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Essays on the US Higher Education

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Abstract

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Two-year colleges, or community colleges, are an integral part of the US higher education system. More than 40% of undergraduate enrollment occurs at the two-year level. Moreover, two-year colleges are closely related to four-year programs, as students frequently transfer between two-year programs and four-year programs. In fact, close to 50% of bachelor's degree recipients have enrolled in two-year colleges before transferring to four-year programs. The following essays discuss three topics related to the US higher education system while emphasizing two-year colleges' role in this system.

The first chapter studies policies that can address the low completion rate of two-year college students. Utilizing two recent institutional reforms in the University System of Georgia, I show that allowing community colleges to offer bachelor's degrees and consolidating institutions increase two-year students' bachelor's degree attainment by around 3 percentage points, which represents a 20% improvement. Both reforms increased the two-to-four transfer rate, and institutional consolidations also increased bachelor's degree attainment, conditional on transferring. Moreover, I find evidence that a reduced loss of credits during transfer is the driving force of the improvements. In particular, the reforms reduced credits lost during transfer by around 40%.

The second chapter examines whether free community college could fulfill its promise to boost upward mobility or create a trap that promotes associate degrees over the more lucrative bachelor's degrees. Using adminisitrative data from Texas, I build and estimate a model of college choice, educational attainment, and earnings that allows students to transfer between institutions, and captures the complex credit transfer rules between community colleges and four-year colleges. Leveraging this model, I find that providing free community college improves students' welfare and associate degree attainment, but decreases bachelor's degree attainment by 7 percentage points (a 21% decrease) and average life-time income by more than 1%. This is because the policy diverts students to the less lucrative community colleges and subjects more students to imperfect information about the transfer pathways. In particular, students transferring from community colleges to four-year colleges severely underestimate the credit lost before transferring. I propose a

cost-equivalent proportional tuition reduction that creates notably larger welfare and income improvements. In addition, I find that eliminating credit lost during transfer and providing perfect information on credit transfer rules significantly improves transfer students' outcomes. Finally, I show that the existence of transfer options is crucial for the overall bachelor's degree attainment rate and has a modest impact on student welfare.

While community colleges educate more than 40% of US undergraduates, anecdotal evidence suggests widespread discrimination against community college graduates. In the third chapter, I use a national labor market audit study to examine the existence and nature of such discrimination. I send out more than 3600 artificial job applications through one of the largest online job platforms in the US. All applicants have four-year Bachelor's Degrees, and a randomly selected subset of the applicants attended community colleges for their first two years of college. I find that the callback rate from accounting firms is 50% lower for applicants with community college experience. This is equivalent to the effect of a drop in college GPA from 3.6 to 3.2. In comparison, sales and marketing positions' callback rate do not exhibit such a discrepancy. Furthermore, I find suggestive evidence that the discrimination is due to irrational bias on community college students' ability. I also find that this bias significantly reduces employers' valuation of the candidates' other qualifications, such as college selectivity.

Essays on the US Higher Education System

A Dissertation

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Dissertation Directors: Joseph Altonji, Costas Meghir, and Cormac O'Dea

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The research project in chapter 1 commenced in 2017, the project in chapter 2 commenced in 2018, and the project in chapter 3 commenced in 2019. Meanwhile, I started dating my wife in 2017, proposed to her in 2018, and we got married in 2019. As an econometrician, I am convinced that this perfect overlap in time periods proves that this dissertation would not have been possible without her support. Shurui is not only the love of my life, but also my best friend, and most trusted critic. My research have benefited immensely from countless intellectual conversations we had during walks, phone calls, and even during our dates.This dissertation is dedicated to you and our cat Oreo.

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To my time studying labor economics.

Chapter 1: Improving Graduation Rates in the Two-to-Four Transfer Pathway

[1]

(Accepted at the *Education Finance and Policy*)

The two-to-four-year college pathway is an important route towards a bachelor's degree in the US higher education system. After graduating from high school, a student can first enroll in a two-year program designed to substitute for the first two years of a four-year program.[2] Then, the student would transfer into a four-year program with third-year standing, and seek to graduate with a bachelor's degree in two additional years.

The two-to-four transfer pathway enrolls a large portion of students seeking bachelor's degrees. Unfortunately, the pathway has a very high attrition rate. Among students who received a bachelor's degree in 2015, 49% had previously enrolled at a two-year college (Shapiro et al, 2017). Partly due to community colleges' lower costs, 44% of students with family incomes less than $25,000 per year attend community colleges as their first college after high school, while only 15% of higher income students do so (Shapiro et al., 2017). More than 70% of all community college freshmen intend to obtain a bachelor's degree, but only 26% successfully transfer into a bachelor's degree program. Furthermore, only 57% of those who transfer obtain a bachelor's degree. This graduation rate is significantly lower than that of non-transfer junior-year students, which is around 70%.

The high attrition rate in the two-to-four transfer pathway stems from a long list of challenges students who first enroll at community colleges experience. First, students lose a large number of credits when transferring between institutions, and the longer enrollment and increased tuition required as a result of the credit lost leads many transfer students to drop out of college. On average, students lose 12.7 semester credit hours when transferring between institutions (Simone, 2014). Second, for many students in community colleges, finding a path to degree completion is extremely difficult given the lack of academic advising resources (Scott-Clayton, 2015). A recent national survey finds that only 56% of entering students in community colleges have had an advisor help them set academic goals and create plans for achieving them (Center for

[1]I would like to thank two anonymous referees and the associate editor of the Education Finance and Policy, Cassandra Hart, for their incredibly helpful comments.

[2]These two-year programs are typically offered by community colleges. However, as is the case in the University System of Georgia, many colleges offer both two-year and four-year programs.

Community College Student Engagement, 2018). Third, transferring to a different institution involves significant adjustment costs, and such costs are particularly high for community college students, who tend to be older, are more likely to work part-time, and are more likely to come from disadvantaged socioeconomic backgrounds (Ma and Baum, 2016).

Using a rich administrative dataset from the University System of Georgia (USG), I examine the effects of two groups of recent institutional reforms in USG on bachelor's degree attainment through the transfer pathway. The first group of reform consisted of two institutional upgrades, during which two two-year colleges started offering bachelor's degrees. The second group of reform consisted of three institutional consolidations, during which three pairs of colleges merged their academic programs. I use difference in differences (DID) to estimate the treatment effects of the two reforms. For each type of reform, I estimate two different treatment effects. The first is the treatment effect on the first few cohorts of students that *transferred out* of a two-year program after its reform (*exit cohort*). The second is the treatment effect on the first few cohorts of students that *entered* a two-year program after its reform (*enter cohort*).

I find that both institutional upgrades and institutional consolidations increase bachelor's degree attainment rate for 2-year program freshmen by around 3 percentage points, which represents a 20% improvement. In particular, both the institutional upgrades and consolidations improved the transfer rates into four-year programs (11% and 23% improvements), and consolidations additionally improved students' graduation rate conditional on transferring (22% improvement). I find evidence that reduced loss of credits during transfer is the driving force for the positive treatment effects of both reforms — both reforms reduced loss of credits during transfer by around 48%.

I then move on to unpack the treatment effects into policy-relevant factors that can be generalized beyond the context of institutional consolidations and upgrades. Using a simple model of two-to-four bachelor's degree attainment as a guide, I find suggestive evidence that improved pre-transfer academic advising may be partly responsible for the treatment effects. In particular, I find that students transferring to four-year programs that are not associated with the reforms also experienced improvement in degree attainment, suggesting that these improvements are not exclusively the results of improved articulation between programs. In addition, I find that the improvement in degree attainment is mostly confined to students who pursue majors that are targeted by the reforms.

While previous studies have examined factors that may affect the graduation rates of two-year and four-year institutions separately (Jacoby, 2006; Weiss et al., 2019; Scott, Bailey, and Kienzl, 2004; Bound, Lovenheim, and Turner, 2010), less is known about solutions to the special challenges faced by two-to-four transfer students. This paper focuses on bachelor's degree attainment through the two-to-four pathway, and seeks to identify policies that can improve the bachelor's degree attainment rate of this important student population.

This paper also brings attention to two increasingly popular policies: institutional upgrades and institutional consolidations. As of 2018, 19 US states allow community colleges to offer bachelor's degree programs, and many states are entering debates on whether they should follow.[3] With regards to institutional consolidations, about 24 mergers or acquisitions took place between 2010 and 2017, compared to only 12 cases in the earlier decade. Some of the benefits of consolidations and upgrades are obvious: they both significantly reduce the operating cost of bachelor's degree programs and may potentially provide students with cheaper options for obtaining bachelor's degrees. However, the policies' effect on student academic outcomes are unclear.

To the best of my knowledge, this paper is the first to provide causal evidence on the positive effects of community college upgrades and college mergers on the outcomes of two-to-four transfer student. The results contribute to the small but growing literature examining the effect of institutional restructuring on student outcomes (Capuccinello and Bradley, 2014; Beuchert, Humlum, Neilsen, and Smith, 2016; Russell, 2019). In qualitative studies, Levin (2004) and McKinney and Morris (2010) examines the challenges and promises of the community college baccalaureate programs. This paper is closely related to Russell (2019), a contemporaneous and independent work that examines the effect of USG's institutional mergers on retention rates and on-time graduation rates. Russell (2019) finds that institutional mergers increase retention rates by 8% and increase on-time graduation rates by 29%. This paper complements Russell's analysis by confirming the institutional mergers' positive effects on student outcomes and by providing empirical evidence to support Russell's hypothesis that consolidations improved student outcomes through improved academic support. I make two additional contributions. First, I examine the effect of another important institutional reform — institutional upgrades. Second, I study these institutional reforms through the lens of two-to-four transfer students, and identify policies that target this historically disadvantaged student population.

[3] Some recent examples include California Senate Bill No. 850 signed in 2014 and the Ohio 2018-2019 state budget.

This study also contributes to the literature on articulation agreements — agreements between institutions that help facilitate smooth transfer of credits. While earlier qualitative studies advocate for the ability of articulation agreements to eliminate credit loss during transfer (Barry and Barry, 1992; Ignash and Townsend, 2001; Kintzer and Wattenbarger, 1985), later empirical studies find mixed results regarding the effects of state-wide articulation agreements. Anderson, Sun, and Alfonso (2006) uses a nationwide survey data to find that state-wide articulation agreements generally do not have positive effects on community college students' transfer rate. However, using a recent statewide articulation policy in Ohio, Boatman and Soliz (2018) finds that the policy improved the transfer rate and credit transfer. This paper contributes to this literature by providing a potential explanation for such mixed results: I argue that without appropriate pre-transfer academic advising, community college students may be choosing courses that are not transferrable even with articulation agreements in place. While many statewide articulation policies only provide a complicated crosswalk for courses that transfer between different institutions, the Ohio Transfer Module policy studied by Boatman and Soliz defines a uniform module that applies to all institutions. This study argues that the simplicity and clarity of course selection matters for the effectiveness of articulation policies.

This paper proceeds as follows. In the next section, I will introduce the data used for the empirical analysis, describe the institutional background pertinent to the empirical strategy, and discuss the external validity of the paper's results. In Section 3, I will discuss the empirical strategy and strategies for robustness checks. In Section 4, I will present the empirical results. In Section 5, I will attempt to decompose the main treatment effects and draw suggestive evidence on the mechanisms. In Section 6, I will draw the policy implications of the study and conclude.

1 The Georgia Administrative Data and Institutional Background

This paper uses administrative data from USG, which is the sixth largest university system in the US. This university system currently has 26 institutions,[4] with more than half of them offering two-year programs. The data has information on all students who entered USG between 2008 and 2015 and have once enrolled in one of USG's two-year programs. Although the panel data is for the subsample of students

[4] USG had around 30 institutions during the years observed in the data. Recent mergers and consolidations brought the total number of institutions to 26.

who have once enrolled in a two-year program, it contains detailed information on these students if and while they are enrolled in USG's four-year programs. The data follows these students until the 2017-2018 academic year.[5]

Out of all students in the data, about 29% transferred into a four-year program in USG. Noticeably, among those who transfer into bachelor's degree programs, 33% transferred into bachelor's degree programs offered by the institutions they pursued their two-year degrees. Conditional on transferring into a bachelor's degree program, the graduation rate is 47%.[6] The average GPA of two-to-four transfer students while in four-year programs is 2.7. In comparison, in the 2012-2013 academic year, students transferring between four-year programs have an average post-transfer GPA of 2.89. Additionally, on average, students lose 11 semester credit hours in the transfer process. These statistics indicate that poor academic performance and loss of credits during transfer may be contributing to the low post-transfer graduation rates. Additional summary statistics of the sample, including length of enrollment, and statistics by student ethnicity, are provided in Table 1.

USG went through two episodes of institutional upgrades in 2013, and four episodes of institutional consolidations in 2013. In 2011, USG granted East Georgia State College (EGSC) and Atlanta Metropolitan State College (AMSC) permission to offer four-year bachelor's degree programs, and both institutions started to accept students into their bachelor's degree programs in the 2012-2013 academic year. Following the upgrades, the sizes of the bachelor's degree programs were kept small: EGSC and AMSC enrolled 370 upper-level students in Spring 2013.[7] The four-year programs offered immediately following the institutional upgrades were limited. EGSC and AMSC both offered Bachelor's of Science in Biology following the upgrades. EGSC additionally offered a Bachelor's Degree in Nursing, while AMSC additionally offered a Bachelor's in Business Administration. These subjects were chosen according to the institutions' strength and the demand of the local labor markets.[8]

In 2013, Waycross College and South Georgia College combined their academic programs and formed the South Georgia State College (SGSC). In the same year, Macon State College and Middle Georgia College merged to form the Middle Georgia State College (MGSC), and Gainesville State College and North Georgia

[5]The data cannot track students who transfer out of state or into private institutions. The sample selection for this paper's analysis, however, is unlikely to be severe. According to the National Student Clearinghouse (Shapiro et al, 2018), 82% of transfer students from two-year institutions transfer within state, and 70% transfer to public institutions.

[6]The data contains students who matriculated in the university system in 2014, who only have four years of college experience. As a result, this graduation rate may be an underestimation of the typical six-year graduation rate measure.

[7]Upper-level enrollment refers to students in their third or fourth year of bachelor's degree programs.

[8]In later years, AMSC expanded its Bachelor's Degree programs to also cover mathematics, criminal justice, and arts.

College & State University merged to form the University of North Georgia. Waycross College and South Georgia College did not offer bachelor's degree programs prior to the consolidations. In comparison, prior to the consolidations, Macon State College had a sizable collection of bachelor's degree programs with an upper-level enrollment of 2,600 students, and Middle Georgia College had a smaller upper-level enrollment of 580 students. Following the consolidations, SGSC and MGSC had upper-level enrollments of 300 and 3200, respectively. Prior to the merger, Gainesville mostly offered two-year programs and North Georgia College & State University offered a large number of four-year programs. In sum, while the consolidations did not result in the development of new academic programs, students in the merged institutions gained access to a larger selection of four-year programs in their new home institutions.[9] The mergers did not result in new campuses being built and all academic programs were consolidated and operated in the previously existing campuses. [10]

A compilation of the degree programs offered by the institutions post consolidations and upgrades can be found in Table 2. The statistics presented suggest that the increase in access to bachelor's degree programs was significantly larger in the consolidated institutions compared to the upgraded institutions. Importantly, students transferring from two-year programs to four-year programs are required to go through the regular transfer application process even if they were applying to programs in the same institution.

For students in any of the affected institutions, the reforms increased the number of bachelor's degree programs offered by their home institutions. Such increase has two potential effects. First, it increases students' access to "well-connected" four-year programs, which potentially reduces both the transition cost and the difficulty of credit transfer. Second, it exposes two-year students to a wider range of four-year programs and faculty, which potentially improves the pre-transfer quality of teaching and academic advising. Students in the consolidated institutions would have access to courses, faculty, and academic advisors from the partnering institutions following the consolidations.[11] Data from the institutional upgrades also show significant improvements in educational expenses and faculty resources. Between the 2013 and 2014 academic year, EGSC and AMSC's total faculty salary and total educational budget per student increased by 20% and 11%

[9] I exclude students enrolled in North Georgia College & State University from the treated sample since they would not have experienced any increased access to four-year programs.

[10] In 2013, Augusta State University and Georgia Health Sciences University merged to form Georgia Regents University (now called Augusta State University). Both prior to and after the merger, Augusta State University, Georgia Health Sciences University, and Georgia Regents University did not regularly offer two-year programs. For this reason, I do not study the Georgia Regents University merger in this paper.

[11] Implementation guidelines from the University System of Georgia indicate that academic departments were tightly consolidated: prior to the mergers, program and curriculum differences were addressed, program offerings were streamlined, and tenure and promotion processes were standardized.

compared to the USG average of 3.6% and 6.9%.

Before proceeding to the discussion of the empirical strategy, it is important to discuss the external validity of the Georgia reforms and, relatedly, whether the estimated effects of the upgrades and consolidations may be contaminated by other contemporaneous policy reforms. While all university systems are unique to a certain level, it is helpful to get a sense of the policies' context using institutional reports available.

The institutional upgrades and consolidations studied in this paper are part of a series of similar institutional reforms initiated by USG. In particular, there were two institutional upgrades before our data observation window, one institutional upgrade after our data observation window, and three institutional consolidations after our data observation window. According to archived news releases from USG, university-system wide policies that happened in the same academic year of the reforms of interest include a national partnership to develop distance learning, the implementation of a learning management system to help students manage college classes, the offering of a few new online bachelor's degrees, and the approval of new articulation agreements with the Technical College System of Georgia. There is no indication that any of these reforms differentially impacted schools that went through consolidations or upgrades. Although unlikely, I cannot rule out any interactions between the institutional reforms and distance learning, online bachelor's degrees, and the learning management systems.

Examining the annual reports of the individual institutions studied, I also find no evidence that there were any major institution-specific policy reforms implemented simultaneously. One initiative that may raise some concern is the partnership between EGSC and Georgia Regents University. The partnership started in the same semester as EGSC's bachelor's degree programs and allows selected students from EGSC to take classes in Georgia Regents University. If the dual enrollment in Georgia Regents University is improving the quality of education during students' two-year programs, the results may be picking up the effect of this partnership. However, as will be shown later, I do not find evidence that the results are driven by improved two-year teaching quality, so it is unlikely that the results are contaminated by the partnership between EGSC and Georgia Regents University.

2 Empirical Strategy

I use difference in differences (DID) to estimate the treatment effects of the institutional consolidations and institutional upgrades. Intuitively, I compare the difference between treated and untreated institutions in the differences between their pre- and post-reform cohorts' outcomes. Important to the empirical strategy, I use two different definitions of cohorts. Exit cohort t refers to the cohort of students that exited (either by transferring up or by leaving college altogether) their two-year programs in year t.[12] Enter cohort t refers to the cohort of students that entered their two-year programs in year t. With two types of reforms and two definitions of cohorts, this paper studies four specific treatments. For $m \in \{\text{upgrade}, \text{consolidation}\}$ and $n \in \{\text{exit}, \text{enter}\}$, I denote the treatment effect of reform m on cohort n as TE_m^n.

The regression I run to estimate the treatment effects on bachelor's degree attainment is:

$$\text{BA\_Degree}_{ijt} = \alpha \text{Treatment}_{jt} + \phi_1 \text{College}_j + \phi_2 \text{Cohort}_t + Q'_{jt}\beta + X'_{ijt}\gamma + \eta_{jt} + \epsilon_{ijt} \tag{1}$$

where each observation is for the i-th individual, from two-year program j, of cohort t. BA_Degree_{ijt} is a dummy for bachelor's degree attainment; Treatment_{jt} is an indicator for program j being post reform m in year t, and College_j is a dummy for institution j. Cohort_t is the student's (exit or enter) cohort. In addition, Q_{jt} and X_{ijt} are vectors of controls for the students' cohort, and for the individual students, and η_{jt} and ϵ_{ijt} are error terms. The main parameter of interest is α.[13]

In all regressions, I include students' high school GPA, gender, age at matriculation, ethnicity, and first-generation college status as controls.[14] For regressions that condition on students that transferred to four-year programs, I also include fixed effects for students' BA institution. Throughout the main analysis, I use whether a student *ever* obtained a bachelor's degree as the definition for bachelor's degree attainment. I do not use the standard six-year graduation measure because many of the treated cohorts has not been enrolled in college for more than six-years. I rely on the cohort fixed effect to eliminate the effect of control groups having longer enrollment than treatment groups. In robustness checks, I implement regressions that use an alternative measure of degree attainment — whether a student obtains a bachelor's degree within

[12] The definition of exit cohort would necessarily include students who drop out of college, and therefore are unlikely to transfer to a four-year program. Ideally, the exit cohorts would target the subgroup of students who leave college with an intention of transferring to a four-year college. However, it is not possible to distinguish between students who intend to transfer without a two-year degree and those who simply drop out of community college. As a robustness check, I conduct analyses of exit cohorts by restricting the sample to students who completed their study in their two-year programs and earned a two-year degree. This, however, leads to severe data attrition, since more than 60% of students who transferred to four-year programs have not earned an Associate's Degree. The robustness checks, presented in the Appendix suggest that this sample restriction does not change the results of the analyses. I thank an anonymous referee for pointing out this robustness check.

