and unobserved covariates. Randomization is ensuring we are comparing "like with like." It also ensures that any observed differences in outcomes between the treated and control group is solely due to the treatment. Due to the "fundamental problem of casual inference" unit-level casual effects cannot be estimated, but randomized experiments allow for estimation of the Average Treatment Effect of a population by computing the difference in means of potential outcomes between those in the treatment and control groups. Equation 2 below estimates the Average Treatment Effect (ATE) in a randomized experiment:

$$ATE = E[Y_i(1) - Y_i(0)] \tag{2}$$

This equation would not be suitable to estimate the ATE in my evaluation of the Emerging Leaders Program since it is an observational study and it is a voluntary program. So in general, $E[Y_i \mid E=1] - E[Y_i \mid E=0]$ does not equal the ATE in equation 2, where $E[Y_i \mid E=1]$ is estimated by averaging the observed outcome in the treatment group and $E[Y_i \mid E=0]$ is the average outcome for the controls. In a fully randomized trial, equation 2 allows us not to worry about endogeneity of treatment because the assignment of treatment is random and uncorrelated to any observable or unobservable covariate, which will lead to an unbiased estimation of ATE. So in the case the ATE in equation 2 is estimated $E[Y_i \mid E=1] - E[Y_i \mid E=0]$.

In the case of the Emerging Leaders Program where students voluntarily chose to participate and receive treatment, we cannot say that an observable covariate, like GPA, or an unobservable variable, like conscientiousness, is uncorrelated to the treatment. It may be that students with high GPA's are more likely to join the program and these students may have already been more likely to enroll in college without joining the program. This possibility would bias the estimation of the ATE by skewing it to show that ELP has a bigger effect on college enrollment than it really does. Econometrically, this issue amounts to a violation of assuming independence between potential outcomes and treatment, which is required to obtain unbiased estimates of equation 2. This is an issue that needs to be resolved before one can arrive at an unbiased estimate of ATE and in the following paragraphs, I will explain how this issue can be minimized.

In Rosenbaum and Rubin's 1983 seminal work they laid out how an observational study can get to the point where it looks like a randomized experiment and we can have an unbiased estimate of ATE. In this work they showed that if the assumptions of unconfoundedness and overlap were met, also known as "strongly ignorability", then treatment assignment is ignorable. In other words, the treatment assignment does not necessarily have to be randomized to arrive at unbiased casual estimators. Unconfoundedness means that conditional on observed covariates, the potential outcomes are not influenced by the treatment assignment. Intuitively, this means that by controlling for all the possible covariates that may lead a student to participate in the Emerging Leaders Program and enroll in college or not, we essentially remove the problem of endogeneity of treatment and can calculate an unbiased estimation of the ATE. The overlap assumption says that given a set of covariates, a person with the same set of covariates has a positive and equal opportunity of being assigned to the treated group or the control group. For my study, this essentially means that there are enough students in my control group that have very similar covariate values as the students that participated in the Emerging Leaders Program. I believe that these assumptions hold in my evaluation of the Emerging Leaders Program because I have access to a rich set of covariates, which the program collects on their application needed to admit a student to the program. Also, all of these covariates were available in the HSLS dataset and includes a lot of students to draw upon for comparison in order to meet the overlap assumption. Under these assumptions we can estimate the Average Treatment Effect on the Treated (ATET) since we are conditioning on the observed covariates. Equation 3 below depicts the treatment effect of interest:

$$ATET = \quad E[Y_i(1) - Y_i(0) \,|\, E_i = 1] \tag{3}$$

The ATET in eq. (3) is then estimated by: ATET = $E[Y_i \,|\, E_i = 1, X_i = x] - E[Y_i \,|\, E_i = 0, X_i = x]$

where X_i is the set of covariates used in this evaluation and x is a specific value taken by X_i.

Both assumptions do come with issues. Unconfoundedness is fundamentally untestable. Meaning there is no way to verify that we are making a correct assumption, but a sensitivity analysis can be done to assess its plausibility. The overlap assumption may get hard to satisfy if there are many covariates. It can also necessitate the creation of a synthetic control group if one's dataset only includes treated subjects, which I have done in my study (Rosenbaum and Rubin 1985). To mitigate the issue of confounding covariates to help achieve balance, Rosenbaum and Rubin suggested a balancing score, such as the propensity score, "…can be used to group treated and control units so that direct comparison are more meaningful." They defined the balancing score as follows: "A balancing score, b(x), is a function of the observed covariates x such that the conditional distribution of x given b(x) is the same for treated (z = 1) and control (z = 0) units." They proved that the propensity score is the coarsest balancing score. The estimated propensity score is the conditional probability of being assigned to a particular treatment given a vector of observed covariates (Rosenbaum and Rubin, 1983). They further declared,

> "At any value of a balancing score, the difference between the treatment and control means is an unbiased estimate of the average treatment effect at the value of the balancing score if treatment assignment is strongly ignorable. Consequently, with strongly ignorable assignment, pair matching on a balancing score, subclassification on a balancing score and covariance adjustment on a balancing score can all produce unbiased estimates of treatment effects" (Rosenbaum and Rubin, 1983, 43).