[13] To reduce the notational burden, I leave implicit the fact that the variables, parameters and error terms of equation (1) all depend on the type of treatment and the definition of cohorts. For example, to be complete, we should have $\alpha^{m,n}$ instead of α. The same would apply to equation (2) below.

[14] I do not control for SAT scores because standardized test scores are not required for admission to two-year programs, and it is not uncommon for students to not have taken the SAT or ACT even though they have completed a bachelor's degree through the two-to-four pathway. In my sample, less than 50% of students have reported SAT scores.

three years of transferring — and the results will be reported in the Appendix. As expected, the results do not differ much from the main difference in differences specifications with cohort fixed effects.

I run four separate regressions in the form of equation (1) to estimate the four treatment effects. For the Upgrade-Exit (Enter) treatment, I define Cohort_t using students' Exit (Enter) cohort and exclude institutions that went through consolidations from the control group. For the Consolidation-Exit (Enter) treatment, I use students' Exit (Enter) cohort to define the dummy Cohort_t, and exclude institutions that went through upgrades from the control group. For the specifications using enter-cohorts, I drop students from the treated institutions who are in the two cohorts immediately prior to the reform. This is because these students will also be partially treated by the reform, given that most students stay more than one year in their two-year programs.[15]

To estimate the reforms' effects on four-year program graduation rate conditional on transfer, I restrict the sample to the subset of students who transferred to a four-year program while running regression (1). To study the reforms' effects on transfer rate, I substitute the dependent variable of equation (1) with an indicator for whether a student ever enrolled in a bachelor's degree program.

In the baseline estimation, I assume that η_{jt} is independent of the treatment, and ϵ_{ijt} is i.i.d., so that $\alpha^{m,n}$ is a consistent estimate of TE^n_m. To verify the appropriateness of the assumptions, I present the time-series trends of the main outcome variable, overall bachelor's degree attainment rates, in Figure 1. From the figure, we see that there is no clear differences in pre-trends in the bachelor's degree attainment rates for both types of reforms. Additional evidence supporting this assumption can be found in the event study analysis, which will be discussed later.

Besides differential pre-trends, there is the additional concern of endogenous sample selection and change in student composition due to the reforms. A priori, the extent of this concern is expected to be small. This is because students attending two-year programs typically choose the programs that are closest to their residence. Nevertheless, I check for change in student composition by documenting the trends of three pre-college characteristics of students in the schools that went through reforms in 2013 and in schools that did not. Specifically, I look at the average high school GPA of students and the share of minority students. As shown in Figure 2, there is no evidence of differential trends in the compositional characteristics. Regardless, I use an instrumental variable approach to further control for unobservable student characteristics that may cause undetected composition effects. This method and its results can be found in

[15] I thank the referees and the associate editor for this suggestion.

the Appendix.

To estimate the treatment effects on intermediate outcomes, I run regressions as in equation (1), but with the intermediate outcomes as the dependent variables. I examine three intermediate outcomes: (1) GPA in four-year program; (2) credits lost during transfer; and (3) an indicator for switching major after transferring.

Given the relative lack of informal support from immediate social network, students from under-represented backgrounds may be more susceptible to the lack of academic advising and structure in two-year programs (Swecker, Fifolt, and Searby, 2013). Similarly, students with lower baseline academic aptitude may also suffer more from a lack of coaching, and would therefore benefit more from an improvement in advising. Motivated by these possibilities, I run regressions that interact the treatment indicator with student characteristics, including race and academic ability. Denoting the interacting characteristic as W_{ijt}, the empirical specification is:

$$\begin{aligned}\text{BA\_Degree}_{ijt} = {} & \alpha_1 \text{Treatment}_{jt} + \alpha_2 \text{Treatment} * W_{ijt} + \phi_1 \text{College}_j + \phi_2 \text{Cohort}_t \\ & + Q'_{jt}\beta + X'_{ijt}\gamma_1 + \gamma_2 W_{ijt} + \eta_{jt} + \epsilon_{ijt}\end{aligned} \tag{2}$$

A complication in the estimation is that the sample is inherently clustered. Specifically, due to unobservable characteristics of the programs, the error term ϵ_{ijt} may be correlated among students who enroll in the same two-year program. Disregarding this potential correlation in error structure may lead to severe underestimation of the coefficients' standard errors (Campbell, 1977; and Greenwald, 1983). An additional complication in our setting is the small number of clusters in the data — USG only consists of around 30 institutions. With a small number of cluster and even smaller number of treated clusters, the standard cluster-robust standard error calculations may lead to unreliable estimates (Conley and Taber, 2005). To deal with this issue, I impose parametric structure on the error term, and assume that the errors are equicorrelated within cluster. I estimate this random effect structure using maximum likelihood, and use the estimated error structure to perform feasible GLS (FGLS). [16] In the appendix, I replicate the main regressions using White's robust standard errors, and the results are similar to those reported in the main texts.

To examine the temporal change in treatment effects, I implement an event study, where I separately estimate the treatment effects of both upgrades and consolidations on the first, second, and third cohort of

[16] Another popular method for cluster robust inference is the wild cluster bootstrap of Cameron, Gelbach, and Miller (2011). I do not proceed with this method because the wild cluster bootstrap is known to be unreliable when the number of treated cluster is really small (Mackinnon and Webb, 2015).

students post reform as well as on the last untreated cohort of students pre reform.[17] In particular, in lieu of the specification described in equation (1), I implement the following regression:

$$\begin{aligned}\text{BA\_Degree}_{ijt} &= \alpha_1\text{Treatment\_1}_{jt} + \alpha_2\text{Treatment\_2}_{jt} + \alpha_3\text{Treatment\_3}_{jt} + \alpha_4\text{Treatment\_Minus3}_{jt} \\ &+ \phi_1\text{College}_j + \phi_2\text{Cohort}_t + Q'_{jt}\beta + X'_{ijt}\gamma_1 + \gamma_2 W_{ijt} + \eta_{jt} + \epsilon_{ijt}\end{aligned} \quad (3)$$

where Treatment_1_{jt}, for example, is an indicator for whether cohort jt is the first cohort of students after institution j went through a reform. I examine the treatment effects on overall BA attainment, transfer rate, and graduation rate of transfer students using this specification. In addition to examining the temporal change in treatment effects, regressions following equation (3) can also help examine the assumption of no differential pre-trends. If the assumption holds, we expect α_4 to be non-positive.

3 Results

To present the results of the empirical analysis, I first discuss the overall treatment effects of the institutional reforms on overall bachelor's degree attainment, transfer rate, and post-transfer graduation rates. Then, I present the treatment effects on intermediate outcomes, the heterogeneity results, and results from the event study analysis.

3.1 Main Estimation Results

I first document the effects of the upgrades and consolidations on bachelor's degree attainment, transfer graduation rates, and transfer rates. For each of these outcomes, I run four separate regressions following equation (1) to estimate $\text{TE}^{exit}_{upgrade}$, $\text{TE}^{enter}_{upgrade}$, $\text{TE}^{exit}_{consol}$, and $\text{TE}^{enter}_{consol}$. The results of the estimates are presented in Tables 3, and 4.

As shown in Table 3, both institutional upgrades and institutional consolidations had a sizable positive impact on bachelor's degree attainment for two-year students. Institutional upgrades increased the bachelor's degree attainment rate of enter cohorts by 3.4 percentage points while institutional consolidations increased the bachelor's degree attainment of enter cohorts by 2.8 percentage points. In contrary, the effect of institutional upgrades and consolidations on exit cohorts are not statistically different from 0. As will be

[17] Since students typically enroll in two-year programs for two years, I choose the T-3 cohort of students as the last untreated cohort of students pre reform

further explored in Section 5, the differences between the effects on enter cohorts and exit cohorts suggest that the reforms improved students' pre-transfer experience in two-year programs, but had no significant impact on students' experience during and after transfer.

The positive effects of the reforms on bachelor's degree attainment may come from two different channels. On the one hand, it may be that the reforms encourage students to transfer from two-year programs to four-year programs. On the other hand, the reforms may also improve graduation rate of students conditional on them making the transfer.

In Panel A of Table 4, I show that, both upgrades and consolidations increased the two-to-four transfer rates for both exit cohort and enter cohort students. The effect of consolidation on exit cohorts' transfer rate, however, is not statistically significant. In particular, the upgrades increased the transfer rate of both exit and enter cohorts by around 3.5 percentage points, and the consolidations increased the transfer rate of enter cohorts by 7.3 percentage points. These improvements represent a 11% and 23% increase, respectively. In Panel B of Table 4, I show that, conditional on transfer, the institutional consolidations increased the BA degree attainment rate by 10.3 percentage points, which represents a 22% improvement. The point estimates suggest that upgrades also increased the BA degree attainment conditional on transfer, although the estimates are not statistically significant.

3.2 Intermediate Outcomes, Heterogeneity, and Event Study Results

Next, I discuss how results on intermediate outcomes help decompose the above treatment effects. I run regressions on intermediate outcomes and the results are presented in Table 5. Panel A, B, and C of the table presents the results on credit lost during transfer, GPA during bachelor's degree study, and probability of changing major post-transfer. The results suggest that the only intermediate outcome that appears to be positively affected is credit lost. Both institutional upgrades and consolidations reduced loss of credits of enter cohorts during transfer by around 4 semester credit hours, which represents a 36% reduction. The lack of significant effect on exit cohorts' credit lost suggests that improved credit articulation procedure is likely not the main mechanism through which the reforms improve student outcomes. The reduction in GPA during four-year program, as shown in Panel B of Table 5, implicates that better teaching quality in two-year programs is also unlikely to be the key margin of improvement during the reforms.

A natural question to ask is whether the reduction of credits lost came in the form of general education credits or major-specific credits. Although the data does not include individual course-level credit transfer information, I observe the students' major choices, and I test whether the reforms had any effect on the probability that students switch major during their bachelor's degree programs. The intuition is that a student is more likely to change major after transfer if she realizes that many of her major-specific credits did not transfer. Results in Panel C of Table 5 show that the reforms do not have significant effects on whether students change their major after transferring.

I also study the heterogeneity of the various treatment effects by student characteristics. I run regressions as specified in equation (2), with the interacted characteristics being minority status and high-school GPA.[18] The results of these exercises are presented in Table 6. Columns (1) and (3) show that minority students benefit significantly more from both institutional upgrades (5.8 percentage points) and consolidations (3.9 percentage points). Columns (2) and (4) show that students with lower high school GPA benefit more from both upgrades and consolidations — a one-point increase in high school GPA reduces the treatment effect of upgrades by 3.3 percentage points and that of consolidations by 3.9 percentage points. The coefficient estimate in column (2), however, is not statistically significant with a p-value of 0.15. These heterogeneity results suggest that institutional upgrades and consolidations have large potential in improving degree completion rates of minority and underprepared students.

Finally, the results from the event study analysis are presented in Table 7. The results show consistent treatment effects of both upgrades and consolidations on enter cohorts. Moreover, the small and statistically insignificant estimates on the pre-reform cohorts further support the assumption of no differential pre-trends.

4 Mechanisms

In this section, I use a theoretical model of transfer graduation to guide the decomposition of the treatment effects found in the previous section. Undoubtedly, there may be other factors beside those highlighted in the model that may underly the treatment effects and the model is based on several additional assumptions. I choose the most salient factors under this context and use this model to guide the decomposition of the treatment effects as much as the data allows. Results from this section should be therefore taken as exploratory and offering suggestive evidence.

[18] Students of American Indian, Alaska Native, Black, Hispanic, and Native Hawaiian ethnicity are categorized as racial minority.

4.1 Theoretical Model of Transfer Graduation

Let our main outcome of interest, bachelor's degree attainment of 2-4 transfer students, y, be a function of credits accumulated, h, grades, g, and transition cost during transfer, c, so that $y = y(h, g, c)$. Furthermore, suppose credits accumulated, h, is a function of the quality of the credit-transfer system, a, and the quality of pre-transfer academic mentoring, m, so that $h = h(a, m)$. Finally, let grade in four-year programs be a function of the quality of teaching before transfer, q, so that $g = g(q)$.

Following the above specification, we can write degree attainment as:

$$y = y(h(a, m), g(q), c) = y(a, m, q, c) \tag{4}$$

Let us first consider the components of the treatment effect of upgrades on exit cohorts, $\text{TE}^{exit}_{upgrade}$. The treatment group is the first cohorts of students that exited a two-year program after it went through an upgrade. Students in these cohorts were the first to have the opportunity to transfer to their home institution's four-year programs. The effect of this opportunity are two-folds. First, transferring to a program in the same institution removed institutional barriers to credit transfer. Second, transition costs decreased as students no longer needed to relocate to study in four-year programs. As a result, the treatment effect of upgrades on exit-cohort students can be written as:

$$\text{TE}^{exit}_{up} = y_a * da + y_c * dc$$

where da and dc are the changes in the quality of the credit-transfer system and the changes in transition costs as a result of the upgrades.

Now let us consider $\text{TE}^{enter}_{upgrade}$. The treatment group is the first cohorts of students that entered a two-year program after it went through an upgrade. Students in these cohorts not only had the chance to transfer to four-year programs in the same institution, but also were the first cohorts to fully benefit from the increased exposure to four-year programs during their two-year study. This increased the amount of institutional knowledge in the two-year programs, which potentially led to improved pre-transfer advising and teaching quality. Therefore, $\text{TE}^{enter}_{upgrade}$ can be written as:

$$\text{TE}^{enter}_{up} = y_a * da + y_c * dc + y_m * dm + y_q * dq$$

where dm and dq are the changes in the quality of academic advising and the quality of teaching due to the institutional upgrades.

Institutional consolidations also increased the opportunity for students to transfer into four-year programs in their home institutions, and increased two-year programs' exposure to four-year programs. Therefore, similar to the case with institutional upgrades, the treatment effects can be written as:

$$\text{TE}^{exit}_{consol} = y_a * da^* + y_c * dc^*$$

$$\text{TE}^{enter}_{consol} = y_a * da^* + y_c * dc^* + y_m * dm^* + y_q * dq^*$$

where da^*, dc^*, dm^*, and dq^* are the changes in the quality of the credit transfer system, transition costs, academic advising, and teaching quality due to institutional consolidations.

We can draw two main results from this theoretical model:

Proposition 1: $\text{TE}^{exit}_{upgrade}$ and$\text{TE}^{exit}_{consol}$ both capture the combination of smoother credit transfer and lower transition cost, while $\text{TE}^{enter}_{upgrade}$ and $\text{TE}^{enter}_{consol}$ both identify the combination of smoother credit transfer, lower transition cost, improved teaching quality and academic advising. However, there are two differences between the treatment effects of consolidations and upgrades. First, as documented in Section 2, the increase in the number of four-year programs were significantly larger in the consolidated institutions compared to the upgraded institutions, and some of the consolidated institutions had a history of offering four-year programs prior to the reforms. As a result, we should expect the improvements in academic advising and teaching quality to be more pronounced during consolidations. Second, although institutions merged their academic programs during the consolidations, the physical distance between the campuses did not change. Therefore, the change in transition costs should be less significant for the consolidated schools.

Proposition 2: From equation (3), we can see that $y_a = y_h * h_a$, so that the quality of the credit transfer system affects graduation rates through credit accumulation. In other words, if smoother credit transfer increases graduation rate, it should also increase credit accumulation. Equation (3) also implies that $y_m = y_h * h_m$ and $y_q = y_g * g_q$. In other words, academic advising affects graduation rates through credit accumulation, and teaching quality affects graduation rates through grades. Therefore, if academic advising increases graduation rate, it must also increase credit accumulation, and, similarly, if teaching quality increases graduation rate, it must also increase post-transfer grades.

4.2 Suggestive Evidence on Mechanism

Recall that in section 4, the only treatment-cohort combination that exhibited significant improvement in transfer graduation rate is the consolidation-enter combination. Following "Proposition 1" above, this points towards a combination of smoother credit transfer, lower transition cost, improved teaching quality, and academic advising. Looking at the treatment effects on intermediate outcomes, I find significant reduction in credit lost but do not find significant improvements in students' post-transfer GPA. Following "Proposition 2" above, this suggests that the treatment effects is likely due to either improved credit articulation or improved academic advising.

To distinguish between these two mechanisms, I further examine how the treatment effects depend on students' pre-transfer major and their transfer destinations. First, improved academic advising would only significantly affect students who pursue majors that are targeted by the reforms.[19] For example, EGSC offered bachelor's degree in Biology and Nursing after its upgrade, and two-year program students in EGSC were exposed to faculty and advisors that have more knowledge on bachelor's degree programs. However, an EGSC student who majors in Economics would not benefit from this improvement in academic advising and teaching quality, since there is no faculty and advisors with the appropriate background in EGSC. Second, improved credit transferring would only significantly benefit students who transfer between programs within the same institution. For example, transferring between Macon State College and Middle Georgia College became significantly easier since the consolidation of these two colleges into Middle Georgia State College. However, a student who transfers from the newly formed Middle Georgia State College to Georgia State University would face just as much institutional frictions as pre-consolidation students.

Column 1 of Table 8 shows that while the treatment effect on the consolidation-enter combination is most salient for students who transferred to four-year programs in their home institutions, the treatment effects were also significant for those who transferred to four-year programs outside of their home institutions. Moreover, Column 2 of Table 8 shows that the treatment effect was more significant for students who pursued majors targeted by the consolidations. These results suggest that it is unlikely that improved articulation is the only cause of the treatment effects, and that improved academic advising is a likely mechanism of the policy effects. However, the significant treatment effect on students who do not pursue majors

[19] A concern in studying the heterogeneity with respect to student majors is that students' major choice may be affected by the reforms. I test this concern by implementing equation (2) with an indicator of student choosing a major affected by the reforms as dependent variable. The results of the test suggest that students' major choices are not affected by the reforms.

targeted by the consolidations suggests that there may be other factors affecting the reduced loss of credits. One potential mechanism is that students narrow their choice set for four-year programs following the consolidations, which may lead to fewer courses that do not articulate. Another potential mechanism that reduces the loss of credits during transfer is that students may be able to enroll in previously full classes.[20] I cannot rule out these mechanisms with the data available, and they are interesting areas for future research.

5 Conclusions and Policy Implications

This study uses two recent reforms in the University System of Georgia to study policies that may improve graduation rates of two-to-four transfer students. I find that both consolidations and upgrades can have significant positive effects on two-year students' bachelor's degree attainment, bachelor's degree graduation rate conditional on transferring, and two-to-four transfer rate. These results are encouraging for institutions seeking mergers as well as for the increasingly popular policy proposal to grant community colleges permission to offer bachelor's degrees. However, there are two important caveats. First, the upgraded institutions in USG started off by offering a limited number of bachelor's degrees that are related to their strength. Hence, the results should not be taken as support for reforms to make community colleges substitutes of four-year colleges. Second, one of the central purposes of the consolidations studied here was to promote inter-institutional transfers. Therefore, the results may not apply to mergers designed purely for financial relief.

I find evidence that reducing credit lost during transfer is key to improving bachelor's degree attainment through the two-to-four pathway. I also find suggestive evidence that improved academic advising likely drove this reduction in credits lost during transfer. These results suggest that academic advising in two-year programs may be a complementary tool to improving transfer outcomes. While policy makers have long been aware of significant loss of credits during transfer, most of the policy initiatives have been focused on articulation agreements. This study suggests that, although it is important to make sure transferrable credits are being transferred, it is also important to ensure that students in two-year programs are taking transferrable courses. In addition, an interesting heterogeneity result is that the treatment effects are more salient for students with lower high-school GPA as well as for minority students. In an environment with limited

[20] I thank an anonymous referee for pointing out these potential mechanisms.

resources, policy should be designed to allocate more attention to underprepared and under-represented students.

6 Tables and Figures in Chapter 1

Table 1: Summary Statistics on Georgia Student Transfer

Ethnicity	Share	Transfer Rate	BA Obtainment	Enrollment if BA Degree (Years)	BA Enrollment if BA Degree (Years)	AA Enrollment if Transferred (Years)	Credit Lost
Asian	3.84%	38.73%	18.14%	5.10	3.39	1.70	15.01
African American	36.05%	19.66%	6.72%	5.11	3.51	1.60	12.32
Hispanic or Latino	6.98%	30.86%	12.04%	5.04	3.27	1.77	22.24
White	47.92%	34.77%	16.99%	4.70	3.22	1.48	15.83
Average	-	28.80%	12.71%	5.84	3.30	1.54	15.32

Notes: summary statistics on students transfer rate, overall Bachelor's Degree attainment, length of enrollment, and credit losts during transfer are presented. The statistics are also separately presented for students from different ethnic backgrounds — Asian, African American, Hispanic or Latino, and White. "Transfer Rate" refers to the share of two-year program freshmen who have eventually transferred to four-year programs. "BA attainment" calculates the share of two-year program freshmen who have eventually earned a four-year Bachelor's Degree. "Enrollment if BA Degree (Years)" and "BA Enrollment if BA Degree (Years)" measure the average total length of enrollment (in two-year programs + in four-year programs) and the average length of enrollment in four-year programs for the subgroup of students that eventually earned a four-year Bachelor's Degree. "AA Enrollment if Transferred (Years)" captures the average length of enrollment in two-year programs for the subgroup of students who have eventually transferred to four-year programs. Finally, credit lost measures the average semester credit hours of credits lost during transfer for the subgroup of students who have transferred to four-year programs.

Table 2: Majors Offered by Georgia Institutions Pre- and Post- Reforms

	Pre-Reform Majors	Post-Reform Majors
Consolidation School:		
South Georgia State College (New)		26, 51, 52
South Georgia College	NA	
Waycross College	NA	
Middle Georgia State College (New)		9, 11, 13, 15, 23, 24, 26, 27, 42, 43, 44, 49, 51, 52, 54
Middle Georgia College	13, 26, 43, 49	
Macon State College	9, 11, 13, 23, 24, 26, 27, 42, 44, 51, 52, 54	
University of North Georgia (New)		3, 9, 11, 13, 16, 23, 24, 26, 27, 31, 40, 42, 43, 44, 45, 50, 51, 52, 54
North Georgia College & State University	11, 13, 16, 23, 24, 26, 27, 40, 42, 43, 45, 50, 51, 52, 54	
Gainesville State College	3, 13, 26, 42, 44, 50, 52	
Upgrade School:		
East Georgia State College	NA	26, 51
Atlanta Metropolitan State College	NA	26, 27, 43, 50, 52

Note: this table presents the two-digit classification of instruction programs (CIP) codes for the fields in which institutions affected by the reforms offer four-year Bachelor's Degree programs both before and after reforms. Fields available post-reform reflect the availability of four-year programs in the 2017-2018 academic year and fields available pre-reform reflect the availability of programs in the pre-reform academic year. Interested readers can find a tabulation of the two-digit CIP codes in the Online Appendix.

Table 3: Baseline effect of reforms on Overall BA attainment for full sample

	(1)	(2)	(3)	(4)
	Overall BA Obtainment			
	Upgrade Exit	Upgrade Enter	Consol Exit	Consol Enter
Treatment	0.018	0.034***	0.001	0.028*
	(0.012)	(0.009)	(0.010)	(0.015)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observations	50,603	53,435	66,084	66,292

Note: this table presents the baseline difference in differences estimates for the effect of the reforms on BA attainment rate for the full sample of students who enrolled in two-year programs. Feasible GLS estimates are presented and the cluster sensitive standard errors are reported in parentheses. The significance level convention is: * for 0.10, ** for 0.05, and *** for 0.01. The variable "Treatment" is a dummy that takes value one if the individual is in post-treatment cohorts and in a treatment school. "Demographic" control variables include, race and ethnicity fixed effects, age at matriculation, gender, and an indicator for first-generation college students. The sample size in each column are different for two reasons: 1. students from institutions that went through upgrades (consolidations) are excluded in the estimation of the effects of consolidations (upgrades), and so the number of excluded students are different for the estimation of different treatment effects; 2. the different definitions of enter and exit cohorts lead to different sample sizes available for these cohorts.

Table 4: Baseline effect of reforms on Transfer Rate and BA attainment for transferred sample

	(1)	(2)	(3)	(4)
	Upgrade Exit	Upgrade Enter	Consol Exit	Consol Enter
Panel A:	Transfer			
Transition	0.034**	0.035***	0.016	0.073***
	(0.016)	(0.013	(0.013)	(0.011)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observation	50,603	53,435	66,084	66,292

Panel B:	BA Degree for Transfer Students			
Transition	0.027	0.006	0.028	0.103***
	(0.036)	(0.029)	(0.023)	(0.019)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA and BA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observation	13,548	14,685	19,954	20,350

Note: this table presents the baseline difference in differences estimates for the effect of the reforms on two-to-four transfer rates for the full sample of students enrolled in two-year programs, and on BA attainment rate for the subgroup of students who have transferred to four-year programs. Feasible GLS estimates are presented and the cluster sensitive standard errors are reported in parentheses. The significance level convention is: * for 0.10, ** for 0.05, and *** for 0.01. The variable "Treatment" is a dummy that takes value one if the individual is in post-treatment cohorts and in a treatment school. "Demographic" control variables include, race and ethnicity fixed effects, age at matriculation, gender, and an indicator for first-generation college students. The sample size in each column are different for two reasons: 1. students from institutions that went through upgrades (consolidations) are excluded in the estimation of the effects of consolidations (upgrades), and so the number of excluded students are different for the estimation of different treatment effects; 2. the different definitions of enter and exit cohorts lead to different sample sizes available for these cohorts.

Table 5: Treatment Effects on Intermediate Outcomes

	(1)	(2)	(3)	(4)
	Upgrade Exit	Upgrade Enter	Consol Exit	Consol Enter
Panel A:		Credit Lost		
Treatment	-0.970	-3.870***	1.003	-4.376***
	(1.412)	(1.211)	(0.948)	(0.837)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA and BA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observation	13,124	14,193	19,403	19,765
Panel B:		BA GPA		
Treatment	-0.023	-0.230***	-0.048	-0.095**
	(0.075)	(0.061)	(0.045)	(0.039)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA and BA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observation	13,124	14,193	19,403	19,765
Panel C:		Change Major		
Treatment	-0.034	-0.012	0.011	0.004
	(0.029)	(0.023)	(0.019)	(0.015)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA and BA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observation	13,124	14,193	19,403	19,765

Note: this table presents the difference in differences estimates for the effect of the reforms on intermediate student outcomes for the subgroup of students who have transferred to four-year programs. “Credit Lost” measures the lost of semester credit hours during transfer, “BA GPA” measures the average GPA of students while enrolled in four-year programs, and “Change Major” measures whether students change majors before and after transfer. Feasible GLS estimates are presented and the cluster sensitive standard errors are reported in parentheses. The significance level convention is: * for 0.10, ** for 0.05, and *** for 0.01. The variable “Treatment” is a dummy that takes value one if the individual is in post-treatment cohorts and in a treatment school. “Demographic” control variables include, race and ethnicity fixed effects, age at matriculation, gender, and an indicator for first-generation college students. The sample size in each column are different for two reasons: 1. students from institutions that went through upgrades (consolidations) are excluded in the estimation of the effects of consolidations (upgrades), and so the number of excluded students are different for the estimation of different treatment effects; 2. the different definitions of enter and exit cohorts lead to different sample sizes available for these cohorts.

Table 6: Heterogeneity in Treatment Effects by Minority Status and High School GPA

	(1)	(2)	(3)	(4)
	BA Degree			
	Upgrade Enter		Consolidation Enter	
Treatment	-0.000	0.174***	0.005	0.179***
	(0.012)	(0.027)	(0.010)	(0.032)
Treatment*Minority	0.030***	-	0.046***	-
	(0.010)		(0.012)	
Treatment*HS GPA	-	-0.051***	-	-0.053***
		(0.009)		(0.011)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA School FE	Yes	Yes	Yes	Yes
BA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observation	53,435	53,435	66,292	66,292

Note: this table presents the difference in differences estimation on BA degree attainment when looking at the treatment effect of the Upgrade-Enter treatment and the Consolidation-Enter treatment. Interaction terms are added in this specification to look at the heterogeneity of treatment effects across race and high school GPA. Feasible GLS estimates are presented and the cluster sensitive standard errors are reported in parentheses. The significance level convention is: * for 0.10, ** for 0.05, and *** for 0.01. The variable "Treatment" is a dummy that takes value one if the individual is in post-treatment cohorts and in a treatment school. "Minority" is an indicator of racial minority status, and "Treatment*Minority" is the interaction term between treatment and minority status. The sample size in each column are different because students from institutions that went through upgrades (consolidations) are excluded in the estimation of the effects of consolidations (upgrades), and so the number of excluded students are different for the estimation of different treatment effects.

Table 7: Event Study Analysis on Georgia Reforms

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall BA		Transfer		BA for Transfer Student	
	Upgrade Enter	Consol Enter	Upgrade Enter	Consol Enter	Upgrade Enter	Consol Enter
Treatment	0.046***	0.037***	0.027*	0.076***	0.047	0.082***
(Year 1)	(0.013)	(0.012)	(0.015)	(0.016)	(0.038)	(0.027)
Treatment	0.027**	0.033**	0.038**	0.108***	-0.017	0.103***
(Year 2)	(0.013)	(0.014)	(0.018)	(0.018)	(0.038)	(0.029)
Treatment	0.052***	0.038***	0.061***	0.134***	0.060	0.101***
(Year 3)	(0.013)	(0.013)	(0.018)	(0.017)	(0.037)	(0.028)
Treatment	0.018	-0.008	-0.000	0.004	0.066	-0.027
(Pre Reform)	(0.014)	(0.011)	(0.019)	(0.014)	(0.044)	(0.024)
HS GPA	Yes	Yes	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes	Yes	Yes
AA School FE	Yes	Yes	Yes	Yes	Yes	Yes
BA School FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	53,435	66,292	53,435	66,292	14,685	20,350

Notes: this table presents difference in differences estimates for the time-varying treatment effects on overall BA degree attainment, transfer rate, and BA attainment for transfer students. The upgrade-enter and consol-enter treatment-cohort combinations are considered. Cluster robust standard errors calculated using feasible GLS are reported in parentheses, and the significance level convention is: * for 0.10, ** for 0.05, and *** for 0.01. The variable "Treatment" is a dummy that takes value one if the individual is in the post-treatment cohort and in a treatment school. The sample size in each column are different for two reasons: 1. students from institutions that went through upgrades (consolidations) are excluded in the estimation of the effects of consolidations (upgrades), and so the number of excluded students are different for the estimation of different treatment effects; 2. the different definitions of enter and exit cohorts lead to different sample sizes available for these cohorts.

Table 8: Heterogeneity by Transfer Destination and Major

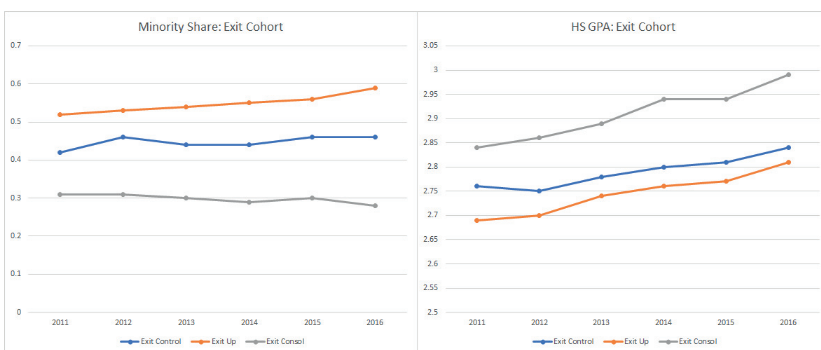
	(1)	(2)
	BA Degree for Transfer Students	
	Consolidation Enter	
Treatment	0.066***	0.062***
	(0.023)	(0.022)
Treatment* Major	0.085***	-
	(0.029)	
Treatment* School	-	0.158***
		(0.038)
HS GPA	Yes	Yes
Demographic	Yes	Yes
AA School FE	Yes	Yes
Cohort FE	Yes	Yes
Observations	19,765	19,765

Note: this table presents the baseline difference in differences estimates for the effect of the reforms on BA degree attainment and analyze the heterogeneity by transfer destination and by students' major in two-year programs. "Treatment*School" is an interaction term that interacts the treatment indicator with an indicator on whether the student transfers to one of the four-year programs offered by their home institution. "Treatment*Major" is an interaction term that interacts the treatment indicator with an indicator on whether the student majored in one of the fields affected by the reforms, e.g. Biology for East Georgia State College or Business for South Georgia State College. Feasible GLS estimates are presented and the cluster sensitive standard errors are reported in parentheses. The significance level convention is: * for 0.10, ** for 0.05, and *** for 0.01. The variable "Treatment" is a dummy that takes value one if the individual is in post-treatment cohorts and in a treatment school. "Demographic" control variables include, race and ethnicity fixed effects, age at matriculation, gender, and an indicator for first-generation college students.

Figure 1: No differential pre-trends in graduation rates

Note: The above figures compare the trends of the bachelor's degree attainment rates between the treatment groups and control group around the time of transitions. The horizontal axis denotes the year students transferred out of the institutions and the vertical axis denotes the graduation rates. The red bar in each graph indicates the time the transitions occurred.

Figure 2: No Composition Effect



Note: This figure presents the trends in minority share and average high school GPA at matriculation across exit cohorts for schools that went through upgrades and consolidation, as well as for schools in the control group.

7 Appendix to Chapter 1: Robustness Checks

Instrumental Variables Method

I present the instrumental variable approach I take to check the results' robustness to the existence of pre-trends and endogenous sample selection. The approach is a version of the instrumental variable method introduced in Freyaldenhoven, Hansen, and Shapiro (2019).

One common critique against the diff-in-diff method is that eye-balling the pre-trends can at most be suggestive evidence that the treatment is independent of the institution-cohort level error term, η_{jt}. In our context, another main concern is that the student compositions of the two-year programs may change as a result to the reforms, even after controlling for observables.

This instrumental variable method relaxes the assumption that η_{jt} is independent of the treatments, and allows for the existence of differential pre-trends and endogenous student composition change. To control for η_{jt}, I leverage an observable measure of student ability: the three-period-lagged average high-school GPA of students in institution j, HS_GPA_{jt-3}. I assume that this measure is correlated with the current period average unobservable ability of students, η_{jt}, and allow the measure to be correlated with the control variables, Q_{jt}. That is, this measure is generated from the following equation:

$$\text{HS\_GPA}_{jt-3} = Q'_{jt}\psi + \delta\eta_{jt} + \nu_j + u_{jt} \tag{5}$$

The key assumption is that u_{jt} is independent of Treatment_{jt}, so that reform in period t does not have any causal effect on the average high school GPA of the period $t-3$ cohort. I argue that this assumption is reasonable since we do not expect high school graduates to base their two-year program choices on reforms three years into the future. Moreover, all reforms studied in this paper were announced one to two years prior to their implementation, making such anticipatory school choice impossible. Solving for η_{jt} in equation (4) and substituting into equation (1), we get that:

$$\begin{aligned}\text{BA\_Degree}_{ijt} = {} & \alpha\text{Treatment}_{jt} + \phi_1\text{College}_j + \phi_2\text{Cohort}_t + Q'_{jt}(\beta - \frac{1}{\delta}\psi) + X'_{ijt}\gamma \\ & + \frac{1}{\delta}\text{HS\_GPA}_{jt-3} - \frac{1}{\delta}\nu_j - \frac{1}{\delta}u_{jt} + \epsilon_{ijt}\end{aligned} \tag{6}$$

Notice that since I assumed that treatment is independent of u_{jt} and ϵ_{ijt}, Treatment$_{jt}$ is no longer endogenous.

The only remaining issue is that HS_GPA$_{jt-3}$ is, by definition, correlated with u_{jt}. To solve this endogeneity, I use a one-period lead of the treatment, Treatment$_{jt+1}$ as an instrument for HS_GPA$_{jt-3}$. Treatment$_{jt+1}$ is a dummy that is equal to one when the $t+1$ cohort of students in institution j is treated by the reforms of interest. If the period $t-3$ student composition is not responsive to the period t treatment, it must also not be responsive to the period $t+1$ treatment. Therefore, the one-period lead of the treatment is independent of u_{jt} and ϵ_{ijt}. Hence the exclusion restriction of the instrument is satisfied. To the extent that we are allowing Treatment$_{jt}$ to be correlated with η_{jt}, Treatment$_{jt+1}$ will also be correlated with η_{jt}, and therefore with HS_GPA$_{jt-3}$. The rank condition for the instrumental variable method is therefore also satisfied. In some sense, the instrumental variable method is different from the standard instrumental variable model where identification requires the instrument to be independent from the confounding error term in the main causal model. Instead, this method relies on the assumption that the instrument is correlated with the key confound, η_{jt}, but otherwise independent from the lagged measure of student ability, HS_GPA$_{jt-3}$. This is because the instrument Treatment$_{jt+1}$ is used to solve the endogeneity caused by u_{jt}, rather than that caused by η_{jt}. The endogeneity caused by η_{jt} is eliminated by including the lagged student ability measure, HS_GPA$_{jt-3}$. For the instrumental variable approach, I estimate equation (5) and instrument HS_GPA$_{jt-3}$ with BA_Available$_{jt+1}$.

The results from the IV exercise are presented in Appendix Table 1, and the results are consistent with the results shown in Tables 3 and 4 in the main texts.

Table A1: Instrumental Variable Diff-in-Diff Robustness Checks

	(1)	(2)	(3)	(4)
	Upgrade Exit	Upgrade Enter	Consol Exit	Consol Enter
Panel A:		Overall BA Degree		
Treatment	-0.010	0.017***	0.179	0.021**
	(0.060)	(0.007)	(0.196)	(0.008)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observation	49.375	49,055	60,833	51,141
Panel B:		Transfer		
Treatment	0.033***	0.016*	0.054	0.052***
	(0.011)	(0.010)	(0.205)	(0.015)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observation	49,375	49,055	60,833	51,141
Panel C:		BA Degree Obtainment for Transfer Students		
Treatment	-0.020	-0.038	0.216	0.075*
	(0.029)	(0.045)	(0.198)	(0.032)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA and BA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observation	12,390	9,810	17,512	13,763

Note: this table presents the instrumental variable difference in differences estimates for the treatment effect on overall BA degree obtainment, transfer rate, and BA obtainment for transfer students. The estimation is performed with two stage least square. Cluster robust standard errors are reported in parentheses, and the significance level convention is: * for 0.10, ** for 0.05, and *** for 0.01. The variable "Treatment" is a dummy that takes value one if the individual is in the post-treatment cohort and in a treatment school. The sample size in each column are different for two reasons: 1. students from institutions that went through upgrades (consolidations) are excluded in the estimation of the effects of consolidations (upgrades), and so the number of excluded students are different for the estimation of different treatment effects; 2. the different definitions of enter and exit cohorts lead to different sample sizes available for these cohorts.

Alternative Measure for Graduation and Alternative Standard Errors Calculation

I also test the robustness of the results to the measure of BA degree obtainment, since most of the treated cohorts in the sample have had less time to complete their study compared to the control cohorts. In particular, I replicate the analysis shown in Table 3 and in Panel B of Table 4, but instead use whether the student obtained a BA degree within 3 years of transfer as the outcome variable. The results of this exercise are shown in Appendix Table 2, and are largely consistent with the results in the main analysis.

Table A2: Robustness Checks using Alternative BA Degree Obtainment Measure

	(1)	(2)	(3)	(4)
	Upgrade Exit	Upgrade Enter	Consol Exit	Consol Enter
Panel A:	Overall BA Degree (Degree 3 Years Post Transfer)			
Treatment	-0.005	0.010**	0.014*	0.020*
	(0.015)	(0.005)	(0.007)	(0.011)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observation	50,603	53,435	66,084	66,292

Panel B:	BA Degree Obtainment for Transfer Students (Degree 3 Years Post Transfer)			
Treatment	-0.022	-0.012	-0.025	0.083***
	(0.016)	(0.026)	(0.035)	(0.021)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA and BA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observation	13,548	14,685	19,954	20,350

Note: this table presents the baseline difference in differences estimates for the effect of the reforms on overall BA obtainment for the full sample of students enrolled in two-year programs, and on BA obtainment rate for the subgroup of students who have transferred to four-year programs. BA Degree obtainment is measured by whether a student obtained a four-year degree within three years of transferring. Feasible GLS estimates are presented and the cluster sensitive standard errors are reported in parentheses. The significance level convention is: * for 0.10, ** for 0.05, and *** for 0.01. The variable "Treatment" is a dummy that takes value one if the individual is in post-treatment cohorts and in a treatment school. "Demographic" control variables include, race and ethnicity fixed effects, age at matriculation, gender, and an indicator for first-generation college students. The sample size in each column are different for two reasons: 1. students from institutions that went through upgrades (consolidations) are excluded in the estimation of the effects of consolidations (upgrades), and so the number of excluded students are different for the estimation of different treatment effects; 2. the different definitions of enter and exit cohorts lead to different sample sizes available for these cohorts.

Additionally, I also examine whether the results of the main analyses are sensitive to the use of FGLS in calculating the standard errors. To do so, I reproduce the results of Tables 3 and 4 but instead report the standard White's heteroskedasticity robust standard errors. The results are presented in Appendix Table 3, and show that the differences in standard error calculations do not lead to change in significance levels of results.