A propensity score is most commonly calculated using Logistic Regression. A Probit model can also be used. Under the assumptions outlined by Rosenbaum and Rubin, I am able to move forward in calculating the Average Treatment Effect-on-the-Treated for those that participated in the Emerging Leaders Program using methods based on propensity scores. The following formula estimates the ATET conditional on the propensity score:

$$ATET = E[Y_i \mid E_i = 1, \hat{e}(X_i) = e] - E[Y_i \mid E_i = 0, \hat{e}(X_i) = e] \qquad (4)$$

where $\hat{e}(X_i)$ is the estimated propensity score and e is a specific value of the propensity score.

Under the assumptions of unconfounded treatment and overlap, there are three general econometric techniques used to estimate treatment effects: regression-based methods, propensity score methods, and matching methods. These methods can be mixed together, and they often produce better estimates. I will be using regression adjustment, inverse-probability weighting, and propensity score matching to estimate the ATET of the Emerging Leaders Program. It is good to use a variety of estimation techniques to see if there is variability in the results and help recalibrate one's model if there is variability. I will discuss the method of regression adjustment in estimating casual effects in some detail, but for a more detailed and deeper understanding of all of these methods please see the following: Abbrin and Heckman (2007), Angrist and Krueger (1999), D'Agostino (1998), Heckman, Lalonde, and Smith (1999), Heckman and Vytlacil

(2007a), Holland (1986), Imbens and Wooldridge (2009), Imbens (2004), Imbens and Rubin (2015), Rosenbaum (1989), Rosebaum (1995), Rosenbaum (2002), Rosenbaum and Rubin (1983b), Rubin (1973b), Rubin (1997), Rubin (2006). The first step in regression adjustment is to calculate propensity scores using a logit or probit model for all subjects. I have included all of my covariates in this calculation. After calculating the propensity score, it is included in a regression model as a covariate along with a treatment variable indicating whether the student participated in the Emerging Leaders Program or not. I have not included any other covariates in this model because they were all included in calculating propensity scores. The dependent variable in this model is whether or not the student enrolled in college within a year of graduating high school. This method is commonly used in clinical research, see Shah (2005) and Weitzen (2004) for how it has been used in clinical research. Also, see Berk and Newton (1985) and Muller et al. (1986) for other studies that used this technique. The benefit of using this two-step process is that it can fit a more complicated propensity score model with interactions and higher order terms if desired (D'Agostino 1998). It also simplifies the model when a propensity score is included. With a smaller model it may allow one to perform diagnostic checks on the fit of the model more reliably than if one were to include many covariates in the model (D'Agostino 1998). This technique does have limitations. It requires a substantial amount of overlap between the treated and the control. If there is a substantial difference in the covariate distributions of these groups, then regression adjustment is not effective. From Rubin (2001) three conditions must be satisfied for regression adjustment to be reliable:

1. The difference in the means of the propensity scores in the two groups being compared must be small (e.g., the means must be less than half a standard deviation apart), unless the situation is benign in the sense that: (a) the distributions of the covariates in both groups are nearly symmetric, (b) the distributions of the covariates in both groups have nearly the same variances, and (c) the sample sizes are approximately the same.

2. The ratio of the variances of the propensity score in the two groups must be close to one (e.g., ½ or 2 are far too extreme).
3. The ratio of the variances of the residuals of the covariates after adjusting for the propensity score must be close to one (e.g., ½ or 2 are far too extreme); "residuals" precisely defined shortly.

I will be using the "teffects ra" command in Stata to run this estimation technique on my data.