Table A3: White Heteroskedasticity Robust Standard Errors

	(1)	(2)	(3)	(4)
	Upgrade Exit	Upgrade Enter	Consol Exit	Consol Enter
Panel A:	Overall BA Degree Obtainment			
Treatment	0.018	0.034***	0.001	0.028***
	(0.011)	(0.008)	(0.012)	(0.008)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observation	50,603	53,435	66,084	66,292
Panel B:	Transfer			
Treatment	0.034**	0.035***	0.016	0.073***
	(0.016)	(0.012)	(0.014)	(0.011)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observation	50,603	53,435	66,084	66,292
Panel C:	BA Degree Obtainment for Transfer Students			
Treatment	0.027	0.006	0.028	0.103***
	(0.040)	(0.028)	(0.026)	(0.019)
HS GPA	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes
AA and BA School FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Observation	13,548	14,685	19,954	20,350

Notes: this table presents difference in differences estimates for the treatment effect on overall BA degree obtainment, transfer rate, and BA obtainment for transfer students. Heteroskedasticity robust standard errors are reported in parentheses, and the significance level convention is: * for 0.10, ** for 0.05, and *** for 0.01. The variable "Treatment" is a dummy that takes value one if the individual is in the post-treatment cohort and in a treatment school. The sample size in each column are different for two reasons: 1. students from institutions that went through upgrades (consolidations) are excluded in the estimation of the effects of consolidations (upgrades), and so the number of excluded students are different for the estimation of different treatment effects; 2. the different definitions of enter and exit cohorts lead to different sample sizes available for these cohorts.

Synthetic Control

The key advantage of the synthetic control method is its ability to systematically construct a control group for analyzing treatment effects (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2010). Instead of arbitrarily choosing one control group, the synthetic control method constructs a control group by taking the weighted average of all available control groups. The weights are chosen to maximize the pre-treatment similarity between the treatment and the synthetic control group.

In brief, the synthetic control method follows three steps: first, pre-treatment data is used to construct a weighted average of all control groups that best matches the characteristics of the treatment groups; second, the weights calculated in the previous step are used to calculate a weighted average of the post-treatment outcomes of the control groups; third, this counter-factual post-treatment outcome is compared to the actual post-treatment outcome of the treatment group to derive the treatment effect.

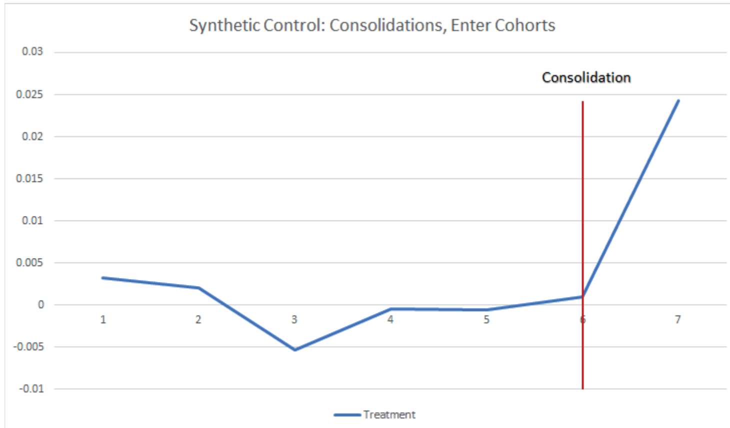
With regards to statistical inference, I follow Abadie, Diamond, and Hainmueller (2010), and use a placebo test procedure akin to a permutation test to perform "exact" inference. In brief, the procedure performs the synthetic control method repeatedly by treating one control group as the placebo treatment group in each iteration. The intuition is that if the synthetic control method is well-behaving, and if there is indeed a positive treatment effect, the estimated treatment effect on the treated groups should be significantly larger than the population distribution of the placebo treatment effects.

The drawback of the synthetic control method is that it is only valid if it is possible to construct a synthetic control group that can trace the pre-treatment outcomes of the treatment group. In contrast, the diff-in-diff method only requires that the control groups and treatment groups are similar in pre-treatment trends, but not necessarily in levels. In the results section, I show that in our setting, the requirement for synthetic control method is not satisfied in some specifications.

I present the results of the synthetic control exercises in figures 3 and 4. The two figures graph the trends of the differences between the observed graduation rates of the treatment groups and the predicted graduation rates of the synthetic control groups. The time of the treatments are indicated by the red bars. Pre-treatment, we would like to see the plotted values to be zeros, as this indicates that the synthetic control groups closely track the properties of the treatment groups. The synthetic control method is appropriate when estimating the treatment effect of consolidations on enter cohorts (figure 3), but is not appropriate when estimating the treatment effect of upgrades on enter cohorts (figure 4).

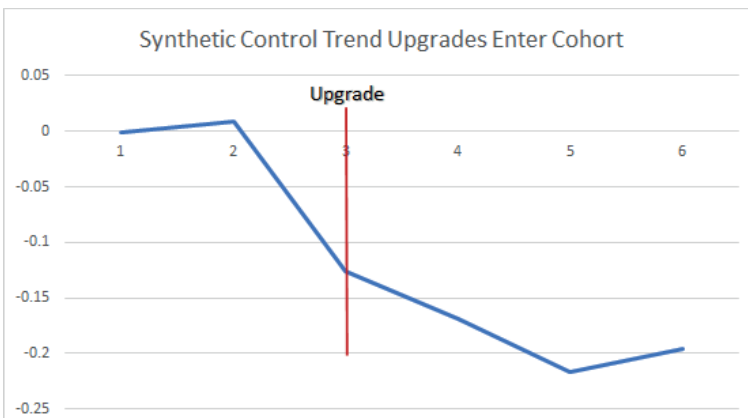
Figure 3 indicates that consolidations increased graduation rates of the enter cohorts by 2.5 percentage points. To check that this result is not by chance, I run placebo tests as described earlier. As can be seen in figure 5, the treatment effect found in figure 3 is the single largest treatment effect in comparison to all placebo tests. Therefore, I conclude that while the synthetic control method may not be appropriate for all treatments considered, the findings generally align with our baseline results.

Figure A1: Synthetic control trend — institutional consolidations with enter cohort



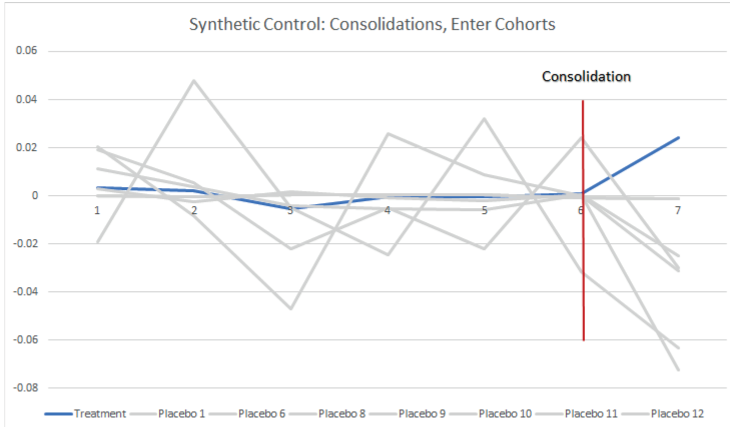
Note: this figure presents the synthetic control results for the Consolidation-Enter treatment. The value of the trend is the difference in graduation rate between the treatment group and the synthetic control group. The red line indicates the year consolidations occurred.

Figure A2: Synthetic control trend — institutional upgrade with enter cohort



Note: this figure presents the synthetic control results for the Upgrade-Enter treatment. The value of the trend is the difference in graduation rate between the treatment group and the synthetic control group. The red line indicates the year upgrades occurred. The large gap between the blue line and zero at time 3 indicates a poor fit of the synthetic control group pre-treatment. This indicates that the prerequisite for the synthetic control method is not satisfied in this case.

Figure A3: Synthetic control trend with placebo — institutional consolidations with enter cohort



Note: this figure presents placebo test results for the synthetic control results for the Consolidation-Enter treatment. The value of the blue line is the difference in graduation rate between the treatment group and the synthetic control group. The value of each grey line is the difference in graduation rate between the placebo treatment group (one of the original control groups) and the synthetic control group for the placebo treatment group. The red line indicates the year consolidations occurred. Following Abadie, Diamond, and Hainsmueller (2010), we discard all placebo treatment groups that lead to a larger than 0.05 gap in the pre-treatment fit.

Alternative Definition of Exit Cohorts

In the main analyses, I define exit cohort y as students who leave their two-year programs in academic year y, either by transferring up to four-year programs, or by leaving college altogether. An unintended disadvantage of this definition is that I cannot distinguish between students who leave their two-year program intending to transfer to a four-year program and those who simply drop out of college. While this would not affect the analyses that condition on students transferring to a four-year program, it may lead to difficulty in interpreting the results from the full sample of two-year college students. As a robustness check on a more comparable and uniform student population, I implement the main analyses on the subsample of students who have earned their two-year degree. In other words, exit cohorts y would be defined as the academic year students earned their two-year degree. The analyses replicates results from Tables 3 and 4 for the exit cohorts, and are reported in the following Table.

Table A4: Alternative Definition of Exit Cohorts

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall BA Obtainment		Transfer		BA Obtainment for Transfers	
	Upgrade Exit	Consol Exit	Upgrade Exit	Consol Exit	Upgrade Exit	Consol Exit
Treatment	0.015	0.011	0.012	-0.010	0.010	0.020
	(0.050)	(0.038)	(0.055)	(0.040)	(0.054)	(0.041)
HS GPA	Yes	Yes	Yes	Yes	Yes	Yes
Demographic	Yes	Yes	Yes	Yes	Yes	Yes
AA School FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,031	9,490	7,031	9,490	4,743	6,594

Notes: this table presents difference in differences estimates for the treatment effects on overall BA degree obtainment, transfer rate, and BA obtainment for transfer students. Sample is restricted to students who have obtained an Associate's Degree. The upgrade-exit and consol-exit treatment-cohort combinations are considered. Exit cohorts are defined by the academic year students obtained their two-year degrees. Cluster robust standard errors calculated using feasible GLS are reported in parentheses, and the significance level convention is: * for 0.10, ** for 0.05, and *** for 0.01. The variable "Treatment" is a dummy that takes value one if the individual is in the post-treatment cohort and in a treatment school. The sample size in each column are different for two reasons: 1. students from institutions that went through upgrades (consolidations) are excluded in the estimation of the effects of consolidations (upgrades), and so the number of excluded students are different for the estimation of different treatment effects; 2. the different definitions of enter and exit cohorts lead to different sample sizes available for these cohorts.

The results reaffirms the findings of the main analyses that neither upgrade nor consolidations significantly improved the overall bachelor's degree obtainment of exit cohorts. The only substantial difference is that the estimated treatment effect of upgrades on the transfer rate of exit cohorts, while still positive, is no longer statistically significant. This is likely because of the loss of statistical power since a large share of transfer students are conditioned out with the current definition of exit cohorts.

8 Appendix to Chapter 1: CIP Codes Used in Table 2 of Main Texts

CIP Code	Major
3	Natural Resources and Conservation
9	Communication, Journalism, and Related Programs
11	Computer and Information Sciences
13	Education
15	Engineering Technologies
16	Foreign Languages, Literatures, and Linguistic
23	English Language and Literature/Letters
24	Liberal Arts and Sciences, General Studies and Humanities
26	Biological and Biomedical Sciences
27	Mathematics and Statistics
31	Parks, Recreation, Leisure, and Fitness Studies
40	Physical Sciences
42	Psychology
43	Security and Protective Services
44	Public Administration and Social Service
45	Social Sciences
49	Transportation and Materials Moving
50	Visual and Performing Arts
51	Health Professions and Related Clinical Sciences
52	Business, Management, Marketing, and Related Support Services
54	History

Chapter 2: Free Community College: Promise or Trap? — A Study on College Transfer in the US

[21]

With increasing tuition for four-year colleges and with significantly cheaper tuitions available in community colleges, a large share of US college students are enrolled in community colleges instead of four-year colleges. According to statistics from the National Center for Education Statistics (NCES), in the 2018-2019 academic year, 42% of public college enrollment were at the two-year level. Moreover, NCES statistics show that while 61.1% of community college students come from families with parental income below $63,000, only 42.5% of four-year college students come from families with parental income below $63,000. Perhaps due to this over-representation of low-income students in community colleges, redistributive tuition policies focusing on community colleges have become increasingly popular at both individual state levels and at the national level. A prime example of such policies is the "America's College Promise" program proposed by President Barack Obama in 2015, which offers to make two years of community college free for students maintaining satisfactory progress towards a degree. The main policy question of this paper is: how would providing free community colleges impact student outcomes? Would it fulfill its promise to boost upward mobility or create a trap that promotes associate degrees over the more lucrative bachelor's degrees? A complete answer to this question requires one to consider both the effect of free tuition on community college students' outcomes, and the effect of such policy on students' decision to enroll in community colleges.

To fully understand students' enrollment decision in community colleges, one must acknowledge that community colleges and four-year colleges do not operate in isolation. While community colleges offer terminal degrees in the form of associate degrees, many community college students plan to transfer to four-year colleges and eventually earn bachelor's degrees. According to a study at the National Student Clearinghouse (NSC), more than 80% of community college freshmen report that they plan to obtain a bachelor's degree, and more than 30% of them do in fact transfer to a four-year college (Shapiro et al, 2015). Another important type of transfer is the "reverse transfer", where students from four-year institutions transfer to community colleges. According to the same study by the NSC, 18% of public four-year college freshmen transferred to a community college at some point during their college enrollment. In such cases,

[21]The administrative data used in this paper is provided jointly by the Texas Higher Education Coordination Board, Texas Education Agency, and Texas Workforce Commission. The results and conclusions of this paper in no ways reflect the official view of the above agencies and the State of Texas.

community colleges provide a crucial buffer for students who cannot complete a bachelor's degree, by providing them an opportunity to transfer their course credits towards a two-year degree that is valued by the labor market (Kane and Rouse, 1995).

The tight connection between community colleges and four-year colleges generates a rich set of college pathways for students to choose from, and creates significant challenges for assessing the full impact of a free community college policy. To address this challenge, I build a model on college students' dynamic school choices, in which students update their enrollment decision each semester, and choose whether to enroll in a community college, a four-year college, or drop out of college. The model takes into account a wide range of fixed student heterogeneity, and incorporates an unusually large number of dynamic factors that may affect students' outcomes and decisions. In particular, the model incorporates students' GPA, credit accumulation, part-time work status, highest degree earned, previous school enrolled, as well as "course schedule coherence", which is a novel measure I introduce to capture students' completion of degree-required courses. In addition, the model takes into consideration the complexity of credit transfer between institutions, and allows for credit loss during transfer as well as incorrect expectation on credit loss, which I find to be an important friction in the transfer pathways.

This rich school choice model is estimated using administrative data from the State of Texas, and allows me to not only predict the full impact of a free community college policy but also answer several important intermediate questions. This paper answers three specific questions. First, how would a free community college policy affect students' enrollment decision and outcomes? Second, what is the overall value of the options to transfer between community college and four-year colleges, and how different will student outcomes be without these options? Third, how would improvements in credit transfer affect students' outcomes?[22]

The main result of the paper is that free community college policies could have significant mixed effects on student outcomes — while free community colleges improves student welfare, it decreases overall bachelor's degree attainment by 7 percentage points (21% reduction) and reduces students' (discounted) lifetime income by more than \$6000 (1.2% decrease). The key reason behind the negative outcomes is that free community college diverts students from four-year colleges to two-year colleges, which has lower labor market returns. In addition, the increase in community college enrollment subjects more students to imper-

[22]I consider both reduction in the number of credits lost during transfer and improvements in students' awareness of credit loss during transfer.

fect information in the two-to-four transfer pathway — I find that while students lose 20% of their credits when transferring from community colleges to four-year colleges, they only anticipate half of the credit loss prior to transferring.

In addition to the main result, I also find that the options to transfer between community colleges and four-year colleges have profound influence on students' degree attainment, and modest impact on student welfare. Counterfactual exercises show that eliminating all transfer options reduces student welfare by $2000 (0.3% decrease), and reduces bachelor's degree attainment by 11 percentage points (33% decrease). Without the transfer options, associate degree attainment increases by 23 percentage points (79% increase).

Finally, I find that improvements in credit transfer can significantly improve transfer student outcomes as well as overall degree attainment. Eliminating credit loss during transfer increases the transfer graduation rate by 13 percentage points (21% increase) and overall bachelor's degree attainment by 7 percentage points (21% increase). Improving students' anticipation of credit loss during transfer, which can be achieved by improved academic advising in community colleges, increases the transfer graduation rate by 7 percentage points (11% increase) and overall bachelor's degree attainment by 4 percentage points (12% increase).

This paper contributes to four separate literature. First, this is one of the first papers that analyzes the effect of a free community college policy. In doing so, I contribute to the literature examining the effects of student aid and tuition reduction on students' enrollment decisions and subsequent outcomes (Dynarski, 2003; Denning, 2017; McFarlin, McCall, and Martorell, 2017; Andrews, DesJardins, and Ranchhod, 2010). A recent paper based on evidence from a local free community college program in Knox County, Tennessee, shows that free community college increases two-year degree attainment but has mixed effect on bachelor's degree attainment (Carruthers, Fox, and Jepsen, 2020). In comparison to Carruthers et al (2020), this paper provides three important contributions. First, this paper examines the potential effect of a state-wide free community college policy in one of the largest states in the country, and is therefore less susceptible to external validity concerns. Second, using the Texas administrative data, I am able to track the long term transfer history, degree completion and earnings of students, and can therefore directly analyze the long-run impact of free community college on degree attainment and life-time income. Third, the structural approach of this paper allows me to directly compare free community college with other cost-equivalent tuition policies.

Second, this paper contributes to the small but growing literature on student success in the two-to-four transfer pathway (Kane and Rouse, 1995; Monoghan and Attewell, 2015; Long and Kurlaender, 2009). This paper leverages a school choice model to identify and estimate the amount of credit loss during transfer that students anticipate, and employs counterfactual analysis to study the potential effect of a wide range of popular policies designed to improve transfer student success, including elimination of credit loss during transfer, improved academic advising prior to transfer and assistance in adjusting to new environments that may reduce psychic cost of transferring.

Third, to the best of my knowledge, this is the first paper that measures the "coherence" of a student's course selection, and studies how coherence affects students' school choice and degree completion. In doing so, I contribute to the broad literature that studies factors determining student success in post-secondary education (Dynarski, 2003; Castleman and Long, 2016; Fairlie et al, 2014; Hoffman and Oreopoulos, 2009; Scott-Clayton, 2011a) and the literature that discusses the importance of academic advising in students' navigation of degree programs (Bettinger and Baker, 2014; Bahr, 2008; Scott-Clayton, 2011b).

Finally, the use of a rich administrative data allows me to incorporate many important details of the college experience that have not been built into school choice models previously used in the literature (Keane and Wolpin, 1997; Arcidiacono, 2004; Arcidiacono et al, 2016). These include credit accumulation, credit loss during transfer, and course selection coherence. In addition, I also utilize quasi-experimental variation in community college tuition pricing caused by Texas' Community College Taxing Districts (CCTD) to validate the model's parameter estimates. A few other examples of recent papers that combines quasi-experimental evidence with structural estimation are Luflade (2017) and Attanasio et al (2020).

The remainder of the paper proceeds as follows. In the next section, I introduce the Texas administrative data, provide reduced-form patterns of students' transfer behavior, and present results from a regression discontinuity design using CCTD borders. In the third section, I detail the model of dynamic college choice. The fourth section discusses the identification strategy. The fifth section presents the structural estimation results and provides empirical evidence supporting the sources of identification. The sixth section presents and discuss the counterfactual simulation analyses. Finally, the last section provides the main policy interpretations and concludes.

1 Data and Reduced-Form Patterns

1.1 The Texas Administrative Data

The empirical analysis of this paper uses a large administrative data from Texas, which links post-secondary education data from the Texas Higher Education Coordination Board (THECB), to high-school data from the Texas Education Agency (TEA), and to labor-market data from the Texas Workforce Commission (TWC). This allows researchers to observe an individual's progression from high school, to college, and to the labor market, so long as the individual remains in Texas.[23]

The TEA data provides enrollment records, demographic backgrounds, course completion, graduation records, and attendance records for all Texas public high school students since 1993. The TEA data also includes students' performance in the state-wide standardized exams required for high school graduation. Given the high participation rate of this standardized exams, students' percentile ranking in these exams will be taken as their measure for observable ability. The THECB data provides information on all college students enrolled in Texas public higher education institutions since 1992.[24] The Texas public higher education sector is composed of 38 four-year colleges and 50 public community colleges. The THECB data contains records of students' demographic background, enrollment, course selection and course grades, degree obtainment, as well as financial aid and student loan. The TWC data contains quarterly wage records of all Texas residents since 1990, and is pulled from the unemployment insurance records.

[23] While it is possible to track students who enroll in college out-of-state with the link to the National Student Clearinghouse (NSC) data, these records are not used in this paper, since the NSC data does not contain transcript level data and only cover students enrolled in college after 2008. The effect of this data omission is likely small, as only 5% of Texas public high school graduates enrolled in a college out of Texas between 2011 and 2015.

[24] While the data has information on students from both public and private institutions, I restrict my analysis to students in public institutions since critical information including transcript records are not available for private colleges.

1.2 Patterns of Student Transfers in Texas

Among Texas high school graduates who enroll in an in-state public institution, 54.9% of students choose community colleges as their first college and 45.1% of students enroll in four-year colleges following high school. These school choices, however, change significantly as students progress through college. 23.7% of community college freshmen eventually transfer into a four-year college to pursue a bachelor's degree. The reverse transfer is also prevalent — 22.1% of four-year college freshmen transfer to a community college and enroll for at least two consecutive semesters. Even for freshmen in one of the three selective four-year colleges, UT Austin, UT Dallas, and Texas A&M, 12.8% transfer to a community college at some point in their college career. Although not a focus of this paper, transfers also occur between schools in the same category. Using the same data, Andrews and coauthors calculate that 31.03% of non-selective four-year students who only transfer once transfer to another non-selective four-year college (Andrews et al, 2014).

Student outcomes also demonstrate strong interconnection between institutions. Among all community college freshmen who eventually earn a degree, 43.8% go through the two-to-four transfer pathway and obtain bachelor's degrees from a four-year college. Among four-year college freshmen who obtained a college degree, 9% obtained a two-year degree as their highest degree. Empirical patterns confirm the high attrition rates in the two-to-four transfer pathway. As mentioned earlier, only 23.7% of community college freshmen manage to transfer into a four-year college to pursue a bachelor's degree, and the bachelor's degree obtainment rate of students who transfer is 50.3%.

What factors explain students' initial school choices and subsequent transfer behaviors? Tabulation of average student characteristics across different types of institutions depict a familiar pattern, in which socioeconomic status and academic ability are significantly related to students' school choice. As reported in Table 1, students in four-year colleges tend to have higher standardized test scores, higher parental income, and is less likely to come from ethnic minority groups. These summary statistics support the hypothesis that the lower tuition rate and less restrictive admission policies in community colleges are important for students' decision to attend community colleges instead of four-year colleges.

To explore factors that may help further capture students' dynamic decision making, I perform a series of logit regressions on students' next-period school choice conditional on present-period educational outcomes. Specifically, for $s \in \{1, 2\}$, I run regressions in the form of

$$logit(\pi_i) = \beta_0 + \beta_1 A_i + \beta_2 g_{it-1} + \beta_3 h_{it-1} + \beta_4 \text{Univ}_{it-1} + \boldsymbol{\beta X_i} + \epsilon_{it} \tag{7}$$

where $\pi_i = \Pr(s_i = s)$. A_i is a measure of the student's academic ability measured by high school standardized test scores, g_{it-1} is the student's GPA in period $t-1$, h_{it-1} is the student's accumulated credit hours in period $t-1$, and Univ_{it-1} is an indicators for whether the student was enrolled in a four-year college in period $t-1$. The results of these logistic regressions are presented in Table 2, and show several interesting patterns. First, coefficients on students' academic ability reaffirms the selection of higher ability students into more selective institutions. Second, students with higher previous semester GPA are more likely to enroll in a more selective institution in the next period. While this pattern could be reflecting the stricter admission policy for selective institutions, it is also consistent with students updating their school choice by learning new information about their academic ability through GPA. Third, higher credit accumulation in the previous period predicts higher chances of enrolling in a more selective institution, and predicts a lower chance of enrolling in a community college. This pattern is consistent with students updating their school choices according to shocks to their credit accumulation — a negative (positive) shock in credit accumulation would make pursuing a four-year degree more (less) costly, and would therefore make enrollment in more selective institutions less (more) attractive. Finally, students' school choices show a certain level of persistence, in that enrollment in one type of institution reduces the log odds of enrolling in a different type of institution but increases the log odds of enrolling in the same type of institution in the next period. This set of results suggest that there may be significant utility costs in applying for transfer admission and in adjusting to a new environment.

1.3 Regression Discontinuity Using CCTD Borders

Lower tuition rate is perhaps one of the most important features that attract students to enroll in community colleges. Given the focus on the impact of free community college policies in this paper, it is important to empirically examine the sensitivity of students' school choice decisions to tuition, especially in the margin of choosing between community colleges versus four-year colleges. To do so, I utilize the quasi-experimental variation in community college tuitions across borders of community college taxing districts (CCTD), and use a spatial regression discontinuity (RD) design to analyze the effect of residing in a CCTD on students' school choices and educational outcomes. In addition to shedding light on how community college tuition affects students' school choice between community college and four-year colleges, the RD estimates will also be used post-estimation as untargeted moments to verify the validity of the estimated model.

Community College Taxing Districts in Texas

I first provide the institutional details related to CCTDs in Texas. Community colleges in Texas collect property taxes for maintenance and operations from residence of their taxing districts. In 2016, 42% of community college revenue came from these taxes, representing the single largest source of funding for community colleges in Texas. In comparison, another 24% of revenue came from state appropriations, and the remaining 34% came from tuitions and fees. In 2018, while the average property tax rate for counties in Texas was 1.81%, the average community college tax rate was 0.16%. In return to the taxes levied, residences of community college taxing districts pay a significantly lower tuition rate when they enroll in their in-district community colleges. In 2017, for example, the average tuition and fees per semester credit hour for out-of-district students was $143, while that for in-district students was $90.[25]

With only a few rare exceptions, the boundaries of CCTDs in Texas are based on school district boundaries, and 336 of the 1227 Texas school districts are currently listed as part of one of the 50 CCTDs.[26] Figure 1 depicts the spatial distribution of CCTDs in Texas. From the figure, we can see that while there is a denser distribution of CCTDs near Dallas and Houston, CCTDs are not exclusively located near large metropolitan areas. Moreover, there are many instances where some but not all school districts in one county are included in a CCTD. All combined, the spatial distribution of CCTD boundaries creates significant state-wide variation in community college tuition between students that live in close proximity.

[25] Out-of-district is defined as students that are residents of the state of Texas or qualify for in-state tuition but do not reside in the community college district concerned.

[26] Inclusion of a school district into a CCTD is determined either by contract or by election. Annexations are relatively rare events: between 1995 and 2012, 22 districts annexed into 5 CCTDs (Denning, 2017). In this paper, I exclude school districts that were part of an annexation for the regression discontinuity analysis.

Balance Tests for Regression Discontinuity

Before using the CCTD-induced variations in tuition for RD analysis, we have to examine whether the boundaries are otherwise exogenous to student characteristics that may affect school choices and educational outcomes. Intuitively, the concern for endogenous residential choice should be minimal in this context, as parents who have the aspiration for schooling and financial capability to factor children's college choices into housing decisions would likely not intend to send their kids to a community college. Regardless, I implement multiple balance tests to examine whether there is any difference in average student and family characteristics between students in and out of CCTDs.

Figure 2 presents balance tests on four average student characteristics: parental income, gender share, average math score in standardized test, and average reading score in standardized test. The horizontal axes of the figures are the distance to CCTD border in kilometers, and the averages for students in CCTDs (distance equal to 0) are marked with red. Panel a-d shows the average parental incomes, gender share, math score, and reading score for students by 1 km bins. All four figures show no discernable difference between CCTD students and non-CCTD students.

Empirical Specifications and Results

An important detail of the Texas administrative data is that it does not contain information on students' residential address. Instead, I rely on students' high schools and the school district they are affiliated to when classifying students' CCTD status and when calculating their distance to a CCTD boundary. Since CCTDs are mostly based on school districts and since we only consider public high school students, the effect of this data limitation is likely small. Based on this classification, among all Texas public high school students, 63.8% reside in a CCTD, 33.8% live within 100 km distance to a CCTD border but are not in a CCTD, and the remaining 2.4% of students live more than 100km away from a CCTD.

As a first stage for the RD, and to check whether using students' high schools is a sensible simplification, I examine whether graduating from a high school located in a CCTD lead to lower tuition rates when enrolled in a community college. For this purpose, I first plot the average share of community college students who pay in-district tuition over the distance of the students' high school to a CCTD border. The pattern is presented in Figure 3, and shows a discontinuous increase in the chance students in high schools that are located within a CCTD qualifies for in-district tuition. Because of the non-trivial non-compliance on both sides of CCTD borders, the results of the following RD estimation should be taken as the effect of the decrease in average community college tuition for graduates in high schools located within CCTDs, rather than the effect of the policy-stipulated decrease in community college tuition for residing in a CCTD.

The main outcome of the regression discontinuity analysis is the effect of CCTD on the probability a student chooses a community college as her first college after high school:

$$\omega = \Pr(s_{i1} = 1 | s_{i1} > 0 \text{ and } \text{CCTD}_i = 1) - \Pr(s_{i1} = 1 | s_{i1} > 0 \text{ and } \text{CCTD}_i = 0) \tag{8}$$

ω is estimated with the following regression using the subset of students who have once enrolled in college:

$$\text{First\_CC}_i = b_0 + b_1 \text{CCTD}_i + b_2 \text{Distance}_i + BX_i + e_i \tag{9}$$

where First_CC_i is an indicator for choosing a community college as the first college after high school, and X_i is a set of control variables including ethnicity, gender, and standardized test scores. $\hat{b}_1$ is taken as the estimator for ω.

Although not used in the untargeted moment matches, I also analyze the effect of CCTD on students' later educational outcomes, including length of enrollment, credits accumulated, vertical transfer rate, and degree obtainment. These analyses partly address the policy-relevant concern that lowering tuition at community colleges can undermine students' degree obtainment by diverting students from four-year colleges to community colleges (Rouse, 1995; Mountjoy, 2019). For these regressions, I substitute the dependent variable of equation (4) with the various outcomes considered.

The RD results are presented in columns (1) - (6) in Table 3. The results suggest that the CCTD tuition discount increases the probability that a student chooses a community college as her first college by 2.1 percentage points. Furthermore, I find evidence that the lower in-district tuition rate encourages longer periods of college enrollment. Specifically, the CCTD tuition discount increases enrollment length by 0.30 semesters, and cumulative semester credit hours by 2.29. However, as shown in column (4), the CCTD tuition discount lowers vertical transfer rate by 1.4 percentage points. I also find mixed results for degree obtainment, as presented in columns (5) and (6). While the CCTD tuition discount increases the overall degree obtainment rate by 0.4 percentage points, it decreases BA degree attainment rate by 0.6 percentage points.

Using recent annexations of CCTD districts as quasi-experimental variation, Denning (2017) finds results that are similar to the findings above: enrollment in community colleges increases by 3.2 percentage points and enrollment in college overall increases by 3.1 percentage points, leading to a 2.9 percentage point increase in the share of high school graduates initially enrolling in community colleges. Other papers using CCTD borders and CCTD expansions also find similar results (McFarlin et al, 2017; McFarlin et al, 2018)

2 Model of Dynamic College Choice with Transfer Options

2.1 Overview and Notations

The model begins from the time a student, i, graduates from high school, follows her path through the higher education system, and ends when she enters the labor market. There are 13 time periods in the model, with each time period representing a semester, so that $t \in \{0, 1, ..., 12\}$. Each semester, the student makes a discrete school choice, $s_{it} \in \{0, 1, 2\}$, where 0 denotes the labor market, 1 denotes two-year community colleges, and 2 denotes four-year institutions. Moreover, for each school choice, the student also chooses whether or not to work part-time, $l_{it} \in \{0, 1\}$. In the model, I assume that students not enrolling in school are working, and abstract from the labor market participation decision.

Students are heterogeneous in many dimensions. First, students have different parental income, p_i, parental education, PE_i, and observable ability, A_i, which are all fixed throughout the model and perfectly observable. PE_i is measured by a binary indicator of whether the student is a first-generation college student and A_i is measured using the student's percentile ranking in Texas' standardized high school exams. Second, students are heterogeneous in their unobservable ability, u_i, and unobservable preference for schooling θ_i. Unobservable ability, u_i is fixed but not perfectly observable for either the students or the econometrician. Instead, students sequentially learn about their unobservable ability through the revelation of their GPA each semester, and form updated beliefs on their unobservable ability, μ^u_{it}. In addition, students can have either a high or a low preference for schooling — $\theta_i \in \{0, 1\}$. θ_i is observed by the students but unobservable for the econometrician. Student has a high preference with probability q. Finally, students that reside in a CCTD face a significantly lower community college tuition rate.

Besides the heterogeneity described, the model also takes into account three intermediate schooling outcomes that are updated each period. I consider the students' GPA, g_{it}, credit accumulated, h_{it}, and course selection coherence, c_{it}, which measures the completion rate of required, non-elective, courses. The students' school of enrollment and highest degree earned are denoted as s_{it} and d_{it}, and also enter the model as state variables. All combined, students' state variable space is 11 dimensional — $\Omega_{it} = (A_i, p_i, PE_i, \theta_i, \text{CCTD}_i, s_{it}, g_{it}, h_{it}, d_{it}, c_{it}, \mu^u_{it})$.

The production of GPA, credit accumulated, and course selection coherence are stochastic, and, along with iid shocks to in-school utility compose of the main sources of random variation in the model.

Students gain (dis)utility from college enrollment and attach utility to expected life-time earnings. Given these sources of utility, the state variables, and the stochastic factors described, the student chooses s_{it} and l_{it} each period. In what follows, I describe details of the model following the logic of backward induction. I first describe how a student forms expectation on her life-time earnings. Then, I describe the production of educational outcomes and the associated transition equations for state variables. Finally, I specify the students' utility functions and budget constraint, and combine all the details to write down the value function and the student's optimization problem.

2.2 Labor Market Outcome

Students' full-time labor market outcomes are jointly determined by their observable ability, highest degree earned, work experience, cumulative GPA, as well as an error term, ι^d_{it}, which follows an AR(1) process and depends on θ_i. I follow Mincer (1958) in specifying a log wage regression model. For each $d \in \{0, 1, 2\}$:

$$\log(w_{it}) = \beta_{0,d} + \beta_{1,d} A_i + \beta_{2,d} g_{it} + \beta_{3,d} e_{it} + \beta_{4,d} e^2_{it} + \iota^d_{it} \tag{10}$$

$$\iota^d_{it} = \rho_d \iota^d_{it-1} + \zeta^d_{it} \tag{11}$$

where ζ^d_{it} is an iid normal shock that captures both the innovation to the AR(1) process and the purely transitory shock to log wage. I assume that $\zeta_{it}^d \sim N(0, \sigma_{\iota d})$. The initial value of ι_{it}^d depends on θ_i and the highest degree d. All parameters in equations (1) and (2) are specific to the highest degree earned d.

I assume that an individual has 40 years of working time if she chooses to directly enter the labor market at the beginning of the model. Since each time period represents one semester, an individual that enters the labor market at period t has $40 - t/2$ years of working time left.

For part-time work during college enrollment, I specify a similar log linear wage model:

$$\log(w_{it}^p) = \beta_{0,s}^p + \beta_{1,s}^p A_i + \beta_{2,s} g_{it} \tag{12}$$

Students' part-time wage does not depend on their highest degree earned, but rather depend on their current school of enrollment. This reflects the possibility that students enrolled in UT Austin might work in different types of part-time jobs as students enrolled in Houston Community College, although neither group have a college degree. The part-time wage model does not contain an experience trend and AR(1) unobservable heterogeneity because observations of part-time work are much more sporadic.

2.3 Schooling Outcomes: Credit, Grade, Coherence, and Degree Attainment

Accumulation of total credits depends on the type of institution enrolled, student's observable ability, and part-time work status:

$$\Delta h_{it} = \delta_{0,s} + \delta_{1,s} A_i + \delta_{2,s} \mathrm{PT}_{it} + \epsilon_{it}^{h,s} \tag{13}$$

When students transfer from community colleges to four-year colleges, their accumulated credits do not necessarily transfer. The amount of credits transferred is equal to $h_{t-1} \times (1 - \kappa_{12})$, where κ_{12} is the share of credits lost during transfer. To add to the complexity of credit transferring, anecdotal evidence suggests that students do not correctly anticipate all credits that are lost during transfer. In other words, the amount of credit lost sometimes comes as a surprise to students post transfer, and is often listed as a key challenge two-to-four transfer students face. To capture this important information friction, I allow students to have imperfect information on credit transfer, and write the amount of credits student expects to transfer as $h_{t-1} \times (1 - \kappa_{12}\kappa_e)$. κ_e measures the level of error in students' understanding of the credit transfer rules. If $\kappa_e = 1$, for example, students perfectly understand the transfer credit rules and correctly anticipate the amount of credit loss during transfer. When students transfer in the opposite direction from four-year colleges to community colleges, they can transfer a maximum of 45 semester credit hours, and lose the remaining credits.

Course selection coherence of a student in a given major, j, is defined as:

$$c_{it}^j = \max\{1, \frac{h_{it}^j}{n^j}\} \tag{14}$$

where h_{it}^j is the number of the student's courses that count towards the degree requirements for major j, and n^j is the total number of credits in required, non-elective, courses for major j, which includes both core curriculum requirements and major required courses.[27] Once students complete all required courses for a major, the students' course selection coherence is fixed at 1. The student's overall course schedule coherence is defined as the maximum coherence across all majors:

$$c_t = \max_j c_{it}^j$$

Obtainment of degree d_{it} has three requirements. First, students must be enrolled in the associated school, so that $\tilde{s}_{it} = d_{it}$. Second, students need to accumulate enough credits — at least 120 credits for $d_{it} = 2$ or 3 and at least 60 credits for $d_{it} = 1$. Third, all course requirements have to be fulfilled, so that the course selection coherence measure, defined as the share of required course credits completed, reaches one.

Formally, the initial condition is $d_{i1} = 0$, and the transition equation for d_{it} is:

$$d_{it} = \begin{cases} \tilde{s}_{it} & \text{if } h_{it} \geq 120 \ \& \ c_{it} = 1 \ \& \ \tilde{s}_{it} > 1 \\ 1 & \text{if } h_{it} \geq 60 \ \& \ c_{it} = 1 \ \& \ \tilde{s}_{it} = 1 \\ d_{it-1} & \text{otherwise} \end{cases} \tag{15}$$

Course selection coherence, c_{it} is defined as:

$$c_{it} = \max\{1, \max_j(\frac{h_{it}^j}{n^j})\} \tag{16}$$

[27] n^j for most majors is smaller than 120. $120 - n^j$ is the number of elective course credits students are allowed to take to graduate with 120 credits.

where h_{it}^{j} is the number of student i's courses that count towards the degree requirements for major j, and n^{j} is the total number of credits in required courses for major j, which includes required core curriculum courses and required major courses.Once students complete all required courses for a major, the students' course selection coherence is fixed at 1.

Students' course selection coherence is updated each semester, and the updates reflect students' progress towards completing all graduation requirements. Since academic advising can guide students to choose courses that best fit their degree plans, and since different types of institutions likely have different academic advising quality, I allow the production of coherence to depend on the institution students are enrolled in. Furthermore, to reflect the possibility that parents with college experiences may be either complements or substitutes for academic advising, I also allow the first-generation college student indicator, PE_i, to affect the updating of coherence. Specifically, for each $s \in \{1, 2, 3\}$, the coherence measure is updated following:

$$\Delta c_{it} = \gamma_{0,s} + \gamma_{1,s} A_i + \gamma_{2,s} PE_i + \gamma_{3,s} \text{PT}_{it} + \epsilon_{it}^{c,s} \tag{17}$$

where PT_{it} is again the indicator for part-time enrollment.

Another key intermediate outcome in college is the student's GPA. In this model, I allow students' GPA to depend on the school of enrollment, observable ability, as well as unobservable ability. In particular, the student's semester GPA is produced by:

$$g_{it} = \lambda_0 + \lambda_1 A_i + \lambda_2 \text{Uni}_{it} + \lambda_3 \text{Uni}_{it} \times A_i + u_i + \epsilon_{it}^{g} \tag{18}$$

where Uni_{it} is an indicator for enrollment in a four-year college, and u_i is the student's unobservable ability. ϵ_{it}^{g} is the idiosyncratic shock in the GPA production and is assumed to be iid distributed with $N(0, \sigma_g)$.