The other two estimation techniques that I will implement is Propensity Score Matching (PSM) and inverse probability weighting. PSM has a couple of advantages. PSM makes it easy to match subjects. The is method can reduce k-dimension observable variables to just one, that is, the propensity score. This helps researchers by limiting the number of observations that must be sacrificed. This method also eliminates two sources of bias: bias from nonoverlapping supports and bias from different density weighting (Heckman et al., 1998). Also, PSM does not assume a functional relationship between the outcome and control variables. The first step in using PSM is to calculate the propensity score for all the subjects. This is usually calculated using logit or probit models. For my study, I will be using a logit model to calculate propensity scores. The second step is to choose a matching algorithm. There are several types of algorithms such as Nearest Neighbor, Caliper and Radius, Stratification and Interval, and Kernel and Local Linear. Deheija and Wahba (2002) were able to demonstrate that if there is a lot of overlap in the distribution of propensity scores between the treated and control groups, these techniques will produce similar results. The choice of algorithm can vary case-by-case and can largely depend on the data structure (Zhao 2000). For more insight on these different algorithms please see Caliendo and Kopeinig (2008) or Imbens and Wooldridge (2009). I will be employing the nearest neighbor algorithm via the "teffects psmatch" command in Stata. This algorithm matches a subject from the treated group with a subject in the control group that is closet in terms of the propensity score (Caliendo and Kopeinig 2008). With nearest neighbor matching one must also consider matching with replacement or without replacement. In the case of replacement, a subject in the control group can be used more than once as a match, in contrast to without replacement where a subject could only be used once. There are trade-offs between bias and variance when considering whether to use replacement or without replacement. Replacement comes with a decrease in bias and increase in variance. Without replacement produces the opposite, increase in bias and decrease in variance. Since much of the literature focuses on reducing bias rather than variance (Imbens and Wooldridge 2009), I will be using replacement. PSM does come with limitations. First, this method has no test statistics. This means that although it helps find casual inferences, it cannot help with finding statistical inferences. Another limitation is that there is no way to test whether treatment assignment is strongly ignorable. This method cannot eliminate bias due to unobservable variables (Heckman et., 1998). Unobservable variables can increase the bias of the ATET when using this method. Please see Angrist (1998), Dehejia and Wahba (2002), Jalan and Ravallion (2003), and Couch and Placzek (2010) as examples of studies that implemented PSM. Finally, I will also be using inverse probability weighting to estimate the ATET of the Emerging Leaders Program. I will be using the Stata command "teffects ipw" to calculate the ATET. The first step is to calculate propensity scores using a logit or probit model. They are then used to create weights to reweight the treated and control subjects to make them more representative of the population. The weight of a treated subject is the inverse of the propensity score and the weight of a control subject is the inverse of one minus its propensity score. A benefit of this technique is that it leads to a more efficient estimator. One limitation of this method is that if the propensity score is close to zero or one, the weights will be large and make those subjects highly influential in estimating the ATET. This can lead to an imprecise estimator. For a more in-depth discussion on this estimator, please see

Hirano, Imbens, and Ridder (2003). Also, see Chen, Mu, and Ravallion (2008) for a study that used this technique.

V. Data

This study uses data three sources: student applications for the Emerging Leaders Program, college enrollment data from the National Student Clearinghouse, and the High School Longitudinal Survey of 2009. Students participated in the program between the years of 2013-2018. 441 students participated in the program during these years. From the student applications, I created 29 binary covariates to serve as controls. These covariates captured information on sex, race, GPA, whether they were a first-generation student, if they were born in the US or not, their parental/guardian status, whether they attended a public high school and income. My measure of income is based on whether a student indicated they receive free or reduced lunch. For a student to receive free lunch, they must come from a household with incomes below 130 percent of the poverty level. Those with family incomes between 130 and 185 percent qualify for reduced-price lunches. Edu-Futuro deems a student as first-generation if their parent/s did not go to college. In order to measure GPA, I created 8 binary variables that represent ranges of GPAs. The GPA variable did present issues. 41 students did not include a GPA range on their application. Since GPA is a very important covariate, I had to make the decision to drop these observations. There were significant differences in means for key covariates between these 41 students and those with complete data. Dropping these 41 students could somewhat limit the inference of the results. This left 400 ELP students in the sample. For a table of the means of each group, please see table 2 in the appendix. Most of the students that chose to participate in ELP were Hispanic, female, received free or reduced lunch, first-generation students, and attended a public high school.

The dependent variable in my study is whether a student enrolled in a 2-year or 4-year college within one year of graduating high school. To gather this data, I used the National Student Clearinghouse database. This database includes data from over 3,600 colleges and universities that enroll 98% of all postsecondary students in the United States. To obtain college enrollment statuses, I had to submit the names and birthdays of all the students that participated in ELP. One is only able to tell that a student is enrolled in college if results are returned for the student and if no results are returned, that is an indication that they had not enrolled in college. Also, students can withhold their data from researchers if they so choose and researchers will not know which student chose to withhold data. The individual results for that student will appear as they did not enroll in college. There was one case of this happening in the 441 records that were submitted. There was one case of this happening amongst ELP students. After my first submission to the database, I shared my results with Edu-Futuro staff, and they pointed out that there were many students that came back as not enrolled in college when they knew they were. These false negatives were alarming. After contacting some of these students, we found that many students did not include their full legal names on their student applications. Some students did not include middle names or their second last names. It is very common amongst the Hispanic population to legally retain two last names. When including middle names and/or second last names for the students that provided that information on the application, results were returned for many of these students. For students that still did not return results, I called them and ask them if they had enrolled in college or not. To capture all the students that gave verbal confirmation that they were enrolled in college and all of those verified through the database, I created a second dependent variable called College2. The database confirmed 85% of ELP students had enrolled in college. With verbal confirmations, 87% of ELP students had enrolled in college.

The High School Longitudinal Study of 2009 is a nationally representative, longitudinal study of 23,000+ 9th graders from 944 schools. I used the publicly data set to construct a synthetic control group as to be able to compare with ELP students that share similar characteristics. This dataset had all the same variables that were available on the ELP application. This is essential to be able to match them to similar ELP participants. This dataset had a lot of missing data. Over 7,000 students in this dataset did not have any information on whether they were born in the US or in a foreign country. This is an important variable since ELP tries to attract many immigrant students to participate in the program. I was forced to drop all of these students. 903 students were also missing GPA data. I had to drop these 903 students as well because GPA is an important predictor. Please table 2 in the appendix for more information on these students. After dropping all of these students, 15,081 students were left in the control group.