From a student's perspective, there are two sources of uncertainty in the production of GPA: u_i and ϵ_{it}^{g}. While u_i provides useful information for prediction of future wages and utility, ϵ_{it}^{g} is an idiosyncratic shock. However, the student does not know her true unobservable ability, and only has a prior belief on it. The prior belief on true unobservable ability is characterized by a normal distribution $u_i \sim N(\mu_{it}^{u}, \sigma_{it}^{u})$, where μ_{it}^{u} represents the student's current estimate for her unobservable ability and σ_{it}^{u} reflects the amount of uncertainty she has with the guess. Equipped with this belief, a student can form an expectation for her semester GPA:

$$\hat{g}_{it} = \lambda_0 + \lambda_1 A_i + \lambda_2 \text{Uni}_{it} + \lambda_3 \text{Uni}_{it} \times A_i + \mu^u_{it} \tag{19}$$

When the true GPA is revealed to the student, the difference between the true GPA and the expected GPA, $\xi_{it} = g_{it} - \hat{g}_{it}$, would serve as a signal for the student to update her belief on unobservable ability. For example, if $\xi_{it} > 0$, the signal indicates that the student may be underestimating her unobservable ability and should update her belief upwards. Naturally, the extent of this update depends on the amount of uncertainty she has with her belief — the more uncertain she is about her belief, the more she would update her belief according to the signal. In addition, the extent of this update also depends on how noisy the grade signal is — if σ_g is large, the student would infer that the grade signal is largely composed of noise, and would not update her belief by much. These intuitions are captured by the Kalman filter and the Bayesian updating rules (Guvenan and Smith, 2014):

$$K_{it} = \frac{(\sigma^u_{it-1})^2}{((\sigma^u_{it-1})^2 + \sigma^2_g)} \tag{20}$$

$$\mu^u_{it} = \mu^u_{it-1} + K_{it}\xi_{it} \tag{21}$$

$$\sigma^u_{it} = \sigma^u_{it-1}(1 - K_{it}) \tag{22}$$

Finally, students' initial belief on their unobservable ability follow the population distribution, $u_i \sim N(0, \sigma^u_0)$, and so $\mu^u_{i0} = 0$ and $\sigma^u_{i0} = \sigma^u_0$.

2.4 Utility, Constraints, and the Students' Problem

There are two main sources of utility for the students: in-school utility while enrolled in college and wage utility once the student enters the labor market. The amount of utility students derive in school depends on their observable ability, the type of institution, as well as their transfer status. In particular:

$$v_{it}^s = \alpha_0 + \alpha_1 A_i + \alpha_2 \text{Uni}_{it} + \alpha_3 A_i \times \text{Uni}_{it} + \alpha_4 g_{it} \\ + \alpha_5 \text{PT}_{it}^c + \alpha_6 \text{PT}_{it}^u + + \alpha_7 \text{TR}_{it}^{12} + \alpha_8 \text{TR}_{it}^{21} + \alpha_9 \theta_i + \epsilon_{it}^u \tag{23}$$

where PT_{it}^c and PT_{it}^u are indicators for part-time work status when enrolled in a community college and a four-year college, respectively. TR_{it}^{12} is an indicator for two-to-four transfer students and TR_{it}^{21} is an indicator for four-to-two transfer students. This specification allows the utility cost of part-time work while enrolled to depend on the type of school enrolled in and allows the adjustment costs of transferring to depend on the direction of transfer. The shock to in-school utility, ϵ_{it}^u, follows a type 1 extreme value distribution, and is independently distributed across time. The inclusion of θ_i, the unobservable heterogeneous type, provides some persistence in the unobservable components of the utility function, despite the idiosyncratic shock being iid.

The utility from full-time labor market participation is modeled with a linear utility function:

$$v_{it}^w = \alpha_w w_{it} \tag{24}$$

with α_w normalized to 1, so that all utility parameters can be interpreted as having dollar values.

Students can finance their costs of enrollment through three channels. First, students can pay for tuition with their parents' contribution and financial aid, which they do not have to pay back. Second, students can use their part-time income while enrolled to pay for tuition. Third, students can also borrow student loans to finance their college costs. In particular, students can borrow from either the federal student loan program or Texas' college access loan program, which has a higher interest rate.

Financial aids and parental transfers are functions of student's ability, the type of institution, and parental income:

$$\text{FA}_{it} = \zeta_0 + \zeta_1 \text{ParentIncome}_i + \zeta_2 A_i + \zeta_3 \text{Uni}_{it} + \zeta_4 \text{Top}_{it} \tag{25}$$

$$\text{Tr}_{it} = \nu_0 + \nu_1 \text{ParentIncome}_i + \nu_2 A_i + \nu_3 \text{Uni}_{it} + \nu_4 \text{Top}_{it} \tag{26}$$

Finally, assuming a constant discount factor, β, I formulate the student's optimization problem with the Bellman equation for period $t < 12$ and for $T = 12$:

$$V_{it}(\Omega_t) = \max_{s_{it+1}}[v_{it+1}(A_i, s_{it+1}, s_{it}) + \beta\mathbb{E}(V_{it+1}(\Omega_{it+1})|\Omega_{it}, s_{it+1})] \tag{27}$$

$$V_{iT}(\Omega_T) = \sum_{y=T}^{40-T/2} \beta^y \alpha_w(w_{iy} - \text{loan}_{iy}) \tag{28}$$

where $v_{it} = v_{it}^w$ if $s_{it} = 0$ and $v_{it} = v_{it}^s$ otherwise. Labor market is an absorbing state — students cannot return to college once they have entered the labor market. School choice is also subject to an admission constraint, modeled as a cutoff high school test score for freshmen admission and a cutoff GPA for transfer applicants. The cutoffs are chosen empirically, and details can be found in the model appendix B.

3 Identification and Estimation Strategy

In this section, I discuss how parameters of the model are identified and estimated using the Texas administrative data. I first discuss parameters estimated outside of the model. Then, I turn to the identification of parameters estimated in the model using the simulated method of moments.

3.1 Out of Model Identification and Estimation

The credit accumulation equation, the production of course selection coherence, and the equations related to the budget constraint are seen as exogenous "technologies" of the higher education institutions and are identified and estimated out of model.

The credit accumulation and coherence production equations are estimated with the following regressions:

$$\Delta h_{it} = \delta_{0,s} + \delta_{1,s}A_i + \delta_{2,s}\text{PT}_{it} + e_{it}^{h,s}$$

$$\Delta c_{it} = \gamma_{0,s} + \gamma_{1,s} A_i + \gamma_{2,s} PE_i + \gamma_{3,s} \text{PT}_{it} + e_{it}^{c,s}$$

for $s \in \{1, 2\}$.

The share of credits lost during two-to-four transfer is taken from Simone (2014), which uses transcript data from Beginning Postsecondary Students Longitudinal Study (BPS) data. The share of credits lost, κ_{12}, is set to be 20%.

The financial aid and parental transfer equations are estimated with regressions:

$$\text{FA}_{it} = \zeta_0 + \zeta_1 \text{ParentIncome}_i + \zeta_2 A_i + \zeta_3 \text{Uni}_{it} + \zeta_4 \text{Top}_{it} + e_{it}$$

$$\text{Tr}_{it} = \nu_0 + \nu_1 \text{ParentIncome}_i + \nu_2 A_i + \nu_3 \text{Uni}_{it} + \nu_4 \text{Top}_{it} + e_{it}$$

The intertemporal discount rate β is taken to be 0.95 (Gourinchas and Parker, 2003; Arcidiacono, 2004; Laibson, Repetto, and Tobacman, 2007).

3.2 In Model Identification and Estimation

The remaining parameters are divided into four groups: (1) the grade parameters: $\{\lambda_0, \lambda_1, \lambda_2, \lambda_3, \sigma_g, \sigma_0^u\}$; (2) the utility parameters: $\{\alpha_0, \alpha_1, \alpha_2, \alpha_3, \alpha_4, \alpha_5, \alpha_6, \alpha_7, \alpha_8, \alpha_9, q\}$; (3) parameters related to the full-time and part-time wage processes: $\{\beta_{0,d}, \beta_{1,d}, \beta_{2,d}, \beta_{3,d}, \beta_{4,d}, \iota_H^d, \rho_d\}_{d\in\{0,1,2\}}$ and $\{\beta_{0,s}^p, \beta_{1,s}^p, \beta_{2,s}^p, \sigma_{p,s}\}_{s\in\{1,2\}}$;[28] and (4) the share of transfer credit lost expected by students: κ_e .

These parameters are estimated jointly using the simulated method of moments (McFadden, 1989; Pakes and Pollard, 1989), where I minimize:

$$\min_{\Pi} (\hat{\phi}_{\text{data}} - \phi_{\text{model}}(\Pi))' \mathcal{F} (\hat{\phi}_{\text{data}} - \phi_{\text{model}}(\Pi)) \tag{29}$$

where the vector Π contains the remaining parameters described above, $\hat{\phi}_{\text{data}}$ is the vector of empirical moment estimates chosen to identify Π, and ϕ_{model} is the vector of corresponding moments calculated with

[28] $\{\iota_L^1, \iota_L^2, \iota_L^3\}$ are set so that the mean of unobservable labor market productivity is zero

model simulated data. I use the inverse of the variance-covariance matrix of $\hat{\phi}_{\text{data}}$ as the weighting matrix $\mathcal{F}$. The standard errors of $\hat{\Pi}$ is computed using the asymptotic formula for simulated method of moments estimates (Pakes and Pollard, 1989; Duffie and Singleton, 1993). Details of this formula can be found in Appendix C.

The moments included in ϕ is chosen to identify the parameters in Π. In particular, identification is achieved if the collection of empirical moments cannot be produced by two different sets of structural parameters. Guided by this intuition, I choose a set of empirical moments that each has clear links to one or more of the remaining structural parameters. There are four sets of empirical moments included in ϕ.

First, I run auxiliary grade regression following equation (24), and include the coefficient estimates and residual standard error in ϕ.

$$g_{it} = \tilde{\lambda}_0 + \tilde{\lambda}_1 A_i + \tilde{\lambda}_2 \text{Uni}_{it} + \tilde{\lambda}_3 \text{Uni}_{it} \times A_i + e_{it} \tag{30}$$

The coefficient estimates and residual standard error help in identifying the grade parameters. Although the auxiliary regression suffers from the endogeneity of school choice due to the omission of unobservable ability u_i, the selection process can be replicated by the model since school choice is explicitly built in.

Second, I run auxiliary logit regressions on students' school choice:

$$\text{logit}(s_{it} = j) = \tilde{\alpha}_{j0} + \tilde{\alpha}_{j1} A_i + \tilde{\alpha}_{j2} g_{it-1} + \tilde{\alpha}_{j3} 1[s_{it-1} \neq s] + e_{it} \tag{31}$$

for $j \in \{1, 2\}$. $1[s_{it-1} \neq s]$ in the auxiliary school choice logit regressions is an indicator for whether the student was previously enrolled in school type s. Intuitively, students' school choice behaviors as captured in the auxiliary logit regressions help in identifying their preferences reflected in their in-school utility functions. For example, $\tilde{\alpha}_{21}$, the correlation between students' log odds of choosing four-year college and their observable ability, helps in identifying the effect of observable ability on students' in-school utility in four-year colleges, $\alpha_1 + \alpha_3$. $\tilde{\alpha}_{13}$ and $\tilde{\alpha}_{23}$ captures how students' last-period school enrollment affect their current period school choice, and help identify the disutility of adjusting to a new environment, α_7 and α_8.

Third, I run auxiliary Mincer wage regressions for both full-time and part-time wage earnings:

$$\log(w_{it}) = \tilde{\beta}_{0,d} + \tilde{\beta}_{1,d} A_i + \tilde{\beta}_{2,d} g_{it} + \tilde{\beta}_{3,d} e_{it} + \tilde{\beta}_{4,d} e_{it}^2 + e_{it} \tag{32}$$

for $d \in \{0, 1, 2\}$, and

$$\log(w_{it}^p) = \tilde{\beta}_{0,s}^p + \tilde{\beta}_{1,s}^p A_i + \tilde{\beta}_{2,s}^p g_{it} + \epsilon_{it} \tag{33}$$

for $s \in \{1, 2\}$. Similar to the auxiliary grade regressions, although the wage regressions above suffer from the endogeneity of degree attainment, school choice, and part-time work decision, all of these selection processes are built in the model and can therefore be replicated by the model simulations.

Fourth, I include the four-year degree attainment rate of two-to-four transfer students in ϕ to facilitate the identification of κ_e. The intuition of identification is as follows: if κ_e increases, students correctly anticipate more of the credit lost during transfer. As a result, there will be less surprise to credit accumulation following transfer, which leads to less post-transfer adjustments in expected time-to-degree. This will result in less surprise in the expected time and cost to bachelor's degree, and so ultimately leads to higher transfer graduation rate. I use this relation between κ_e and the transfer graduation rate to identify κ_e.

4 Estimation Results and Identification Checks

In this section, I first present the estimation results for the parameters estimated outside of the model. Second, I present the parameter estimates from the simulated method of moments. Third, I provide results of untargeted moment fits and discuss the Andrews, Gentzkow and Shapiro (AGS) sensitivity matrix to verify the sources of identification for the parameters.

4.1 Estimation Outside the Model

The parameters in the credit accumulation equation, the production function for course selection coherence, and the financial aid and parental transfer equations are estimated outside the model. In this subsection, I present the results for each of these parameter estimates following the empirical specifications discussed in section 4.1.

Estimation for the credit accumulation equation and coherence production functions are presented in Table 4. The intercept for credit accumulation is 11.05 in community colleges and 13.49 in four-year colleges.

Students with higher observable ability accumulate slightly more credits. Students who work at-least part-time earn 2.2 credits less in community colleges, and 1.5 credits less in four-year colleges. The intercept for coherence accumulation is 0.11 in community colleges and 0.14 in four-year colleges. Students with higher observable ability and higher parental education accumulate slightly more coherence each semester. Students who work at-least part-time accumulate 0.022 less coherence in community colleges and 0.015 less coherence in four-year colleges.

Estimates for the financial aid and parental transfer equations are presented in Table 5, and the results are as expected: students from wealthier families receive more parental contribution and less financial aid; student with higher ability receive more financial aid and less contribution from parents; and students enrolled in four-year colleges receive more financial aid and more parental contribution than students enrolled in community colleges.

4.2 Simulated Method of Moments

The parameter estimates from the simulated method of moments and the corresponding standard errors are presented in Tables 6, 7, 8, and 9.

The estimates of the grade regression and κ_e are presented in Table 6. The results suggest that a one percentile increase in student ability increases GPA in a community college by 0.007, and GPA in a four-year college by 0.012. For a student at the bottom of the ability distribution, attending a four-year college decreases GPA by 0.21. κ_e is estimated to be 0.51, implying that students only expect around 51% of the amount of credit lost during transfer.

Table 7 presents the parameter estimates of the utility parameters. Since α_w in the linear income utility function is normalized to one, the estimates of the in-school utility parameter can be interpreted in dollar value. For a student at the bottom of the ability distribution, enrolling in a community college for one semester generates utility equivalent to \$2800. For the same student, enrolling in a four-year college decreases utility by \$1188. Unsurprisingly, students with higher academic ability enjoy college enrollment more, and there is a significant complementarity between student ability and college selectivity. A one percentile increase in academic ability increases utility in community colleges by \$22, and utility in four-year colleges by \$43. Students also derive utility from performing well in school. A one unit increase in GPA increases in-school utility by \$1215. Notice that since unobservable ability affects GPA, this may partially reflect the impact of unobservable ability on in-school utility. Moreover, I find that there is a non-trivial utility cost associated with transferring. Two-to-four transfer incurs a \$390 utility cost while four-to-two transfer incurs a \$205 utility cost. Finally, working while enrolling in school has significant utility cost. The utility cost of working at-least part-time is equivalent to \$4282 for community college enrollees and \$5608 for four-year college enrollees.

Finally, Table 8 presents the estimates for labor market parameters. Given the parameter estimates, the constant returns to community college over a high school diploma is 20%, and the income trend with work experience are similar for community college degree holders compared to high school graduates. The returns to community college estimated is slightly higher but similar to the returns estimated in Mountjoy (2019) using the same dataset. The returns is expected to be higher, since Mountjoy (2019) estimates the increase in earnings for individuals induced to community college by closer access, whereas here I estimate the returns for all individuals regardless of their closeness to community colleges. In any case, the similarity in estimated returns to degree is comforting.

The estimates presented in Table 9 show that community college students earn more when working part-time during college. This reflects the fact that community college students are more likely to work part-times during college and tend to work longer hours when working while enrolled.

The standard errors indicate that almost all estimates from the simulated method of moments are significant at the 1 percent level. The only exceptions are the estimates for the utility cost of transfer, which are significant at the 5 percent level for two-to-four transfers and significant at the 10 percent level for four-to-two transfers. As reported in Tables 10, 11, and 12, the estimated model fits the targeted empirical moments well.

4.3 Untargeted Moment Fits and AGS Sensitivity Matrix

To validate the structural estimation, I first check whether the structural model can match empirical moments that are not targeted in the estimation stage. In addition, I present the AGS sensitivity matrix to examine whether the structural parameters are identified by the expected sources of variations.

The first set of out-of-sample fits are presented in Figure 4 and examines the model's ability to capture students' school enrollment decisions. The figure presents the share of college students enrolled in community colleges versus in four-year colleges. While the overall trends of the school choices fit well, there are some difference in the details. In particular, compared to the observed data, the share of students enrolled in community colleges declines faster over semester of enrollment, while the share of students in four-year colleges grows faster.

The second out-of-sample fit examines whether the model is able to correctly capture students' sensitivity to tuition variations. I attempt to match an untargeted moment from the regression discontinuity exercise using CCTD borders. In particular, I match the estimate ω, which is the effect of the CCTD tuition discount on the probability of students' choosing community colleges as their first college after high school. In the simulation of the structural model, the chance of choosing a community college as the first college is 2.7 percentage points higher for students in a CCTD. This matches well with the empirical estimate of $\omega = 0.021$.

Finally, I present the AGS sensitivity matrix to verify the source of identification (Andrews, Gentzkow, and Shapiro, 2017). The AGS sensitivity matrix Λ is defined as

$$\Lambda = -(G'WG)^{-1}G'W$$

where W is the probability limit of the weight matrix for the matched moments, and G is the Jacobian of the probability limit of the matched moments $\hat{g}(\theta)$ at the true value θ_0. Intuitively, G measures how the matched moments change with the structural parameters, and so Λ is a local approximation to the mapping from empirical moments to estimated structural parameters. The estimate for Λ is calculated with plug-in estimates of G and W, which are side products from the standard error calculation, making the calculation essentially costless computationally. Statistics from the estimated sensitivity, $\hat{\Lambda}$, are presented in Appendix C, and show that the sources of identification are largely as outlined in section 4.2.

5 Counterfactual Simulations

Using the estimated model, I simulate the school choices and educational outcomes of a large number of students that are heterogenous in observable and unobservable ability, parental income, and parental education. I also simulate students' decisions and outcomes under different counterfactual settings. These simulated data allow me to answer the three main research questions: 1. what are the impacts of a free community college policy? 2. how important is the option to transfer between two- and four- year colleges? 3. can improvements in credit transfer improve student outcomes?

I first examine the effect of counterfactually reducing community college tuition to zero. Unsurprisingly, the policy increases average student welfare and associate degree attainment. In particular, the policy increases average student welfare by $7000 and the share of high school graduates that attain associate degrees by 7 percentage points (24% increase). However, free community college significantly reduces bachelor's degree attainment and average life-time income of students. The share of high school graduates that receive bachelor's degree drops by 7 percentage points (21% decrease), while the average discounted life-time income of students drops by $6000 (1% decrease).

What explains the negative impacts free community college has on bachelor's degree attainment and life-time income? First, free community college reduces community college students' need to work while enrolled, and therefore increases the short-term utility of enrolling in two-year programs. This induces students to trade long-run income for short-term utility and diverts students away from four-year programs, which have higher long-run returns. Second, the diversion effect subjects more students to the imperfect information in the credit transfer process. This further reduces the bachelor's degree attainment.

To verify these mechanisms, I perform a counterfactual exercise with free community colleges and no imperfect information in the credit transfer process. Under this setting, the second of the two mechanisms above will no longer exist, and should result in less diversion and decline in life-time income. The results from this counterfactual confirms the hypothesis: bachelor's degree attainment decreases by only 1 percentage point, and average life-time income decreases only by $3000. Student's welfare also increases significantly more compared to the counterfactual with free community college but with imperfect information.

Figure 5 presents the distribution of average life-time income across parental income distribution for the base-line simulation, the counterfactual with free community college, and the counterfactual with free community college and perfect information. The figure shows that students from middle income family experience the most reduction in life-time income as a result of free community college. This is expected, since students at the bottom of the parental income distribution seldomly have the financial resources to transfer from two-year to four-year programs, whereas students at the top of the distribution rely less on part-time earnings to finance their college tuition and are therefore less sensitive to tuition changes in community colleges.

Intuition suggests that a proportional tuition reduction in both two-year and four-year college tuition could avoid the diversion caused by free community colleges. Back of the envelope calculation suggests that reducing community college and four-year college tuition by 11% costs the same as providing free community colleges. I perform a counterfactual simulation with tuition for all colleges reduced by 11%, and find significant improvements in student outcomes. The proportional tuition reduction increases bachelor's degree attainment by 8 percentage points, average life-time income by $9,000 (1.8% increase) and overall student welfare by $9,000 (1.4% increase). The distribution of average life-time income change presented in Figure 6 suggests that students from all income level households experience improvements in life-time income.

Proponents for free community colleges may stress the redistributive purpose of such policy, given the overrepresentation of lower socioeconomic status and/or minority students in community colleges. In this spirit, I implement a counterfactual policy experiment in which I proportional reduce community college and four-year college tuition by 22% for students from the bottom 50% o the income distribution, and hold tuition fixed for the other students. The effect of this policy experiment on average life-time income across the income distribution is presented in Figure 7 and suggests that this policy would perform significantly better compared to free community colleges in promoting upward mobility.

Besides free community college, elimination of credit lost during transfer and transparency of the credit transfer rules are two popular policies among community college researchers and administrators. Using the estimated model, I implement counterfactual policy experiments to analyze the effect of eliminating credit lost during transfer ($\kappa_{12} = 0$) and providing complete transparency in credit transfer rules ($\kappa_e = 1$). The results shows that eliminating credit lost during transfer increases community college to four-year college transfer rate by 2 percentage points (5% increase), transfer graduation rate by 13 percentage points (21% increase), and overall bachelor's degree attainment by 7 percentage points (21% increase). Providing perfect information on transfer credit lost, which can be achieved for example by high quality pre-transfer advising, increases transfer graduation rate by 7 percentage points (11% increase) and overall bachelor's degree attainment by 4 percentage points (12% increase). As expected, perfect transparency in transfer credit lost reduces community college to four-year college transfer rate, since students (correctly) expect more credit lost during transfer.

Finally, I examine the overall importance of the option to transfer between community colleges and four-year colleges by shutting down the possibility to transfer between institutions. The counterfactual exercise suggests that overall bachelor's degree attainment would decrease by 11 percentage points (33% decrease) if transfer options were not available. This large impact reflects the fact that 78% of Texas bachelor's degree graduates have previously enrolled in community colleges (Jenkins, 2013). Moreover, average welfare of students will decrease by a modest $2,000 (0.3% decrease) if no transfer options were available.

6 Conclusions

The relation between community colleges and four-year colleges are characterized by frequent student transfer in both directions. This paper combines rich administrative data from the state of Texas and a model on students' school choices to study the design of higher education policies, including free tuition policies, while being mindful of this tight interconnection between institutions.

I find that free community colleges may not be the best policy to provide the promise of upward mobility, and may instead lead to a trap of promoting associate degree programs instead of the more lucrative bachelor's degree programs. Although free community colleges would improve student welfare and associate degree attainment, it would also decrease bachelor's degree attainment and reduce average life-time income. Instead, I find that a proportional tuition reduction that reduces both community college and four-year college tuitions can cost the same for taxpayers while providing significant improvements in bachelor's degree attainment and life-time income improvements.

In addition, I find significant friction in the community college to four-year college transfer pathways due to imperfect information on credit transfer rules. Eliminating this friction would lead to large improvements in bachelor's degree attainment for transfer students. Relatedly, eliminating credit lost during transfer altogether lead to notable improvements in bachelor's degree attainment for transfer students as well as transfer rate of community college students. These results suggest that policies such as comprehensive credit articulation and improved transfer advising in community colleges can have important impact on transfer student outcomes.

Above all, this paper emphasizes the importance of considering community colleges and four-year colleges as components of an ecosystem tightly connected by student transfers. Counterfactual simulation suggests that eliminating the options to transfer between institutions would lead to significant decrease in bachelor's degree attainment and modest decline in student welfare. From a policy perspective, acknowledging the interconnection between institutions can help avoid traps that are not visible from a single institution's point of view.

7 Tables and Figures in Chapter 2

	Average Standardized Test %	Average Parental Income	Share Minority
Community College	57	$67,000	45%
4-Yr College	72	$76,000	36%

Table 1: Summary Statistics for Community Colleges vs. Four-Year Colleges

	(1)	(2)
	Logit	
School	Community College	4-Yr College
Test Score Percentile	-0.004***	0.006***
	(0.000)	(0.000)
Prior GPA	0.096***	1.500***
	(0.001)	(0.002)
Prior Credit	-0.004***	0.013***
	(0.000)	(0.000)
Prior 4-Yr Enrollment	-3.424***	4.160***
	(0.003)	(0.003)
Observations	6,639,304	6,639,304

Table 2: School Choice Logit Regressions

	(1) First CC	(2) Length of Enrollment	(3) Credits Accumulated	(4) Ever BA	(5) Ever Degree	(6) Ever BA Degree
CCTD	0.021***	0.296***	2.290***	-0.014***	0.004**	-0.006***
	(0.002)	(0.028)	(0.277)	(0.001)	(0.002)	(0.001)
Distance	0.000***	0.003***	0.059***	-0.000***	0.000***	-0.000***
(KM)	(0.000)	(0.001)	(0.007)	(0.000)	(0.000)	(0.000)
Constant	0.981***	5.797***	34.990***	0.009	0.091***	0.019**
	(0.012)	(0.179)	(1.754)	(0.008)	(0.011)	(0.008)
Ethnicity	Yes	Yes	Yes	Yes	Yes	Yes
Gender	Yes	Yes	Yes	Yes	Yes	Yes
Test Score	Yes	Yes	Yes	Yes	Yes	Yes
N	243,104	206,456	206,456	206,456	243,104	243,104
Sample Restriction	All College Freshmen	All CC Freshmen	All CC Freshmen	All CC Freshmen	All College Freshmen	All College Freshmen

Table 3: Regression Discontinuity Estimates

	(1)	(2)	(3)	(4)
Intermediate Outcome	Credit Accumulation		Coherence Accumulation	
School	Community College	4-Yr College	Community College	4-Yr College
Observable Ability	0.003***	0.003**	0.0001***	0.0001***
	(0.000)	(0.000)	(0.000)	(0.000)
Part-Time Work	-2.211***	-1.501***	-0.022***	-0.015***
	(0.006)	(0.001)	(0.000)	(0.001)
Parental Education	-	-	0.011***	0.010***
			(0.002)	(0.001)
Constant	11.051***	13.492***	0.111***	0.135***
	(0.002)	(0.005)	(0.001)	(0.000)
Observations	17,841,730	3,383,791	17,841,730	3,383,791

Table 4: Credit and Coherence Production Functions

	(1)	(2)
Dependent Variable	Family Contribution	Financial Aid
Observable Ability	8.60***	11.98***
(Percentile)	(0.38)	(16.73)
Parental Income	1889.06***	-629.64***
(Decile)	(3.06)	(1.52)
4-Yr College	350.31***	1616.84***
	(17.61)	(8.75)
Constant	-4852.21***	4240.27***
	(33.67)	(16.73)
Observations	465,552	465,552

Table 5: Financial Aid and Parental Contribution Production Functions

	Estimate	Standard Error
λ_0 - Constant	2.53***	(0.02)
λ_1 - Observable Ability	0.006***	(0.0004)
λ_2 - 4-Yr College	-0.30***	(0.04)
λ_3 - Ability$\times$4-Yr	0.005***	(0.001)
κ_e - Imperfect Info	0.51***	(0.02)

Table 6: Parameter Estimates: Grade Equation

	Estimate	Standard Error
α_0 - Constant	2802.51***	(782.4)
α_1 - 4-Yr College	-1188.62***	(204.1)
α_2 - Observable Ability	22.45***	(2.1)
α_3 - Ability$\times$4-Yr	21.43***	(5.4)
α_4 - Part-Time CC	4281.66***	(62.5)
α_5 - Part-Time 4-Yr	5607.88***	(30.9)
α_6 - GPA	1215.23***	(181.7)
α_7 - 2-4 Transfer	390.54**	(154.9)
α_8 - 4-2 Transfer	204.88*	(124.0)
α_9 - High Type	2341.83**	(1008.5)

Table 7: Parameter Estimates: Utility Function

Log Full-Time Wage	(1)	(2)	(3)
Degree	High School	Community College	4-Yr College
Observable Ability (Percentile)	0.002*** (0.000)	0.0020*** (0.0001)	0.0025*** (0.0001)
College GPA	-	0.005*** (0.001)	0.025*** (0.007)
Experience	0.028*** (0.003)	0.028*** (0.002)	0.040*** (0.003)
Experience^2	-0.0005*** (0.0001)	-0.0004*** (0.0001)	-0.0008*** (0.0002)
Constant	10.25*** (0.30)	10.45*** (0.40)	10.50*** (0.32)

Table 8: Parameter Estimates: Full-Time Wage Equations

Log Part-Time Wage	(1)	(2)
School	Community College	4-Yr College
Observable Ability (Percentile)	0.001*** (0.000)	0.002*** (0.000)
College GPA	0.022*** (0.003)	0.020*** (0.004)
Constant	8.601*** (0.520)	8.121*** (0.633)

Table 9: Parameter Estimates: Part-Time Wage Equations

Grade Coefficients	Data	Model
$\tilde{\lambda}_0$ - Constant	2.50	2.39
$\tilde{\lambda}_1$ - Ability	0.005	0.005
$\tilde{\lambda}_2$ - 4-Yr College	-0.08	-0.04
$\tilde{\lambda}_3$ - Ability$\times$4-Yr	0.0009	0.0008
σ^g - Residual SE	0.88	0.83

Table 10: Targeted Moment Fit: Grade Regression

Logit Coefficients	Community College		4-Yr College	
	Data	Model	Data	Model
$\tilde{\alpha}_0$ - Constant	1.6	1.0	-7.0	-7.3
$\tilde{\alpha}_1$ - Ability	-0.004	-0.015	0.06	0.03
$\tilde{\alpha}_2$ - GPA	0.1	0.7	1.5	2.1
$\tilde{\alpha}_3$ - Previous 4-Yr	-3.6	-3.9	4.2	4.9

Table 11: Targeted Moment Fit: School Choice Logit

Full-Time Log-Wage	High School		Community College		4-Yr College	
	Data	Model	Data	Model	Data	Model
$\tilde{\beta}_0$ - Constant	10.24	10.24	10.48	10.48	10.54	10.53
$\tilde{\beta}_1$ - Ability	0.0021	0.0020	0.0021	0.0020	0.0026	0.0025
$\tilde{\beta}_2$ - GPA	-	-	0.003	0.002	0.027	0.026
$\tilde{\beta}_3$ - Exp	0.027	0.026	0.029	0.028	0.038	0.039
$\tilde{\beta}_4$ - Exp^2	-0.0005	-0.0005	-0.0004	-0.0004	-0.0007	-0.0007

Table 12: Targeted Moment Fit: Full-Time Wage Regressions

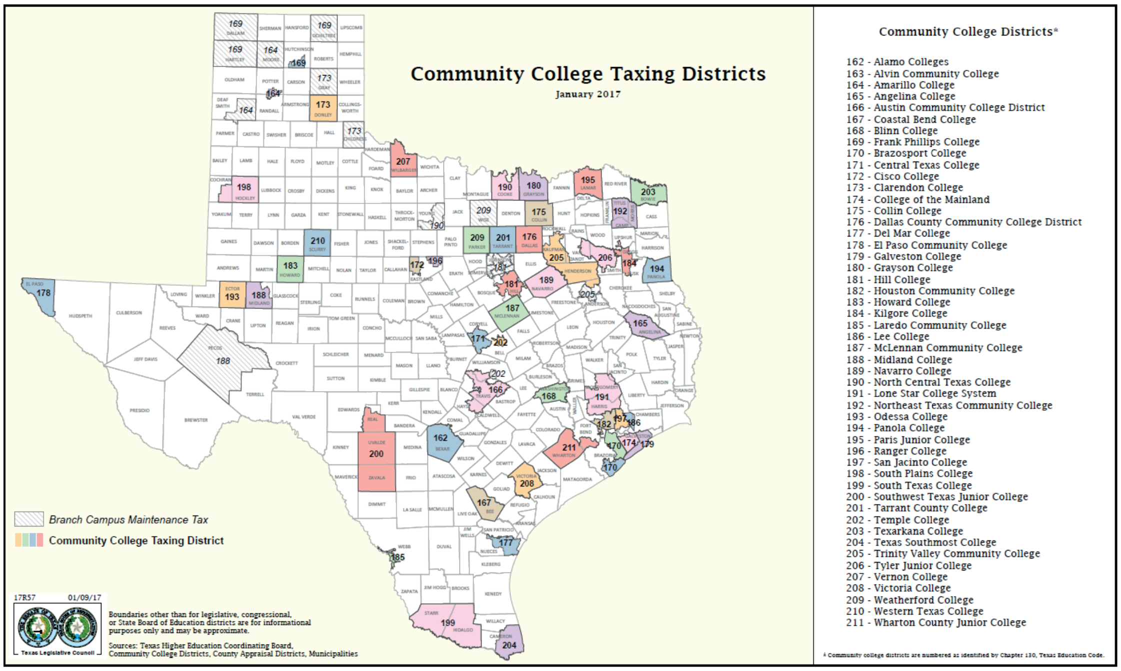
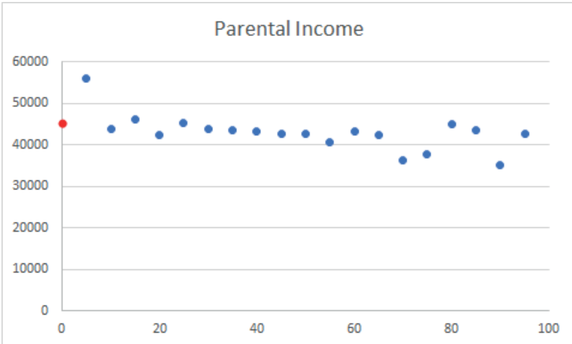
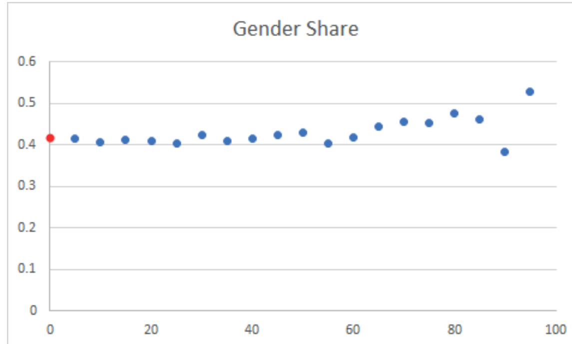


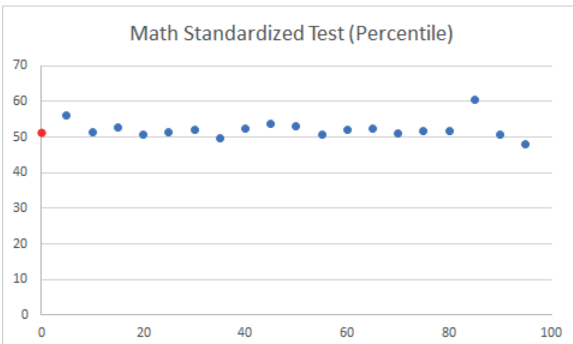
Figure 1: CCTD Distribution



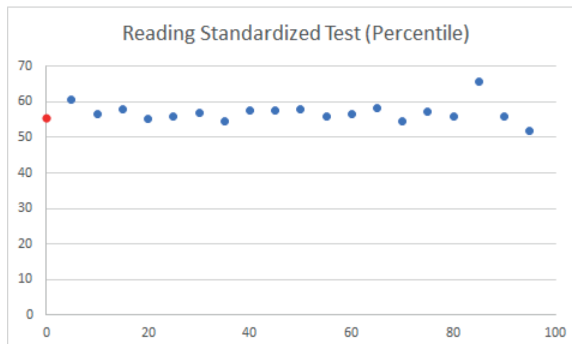
(a) Average Parental Income



(b) Gender Share



(c) Average Math Score



(d) Average Reading Score

Figure 2: Balance Test

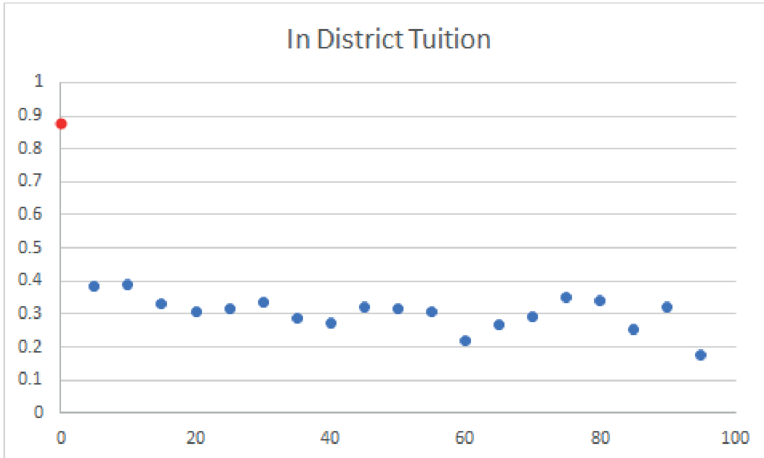
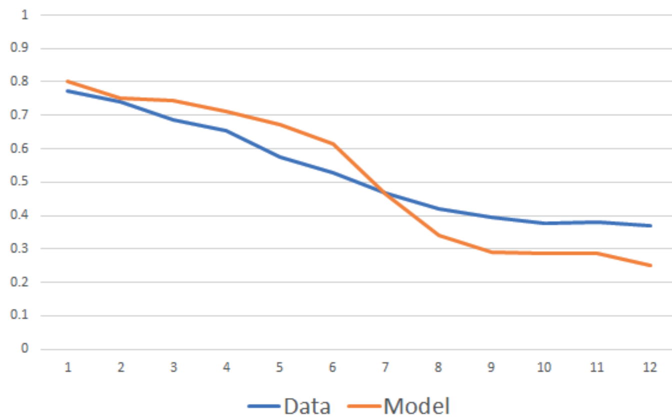
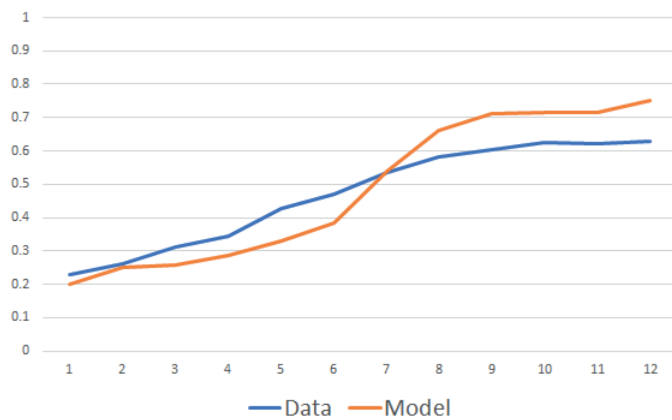


Figure 3: RD First Stage



(a) Share Enrolled in CC



(b) Share Enrolled in 4 Yr

Figure 4: Time Trend for School Choice

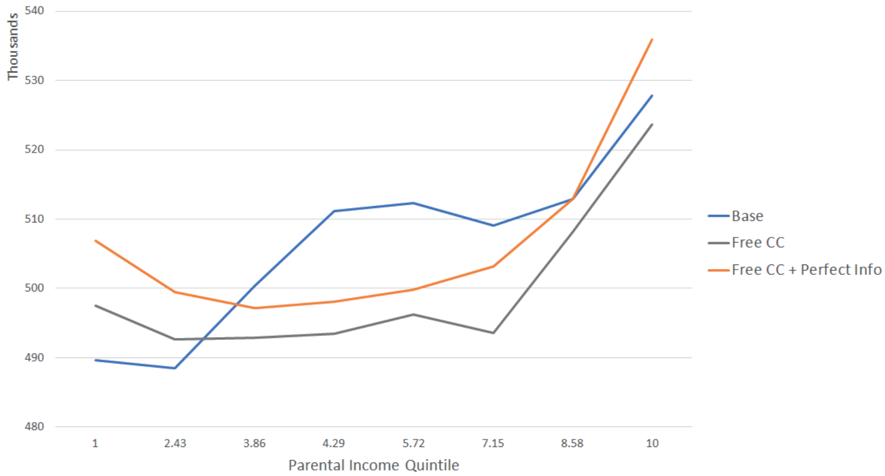


Figure 5: Counterfactual Simulation for Free CC Policies

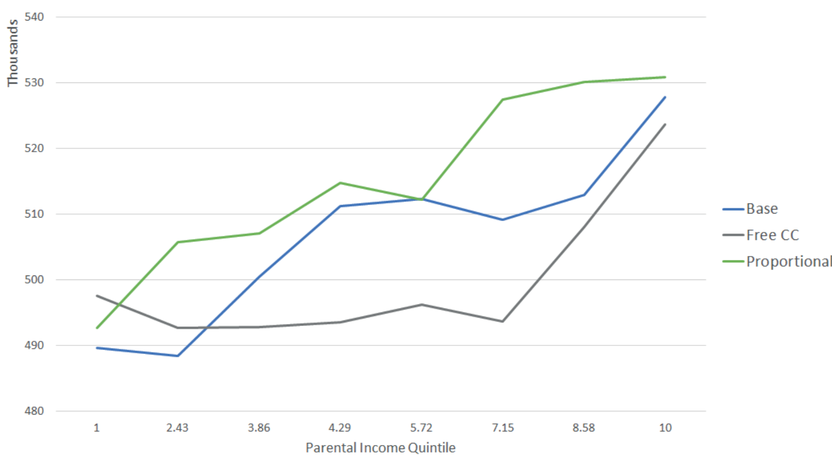


Figure 6: Counterfactual Simulation for Proportional Tuition Reduction

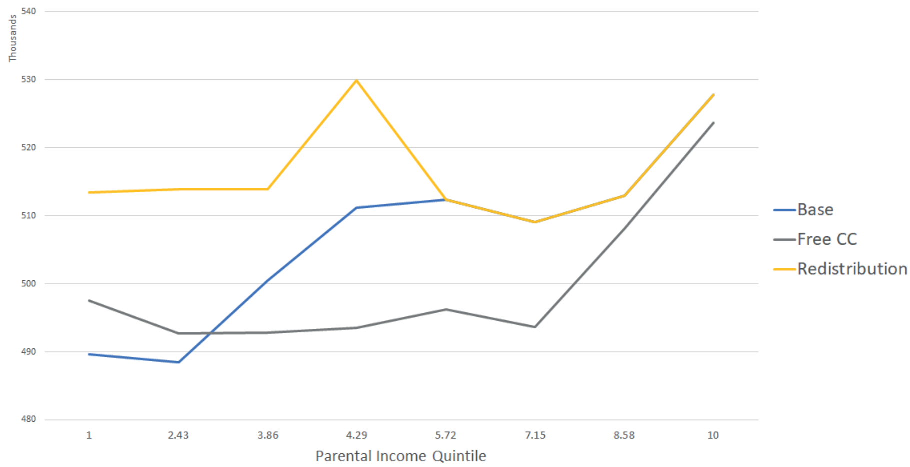


Figure 7: Counterfactual Simulation for Redistributive Proportional Tuition Reduction

8 Appendix to Chapter 2: Model and Identification Details

Admission Constraint

School choice is subject to an admission constraint, modeled as a cutoff high school test score for freshmen admission and a cutoff GPA for transfer applicants. The freshmen admission cutoff is set as the 25th percentile test score of all admitted students for non-selective four-year colleges and the 50th percentile test score of all admitted students for selective four-year colleges. As a result of this admission rule, freshmen applicants who score above the 60th percentile in the high school standardized score are granted admission to non-selective four-year colleges and those who score above the 89th percentile in the high school standardized score are granted admission to selective four-year colleges. Notably, the calculated admission criteria for selective four-year colleges matches almost exactly with the Texas Top 10 policy, which guarantees Texas high school graduates ranking in the top 10% of their high school admission to any Texas public institutions. The transfer admission cutoff is calculated in a similar manner. The cutoff GPA for transfer admission to non-selective four-year colleges is set as the 25th percentile GPA of all students admitted for transfer admission, which is 2.5. Community colleges are open admission.