Table 1 on the following page is a table of the summary statistics of the two groups. Included are the differences in means and standard errors, as well as a t-statistic. There are significant differences in all variables except Black, Asian, Pacific Islander, Lives with Guardian, and GPA4. These differences indicate that these two groups are not comparable, but with different techniques they can be made to be comparable.

Table 1.

Treated and Control Groups Summary Statistics

Variables	Mean/Std.Dev for Treated (Obs: 400)	Mean/Std.Err. for Control (Obs: 15,081)	Difference (T-C) in Mean/Std.Err.	T-Value
Male	0.338/0.473	0.501/0.500	-.164/.025	-6.48***
Female	0.663/0.473	0.499/0.500	.164/.025	6.48***
Free or Reduced Lunch	0.693/.462	0.402/0.490	.291/.025	11.71***
Regular Lunch	0.308/.462	0.598/0.490	-.291/.025	-11.71***
Hispanic	0.76/.428	0.156/0.363	.604/.018	32.70***
Black	0.123/.328	0.129/0.335	-.006/.017	-.36
White	0.04/.196	0.648/0.478	-.606/.024	-25.41***
Asian	0.003/.255	0.076/0.265	-.006/.013	-.43
Pacific Islander	0.005/.05	0.005/0.071	-.003/.004	-.71
Multiracial Non-Hispanic	0.005/.071	0.086/0.280	-.081/.014	-5.76***
Native American	0/0	0.007/0.086	-.007/.004	-1.68*
Native Born (US & Puerto Rico)	0.493/0.501	0.924/0.265	-.432/.014	-31.18***
Foreign Born	0.508/0.501	0.076/0.265	.432/.014	31.18***
First Generation Student	0.695/0.461	0.279/0.449	.416/.023	18.29***
Lives with Two Parents	0.63/0.483	0.746/0.435	-.116/.022	-5.26***
Lives with One Parent	0.353/0.478	0.197/0.398	.156/.020	7.68***
Lives with a single mother	0.3025/0.460	0.168/0.374	.134/.019	7.05***
Lives with a single father	0.05/0.218	0.029/0.167	.021/.009	2.47**
Lives with a guardian	0.018/0.131	0.028/0.166	-.011/.008	-1.29
Other living situation	0/0	0.027/0.161	-.027/.008	-3.31***
Attends Public School	0.993/0.086	0.807/0.395	.186/.020	9.40***
Attends Private School	0.008/0.086	0.193/0.395	-.186/.020	-9.40***
GPA0 (0-.99)	0/0	0.019/0.136	-.019/.007	-2.77***
GPA1 (1.00-1.49)	0.005/0.072	0.031/0.173	-.026/.009	-2.98***
GPA2 (1.50-1.99)	0.013/0.111	0.058/0.234	-.046/.012	-3.90***
GPA3 (2.00-2.49)	0.07/0.255	0.113/0.317	-.043/.016	-2.70***
GPA4 (2.50-2.99)	0.155/0.362	0.162/0.369	-.007/.019	-.40
GPA5 (3.00-3.49)	0.34/0.474	0.195/0.396	.145/.020	7.21***
GPA6 (3.50-3.99)	0.325/0.469	0.204/0.403	.121/.021	5.89***
GPA7 (4.00+)	0.0925/0.290	0.218/0.413	-.125/.021	-6.03***
College	0.853/0.355	0.541/0.498	.312/.025	12.43***
College2	0.878/0.328	0.541/0.498	.337/.025	13.43***

*p<.10, **p<.05, ***p<.01

VI. Analysis & Results

Before using the three estimation techniques that were discussed earlier, I produced some preliminary results to be used as a baseline to understand the extent and direction of bias. A simple comparison of means of the dependent variable, College, between treatment and control groups, shows a difference of .312 between the two groups. The treatment group had a larger means. I also did a simple regression of all covariates including the treatment variable on the dependent variable College. This regression produced a coefficient of .369 on the treatment variable. See the results in the table on the following page:

Table 2. Regression of all Covariates including Treatment on College

Variables (DV: College)	Coeff.	Std. Err.	t-value	p-value	95% Confidence Interval	
Male	-0.005	0.007	-0.74	0.462	-.018	.008
Free or Reduced Lunch (Ref: Regular Lunch)	-0.106	0.008	-13.79	0	0.091	0.121
Race (Ref: White)						
Hispanic	0.006	0.010	0.56	0.572	-0.014	0.025653
Black	0.070	0.011	6.36	0	0.048	0.0914844
Asian	0.047	0.014	3.35	0.001	0.020	0.0750593
Pacific Islander	-0.086	0.048	-1.78	0.075	-0.181	0.0086407
Multiracial NonHispanic	-0.031	0.013	-2.43	0.015	-0.056	-0.0060256
American Indian	-0.043	0.041	-1.05	0.294	-0.124	0.037502
Born in another country (Ref: US Born)	-0.003	0.013	-0.23	0.815	-0.029	0.023
First Generation Student	-0.117	0.008	-14.57	0	-0.133	-0.101
Living Situation (Ref: Other Living Situation)						
Lives with Two Parents	0.067	0.021	3.23	0.001	0.026423	0.108
Lives with One Parent	0.050	0.022	2.3	0.022	0.007318	0.092
Lives with Guardian	-0.066	0.029	-2.29	0.022	-0.12217	-0.010
Attends Public School (Ref: Private School)	-0.106	0.009	-11.78	0	-0.12412	-0.089
GPA Ranges (Ref: GPA0)						
GPA1	-0.039	0.032	-1.21	0.224	-0.100	0.024
GPA2	-0.002	0.029	-0.08	0.938	-0.059	0.054
GPA3	0.127	0.027	4.67	0	0.073	0.179
GPA4	0.252	0.026	9.53	0	0.200	0.304
GPA5	0.391	0.026	14.86	0	0.340	0.443
GPA6	0.519	0.026	19.65	0	0.468	0.571
GPA7	0.620	0.027	23.29	0	0.568	0.672
Participate in EduFuturo	0.369	0.023	16.12	0	0.324	0.414
Constant	0.159	0.034	4.72	0	0.093	0.225

The first attempt to produce the ATET of ELP with the regression adjustment method produced a coefficient of .385. This would mean that the average casual effect of ELP for participating students is about 38.5 percentage points. Meaning that by participating in the program their probability for enrolling in college increased by about 38.5 percentage points. To verify that these results are reliable, I checked the three points found in Rubin (2001). I found that my results violated these three points. When these three points are violated that means that there is not balance in the covariates. Figure 1 and 2 show the distributions of the propensity scores for the control and treated groups.

Figure 1. Control Propensity Scores

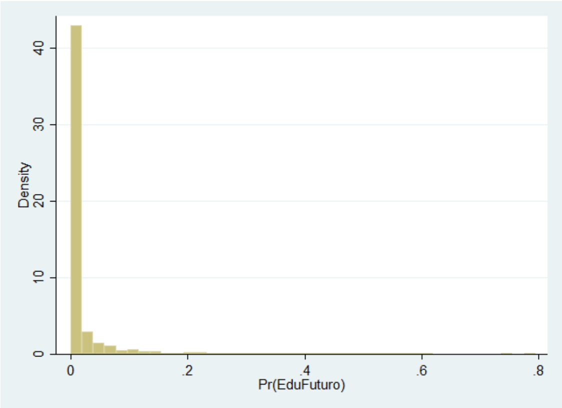
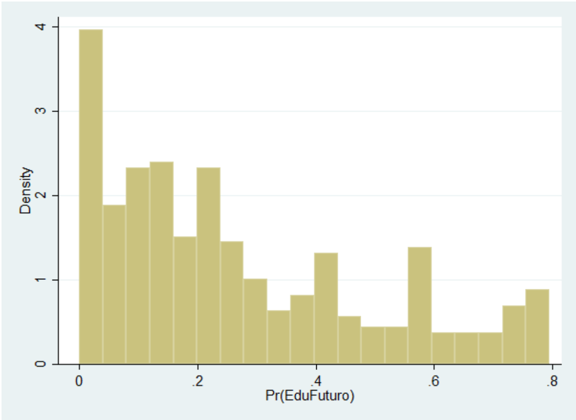


Figure 2. Treated Propensity Scores



These graphs show that there is not much balance between the two groups. One way to create more balance is to trim the propensity scores. I was able to create better balance by dropping observations with propensity scores less than .25 and greater than .75. After trimming, the ATET was .383 and there were 175 subjects in the control group and 128 in the treated group. You can see the better balance in propensity scores in figures 3 and 4 below.

Figure 3. Control after trimming

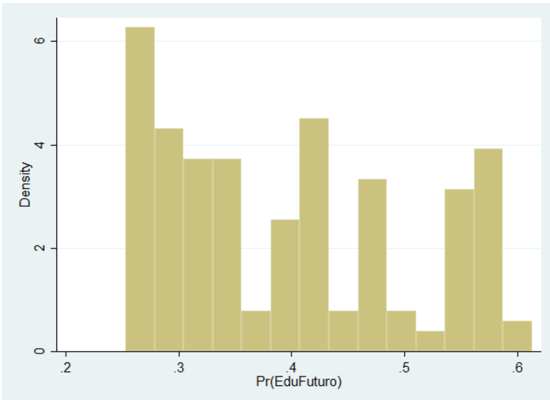
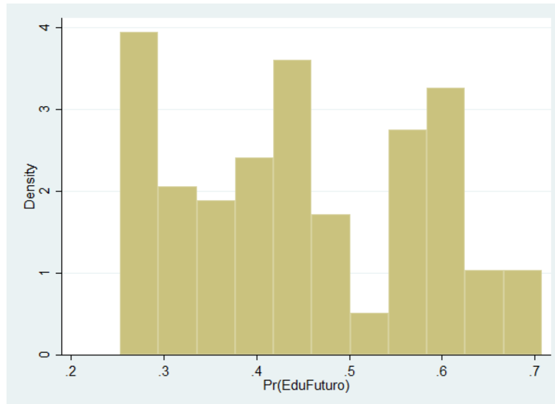


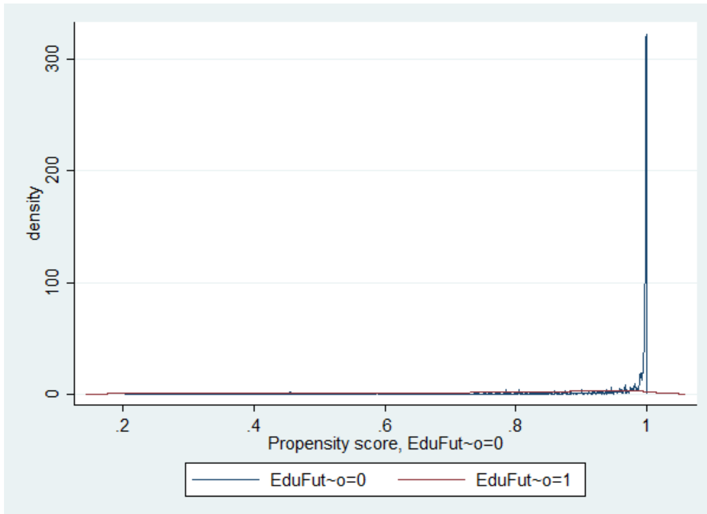
Figure 4. Treated after trimming



After trimming, some variables were dropped because they no longer had any observations in the trimmed sample. This altered the reference groups for race and GPA compared to the simple regression. The racial reference group became Black, and the GPA reference group was GPA7 in the trimmed sample. This caused the Hispanic covariate to become negative and significant. The GPA variables became negative. Overall, there was not much difference between the regression adjustment and the simple regression. For full results, please see table 6 in the appendix.

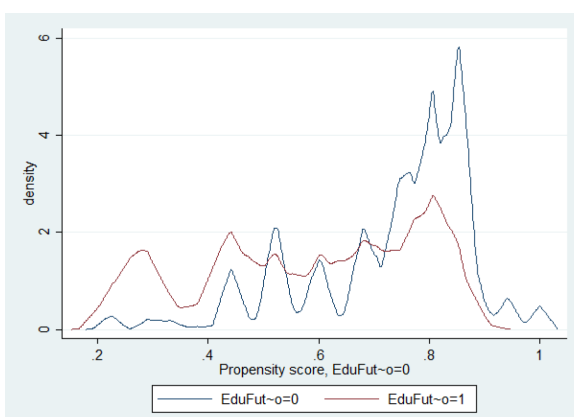
In my first attempt to produce a reliable estimation of the ATET of ELP using the IPW method, I found an ATET of .422. Next, I checked to see if the overlap assumption was violated by using the command "teffects overlap" in stata. This command creates a graph that displays the densities of the probability of getting each treatment level. If a graph shows too much probability mass near 0 or 1, the overlap assumption is violated. Figure 5 below shows that the overlap assumption is clearly violated.

Figure 5. Densities of Probability before Trimming



I also used the “teffects overid” command in stata to perform a chi-squared test for balance in the covariates after using the IPW method and found that they were not. In order to not violate the overlap assumption and balance the covariates, I once again trimmed observations. I trimmed any observation with a propensity score less than or equal to .15. After trimming, 488 observations were left remaining in the control group and 223 in the treatment group. I ran the estimation again and found an ATET of .412. I used the “teffects overlap” command again to see if the overlap assumption was violated and it was not. See Figure 6 below for the graph.

Figure 6. Densities of Probabilities after Trimming



I also retested the covariate balance with the "teffects overid" command and this time the results indicated that they were balanced. The "tebalance summary" command also shows that the covariates are balanced. We know they are balanced since most of the standardized differences are near 0 and the weighted variance ratios are near 1. See the output for this command in the following page:

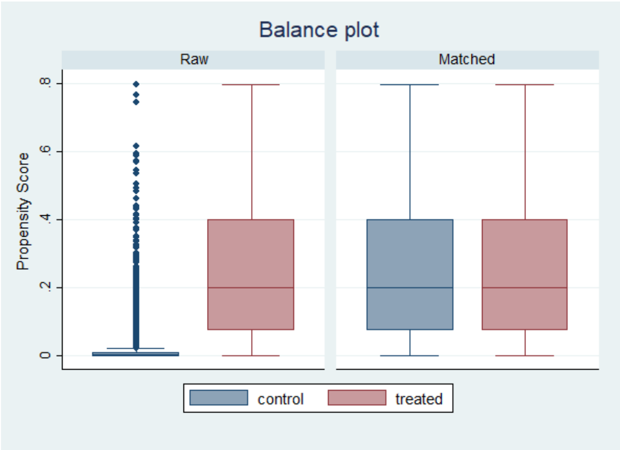
Table 3. Standardized Differences and Variance Ratios of IPW Estimator

	Standardize Differences		Variance Ratio	
Variables	Raw	Weighted	Raw	Weighted
Male	0.067	0.057	1.082	1.066
Free or Reduced Lunch	0.153	-0.013	0.845	1.018
Hispanic	-0.047	0.004	1.114	0.991
Black	0.197	-0.003	1.845	0.992
Born in another country	0.404	0.003	0.758	0.997
First Generation Student	0.160	0.023	0.775	0.958
Lives with Two Parents	-0.262	-0.033	1.142	1.009
Lives with One Parent	0.239	0.040	1.143	1.014
GPA2	-0.180	0.005	0.173	1.075
GPA3	-0.066	0.007	0.741	1.036
GPA4	-0.033	-0.01	0.934	0.973
GPA5	0.001	0.034	1.003	1.018
GPA6	0.138	-0.028	1.068	0.992

Trimming also affected the covariates for the IPW method. The racial reference group became Asian and the GPA reference group became GPA7. Compared to the simple regression, the magnitudes for the Hispanic, Black, Born in another country, and First-generation student greatly increased. The signs for Born in another country and First-generation student switched from negative to positive. The sign for Lives with Two parents also switched from positive to negative compared to the simple regression sample. The ATET also increased using this estimation technique. For full results, please see table 7 in the appendix.

Finally, I estimated the ATET of ELP with a propensity score matching algorithm called nearest neighbor matching. Using the "teffects psmatch" command in Stata, I got an ATET coefficient of .409. Figure 7 below shows a box plot of the propensity scores before and after matching occurred.

Figure 7.



The box plot shows that that there is much better balance in propensity scores after matching has occurred. I also used the "tebalance summarize" command to look at the balance. Most of the covariates are balanced. See the following page for the output of this command:

Table 4. Standardized Differences and Variance Ratios of Nearest Neighbor Matching

	Standardized differences		Variance Ratio	
Variables	Raw	Matched	Raw	Matched
Male	-0.337	0.053	0.897	1.041
Free or Reduced Lunch	0.610	0.022	0.888	0.983
Hispanic	1.523	-0.072	1.389	1.099
Black	-0.019	0.088	0.961	1.250
Asian	-0.022	-0.010	0.932	0.968
Born in another country	1.078	0.035	3.577	1.000
First Generation Student	0.915	0.043	1.056	0.966
Lives with Two Parents	-0.253	-0.068	1.234	1.043
Lives with One Parent	0.354	0.053	1.447	1.036
Public	0.650	-0.032	0.048	1.496
GPA3	-0.150	0.041	0.651	1.154
GPA4	-0.020	-0.034	0.965	0.939
GPA5	0.333	0.053	1.435	1.040
GPA6	0.276	-0.042	1.353	0.971
GPA7	-0.351	-0.009	0.494	0.976

In order to gauge the robustness of the results, I used the "psmatch2" command in Stata with common support implemented. Common support ensures that all observations has a comparable unit. Common support is not used in the "teffects psmatch" command in Stata. Using this command, I got the same exact coefficient but different standard errors. This is to be expected. The "teffects psmatch" considers the fact that propensity scores are estimated rather than known when calculating standard errors. Meaning that the "teffects psmatch" standard errors are more reliable. I also used the trim option with the "psmatch2" command. The trim option imposes common support by dropping a certain percentage of the treatment observations at which the propensity score density of the control observations is the lowest. I did this at the 5%, 10%, and

15 % levels. I got coefficients of .4248, .4205, and .4278, respectively. This indicates that the original estimate is robust. Figure 8 below are all the estimations of the ATET of the Emerging Leaders Program.

Table 5. Results for all Estimation Techniques

Estimation Technique	**ATET (DV: College)**	**S.E**	**ATET (DV: College2)**	**S.E**
Simple Difference	0.312	.	0.337	.
Regression	0.397	0.023	0.393	0.023
Regression Adjustment before trimming	0.385	0.019	0.410	0.019
Regression Adjustment after trimming	0.383	0.052	0.422	0.049
Inverse Probability Weighting before trimming	0.422	0.025	0.429	0.024
Inverse Probability Weighting after trimming	0.412	0.039	0.443	0.037
Nearest Neighbor Matching without common support	.409	0.029	0.449	0.030
Nearest Neighbor Matching with common support	0.424	0.029	0.449	0.028
Nearest Neighbor Matching with common support Trim (5%)	0.425	0.023	0.451	0.022
Nearest Neighbor Matching with common support Trim (10%)	0.421	0.022	0.448	0.021
Nearest Neighbor Matching with common support Trim (15%)	0.430	0.022	0.457	0.021

These results show that the Emerging Leaders Program is increasing participants' probability of enrolling in college. Comparing the results from a simple difference in means to the rest of the estimates shows that the control group was not all that similar to the treated group but with trimming and matching, they became much more comparable. These results show that the ATET of ELP on participants range from 38 percentage points to about 43 percentage points when College is the dependent variable. When the dependent variable is College2, the ATET ranges from 39 percentage points to about 46 percentage points.

VII. Conclusion

My research found that the average casual effect of the Emerging Leaders Program for participating students is in the range of 38 percentage points to 43 percentage points. This means that students that participated in the program increased their probability of enrolling in college in the range of 38 to 43 percentage points. This is a pretty significant increase in probability. Other studies of college access programs did not see an increase this large. I do believe that this program is unique in many ways that could help explain these results. The inclusion of families in events like college trips and information sessions is one reason that I believe this program is very successful. Many families that participate in the program have very little to no knowledge about the whole college process. This lack of knowledge leads to parents not knowing how to appropriately support their child. The knowledge that parents gain during these events help them learn how to emotionally support their child and ease their burden. The strongest component of ELP is the creation "Crews." The crew structure allows students to build stronger relationships with one another. These type of supportive peer relationships have been connected to the continued pursuit of academic goals and school-appropriate behavior (Hudley et al. 2009). Another aspect of the crew that is unique is that they all have at least two young professional adults as mentors. This program tries hard to recruit mentors that have the same backgrounds as the students. The mentors are mostly Hispanic first-generation college graduates. They have and continue to encounter many of the same problems as program participants and still went to college and graduated. They can give participants insights and advice that no other people could have given. These relationships that students can build in the program dramatically increase their social capital and leave the program feeling like they are not alone and have people they can turn to for help and support.

This study can be approved in a couple of ways. The inclusion of more variables that measure academic rigor can help. Many studies have concluded that a rigorous course load is correlated to college enrollment. Currently, this information is not being collected on the student application. Although, the program does conduct a pre-program survey that includes questions that can helpful in constructing measures of motivation and conscientiousness, this data is not collected in a manner that is easily accessible by a researcher. It has been recommended that the program invest in tools that improves data collection and retention in order to improve future evaluations. Another way to improve this study in the future is to include students that applied to the program but decided not to participate in the control group. This could help control for a lot of more environmental variables that could not be controlled for when creating a synthetic control group from a nationally representative sample. Unfortunately, the program does not retain these applications.

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Appendix

Table 6. Regression Adjustment with Trimming

Variables	Coefficient	Robust Std. Err	z	p-value	95% Confidence Interval	
Male	0.167	0.087	1.93	0.054	-0.003	0.337
Free or Reduced Lunch	-0.212	0.090	-2.36	0.018	-0.389	-0.036
Hispanic (ref: Black)	-0.492	0.131	-3.75	0	-0.749	-0.234
ForeignBorn	-0.098	0.153	-0.64	0.524	-0.398	0.203
First Generation Student	-0.157	0.118	-1.33	0.182	-0.388	0.074
Lives with Two Parents (ref: Guardian)	0.463	0.085	5.42	0	0.295	0.630
Lives with One Parent	0.292	0.158	1.85	0.064	-0.017	0.601
GPA3 (ref: GPA7)	-0.109	0.156	-0.7	0.485	-0.415	0.197
GPA4	-0.028	0.154	-0.18	0.854	-0.331	0.274
GPA5	0.014	0.149	0.09	0.927	-0.278	0.305
GPA6	0.140	0.150	0.94	0.348	-0.153	0.434
Constant	0.775	0.391	1.98	0.047	0.009	1.542

Table 7. IPW with Trimming

Variables	Coefficients	Robust Std. Err.	z	P-value	95% Confidence Interval	
Male	-0.301	0.202	-1.5	0.133	-.698	.092
Free or Reduced Lunch (Ref: Regular Lunch)	-0.014	0.221	-0.06	0.949	-.448	.420
Race (Ref:Asian)						
Hispanic	1.919	0.513	3.74	0	.913	2.924
Black	2.022	0.571	3.54	0	.904	3.140
Born in another country (Ref: US Born)	1.653	0.221	7.49	0	1.220	2.085
First Generation Student	1.095	0.261	4.2	0	.583	1.606
Living Situation (Ref:Guardian)						
Lives with Two Parents	-0.577	0.826	-0.7	0.485	-2.196	1.043
Lives with One Parent	0.378	0.836	0.45	0.651	-1.261	2.017
GPA Ranges (Ref:GPA7)						
GPA2	-1.171	1.096	-1.07	0.285	-3.319	.977
GPA3	0.321	0.648	0.5	0.62	- .949	1.591
GPA4	0.823	0.565	1.47	0.143	-.280	1.937
GPA5	1.469	0.541	2.71	0.007	.408	2.530
GPA6	1.803	0.543	3.32	0.001	.739	2.867
Constant	-5.655	1.195	-4.73	0	-7.997	-3.310