Identification of Parameters

Grade Parameters and Population Variance of Unobservable Ability

The grade parameters are identified by the parameter estimates of the following two auxiliary regressions:

$$g_{it} = \tilde{\lambda}_0 + \tilde{\lambda}_1 A_i + \tilde{\lambda}_{NS}\text{Uni}_{it} + \tilde{\lambda}_S\text{Top}_{it} + \tilde{\lambda}_2\text{Uni}_{it} \times A_i + \tilde{\lambda}_3\text{Top}_{it} \times A_i + e^1_{it}$$

where $e^1_{it} \sim N(0, \tilde{\sigma}^g_1)$ and $e^2_{it} \sim N(0, \tilde{\sigma}^g_1)$.

$\tilde{\lambda}_0$ and λ_0 both measure the average GPA in community colleges, and so $\tilde{\lambda}_0$ identifies λ_0. $\tilde{\lambda}_1$ and λ_1 both measure the average effect of observable ability on GPA, and so $\tilde{\lambda}_1$ identifies λ_1. $\tilde{\lambda}_{NS}$ and λ_{NS} both measure the difference in average GPA between non-selective four-year colleges and community colleges, while $\tilde{\lambda}_S$ and λ_S both measure the difference in average GPA between selective four-year colleges and community colleges. As a result, $\tilde{\lambda}_{NS}$ and $\tilde{\lambda}_S$ identify λ_{NS} and λ_S, respectively. Similarly, while both $\tilde{\lambda}_2$ and λ_2

measure the interaction effect of non-selective four-year college enrollment and observable ability on GPA, both $\tilde{\lambda}_3$ and λ_3 measure the interaction effect of selective four-year college enrollment and observable ability on GPA. Thus, $\tilde{\lambda}_2$ and $\tilde{\lambda}_3$ identify λ_2 and λ_3 respectively. Importantly, although the auxiliary regression equation (20) suffers from endogeneity of school choice and sample selection issues, the endogenous school choice and enrollment decisions are also incorporated into the model. Therefore, the endogeneity and sample selection bias in the simulated $\tilde{\lambda}$ and the empirical $\tilde{\lambda}$ will cancel each other out, and the remaining causal effect will be used to identify the structural parameters λ.

Comparing the auxiliary grade regression to the structural grade equation, one can see that e^1_{it} corresponds to a combination of unobservable ability, u_i, and true grade shock, ϵ^g_{it}. As a result, $\tilde{\sigma}^g_1$ cannot separately identify the population standard deviation of unobservable ability, σ^u_0, and the standard deviation of grade shock, σ_g. To separately identify σ^u_0 and σ_g, I use the sensitivity of next-period school choices to the current-period GPA. Due to the Bayesian updating structure, students' school choices are more strongly affected by previous GPA if σ^u_0 is larger than σ_g, as GPA will convey more information on students' unobservable academic ability. In specific, I include students' current-period GPA in the auxiliary school choice logits.

Utility Parameters

The utility parameters, $\{\alpha_0, \alpha_1, \alpha_2, \alpha_3, \alpha_4, \alpha_{NS}, \alpha_S\}$, are identified by the coefficients of a series of schol choice auxiliary regressions. The school choice auxiliary regressions used for identification are the following three logit regressions:

$$\text{logit}(\tilde{s}_{it} = 1) = \beta_{10} + \beta_{11} A_i + \beta_{12} 1[\tilde{s}_{it-1} \neq 1] + \boldsymbol{\beta_1 X_{it}} + e_{it}$$

$$\text{logit}(\tilde{s}_{it} = 2) = \beta_{20} + \beta_{21} A_i + \beta_{22} 1[\tilde{s}_{it-1} \neq 2] + \boldsymbol{\beta_2 X_{it}} + e_{it}$$

$$\text{logit}(\tilde{s}_{it} = 3) = \beta_{30} + \beta_{31} A_i + \beta_{32} 1[\tilde{s}_{it-1} \neq 3] + \boldsymbol{\beta_3 X_{it}} + e_{it}$$

where $1[\tilde{s}_{it-1} \neq j]$ is an indicator for whether or not the student was previously enrolled in school j. In other words, $1[\tilde{s}_{it-1} \neq j] = 1$ would indicate a transfer student. $\boldsymbol{X_{it}}$ is a vector of control variables, and include the student's previous period GPA, accumulated semester credit hours, and academic standing. Since the regression examines the effect of prior college enrollment history and performance in school choices, the three logit regressions are performed on the subset of students who are already enrolled in college, and does not examine the initial school choice of high school graduates.

Recall that α_0 represents the in-school utility of a student enrolled in a community college with $A_i = 0$. Holding everything else fixed, if α_0 increases, the overall enrollment in community colleges would increase. Since β_{10} estimates the overall enrollment in community colleges, it also identifies α_0. α_{NS} and α_S denote the (dis)utility of enrolling in a non-selective four-year and a selective four-year college compared to community colleges. Therefore, if α_{NS} and α_S increases, the overall share of students enrolled in non-selective four-year and selective four-year colleges should increase. Since the overall share of students enrolled in non-selective four-year and selective four-year colleges are estimated by β_{20} and β_{30}, these two empirical moments identify α_{NS} and α_S respectively.

While α_1 defines the increase in the utility for enrolling in a community college when observable ability A_i increases by one unit, β_{11} estimates the increase in log odds in enrolling in a community college when A_i increases by one unit. Since, holding all else fixed, an increase in utility leads to higher enrollment, α_1 is identified by β_{11}. Similarly, while $\alpha_1+\alpha_2$ defines the increase in the utility for enrolling in a non-selective four-year college when observable ability A_i increases by one unit, β_{21} estimates the increase in log odds in enrolling in a non-selective four-year college when A_i increases by one unit. Therefore, β_{21} identifies $\alpha_1 + \alpha_2$. An analogous argument shows that β_{31} identifies $\alpha_1 + \alpha_3$, so that β_{11}, β_{21}, and β_{31} jointly identify α_1, α_2, and α_3.

Finally, since α_4 denotes the disutility associated with transfer, an increase in α_4 should discourage transfer, resulting in lower β_{12}, β_{22}, and β_{32}. As a result, β_{12}, β_{22}, and β_{32} jointly identify α_4.

9 Appendix to Chapter 2: Standard Errors of Simulated Method of Moment Estimator and AGS Matrix

The standard errors of the simulated method of moments (SMM) estimators are computed using the asymptotic normal distribution derived in Pakes and Pollard (1989) and Duffie and Singleton (1993). In particular, they show that the SMM estimator, $\hat{\theta}$, has a variance-covariance matrix of:

$$\Omega_\theta = (G_\theta' W G_\theta)^{-1} G_\theta' W [\Omega_g + \frac{N_d}{N_s}\Omega_g + G_\chi \Omega_\chi G_\chi'] W G_\theta (G_\theta' W G_\theta)^{-1}$$

where G_θ is the gradient matrix of the moment conditions with respect to θ, and G_χ is the gradient matrix of the moment conditions with respect to the first-stage parameters χ. Ω_g and Ω_χ are the variance-covariance matrices of the second-stage moment conditions and of the first-stage parameter estimates. N_d and N_s are the empirical sample size and the simulation sample size. I treat χ as if it were known with certainty, so that $G_\chi = 0$.

I use $W = \Omega_g^{-1}$, and estimate W and Ω_g from the data. The derivatives in the gradient matrix G_θ is approximated with numerical derivatives. These estimates are then plugged into the formula above to compute the variance-covariance matrix of $\hat{\theta}$.

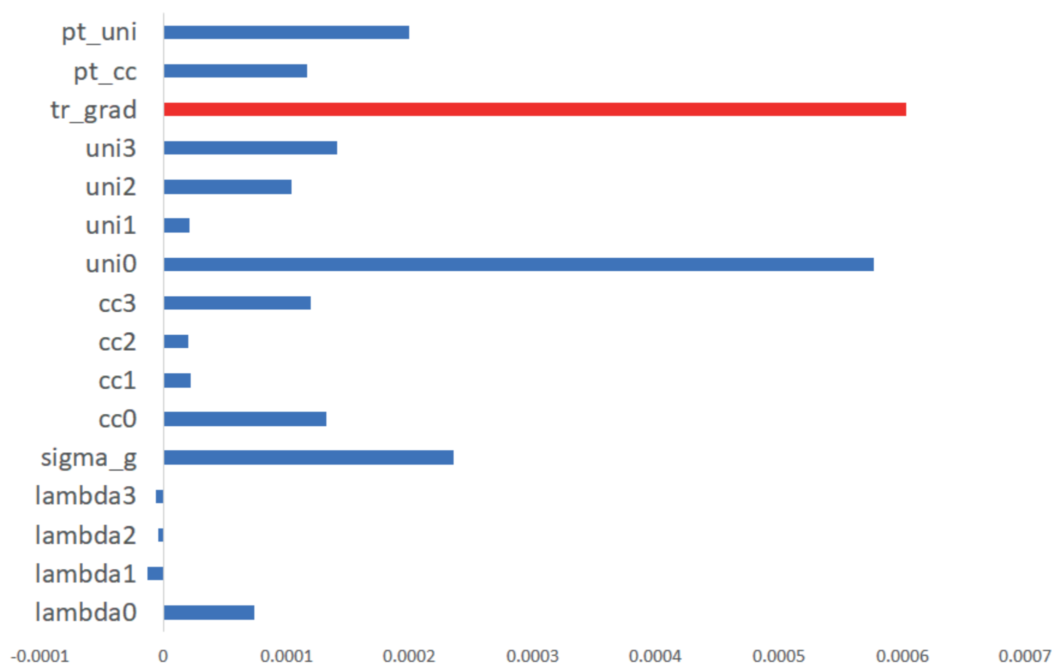


Figure 8: AGS Matrix: κ_e

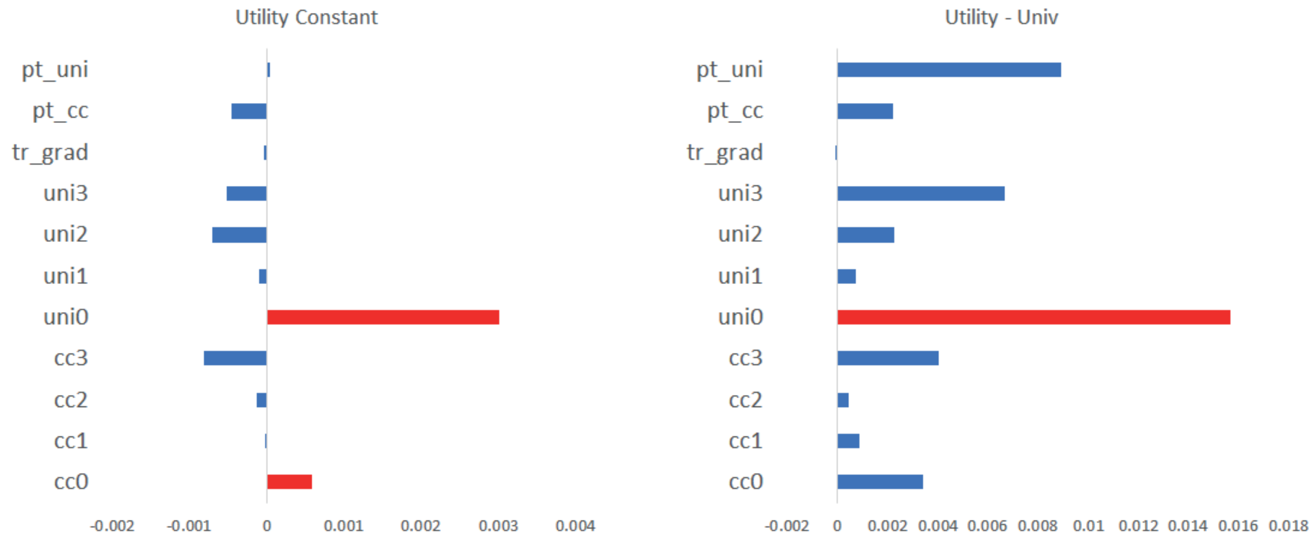


Figure 9: AGS Matrix: Utility Constants — α_0 and α_1

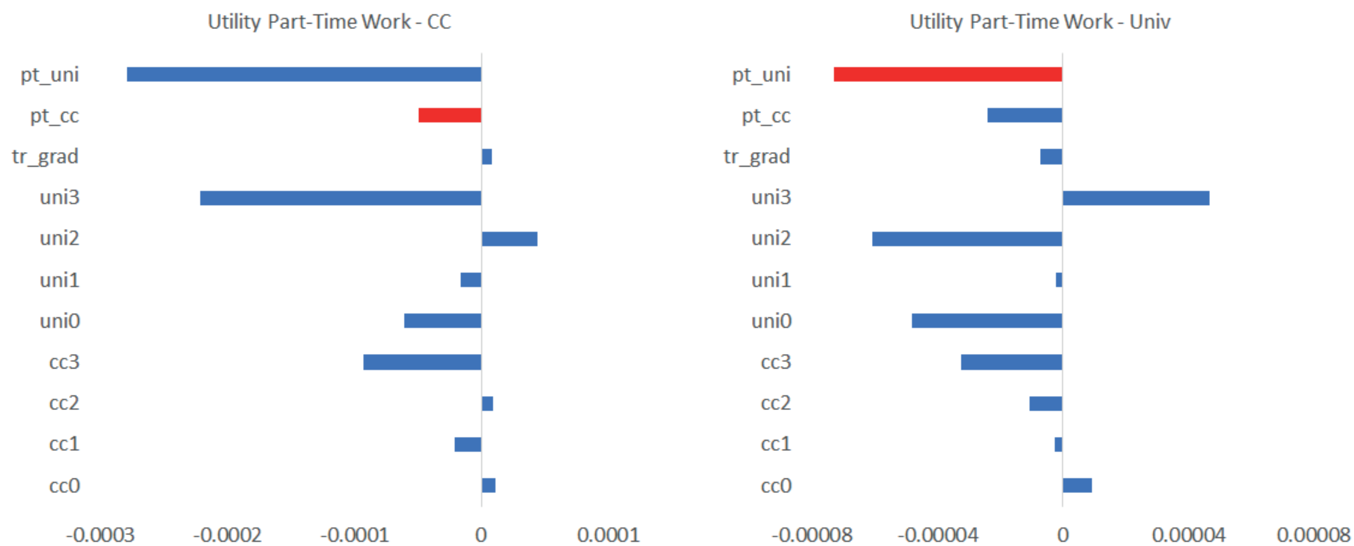


Figure 10: AGS Matrix: Part-Time Utility Costs — α_5 and α_6

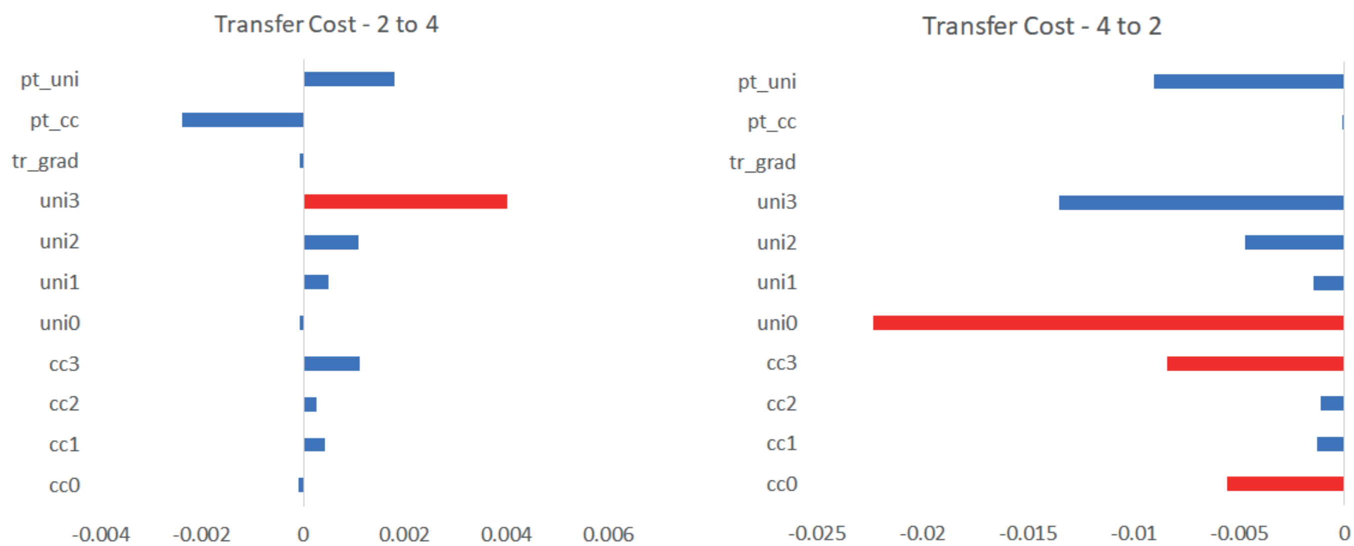


Figure 11: AGS Matrix: Transfer Costs — α_7 and α_8

The sensitivities of the utility parameters related to ability are presented in Figure 8. Panel A shows that if the coefficient for ability in the community college enrollment logit is underestimated, and if that in either of the four-year college enrollment logits is overestimated, then the extra utility for high ability students will be overestimated. Although the negative correlation between the coefficient for ability in the community college logit and the parameter is counterintuitive, the results show that the overall effect of ability on college enrollment is likely positive, which aligns with the identification strategy. Panel B shows that if the coefficient for ability in the non-selective four-year college enrollment logit is overestimated, then the additional utility for high ability students in non-selective four-year colleges, α_2, would be overestimated as well. Similarly, panel C shows that if the coefficient for ability in the selective four-year college enrollment logit is overestimated, then the additional utility for high ability students in selective four-year colleges, α_3, would be overestimated. Both results from panel B and panel C aligns perfectly with the identification strategy.

Finally, the sensitivity of the parameter for utility cost during transfer is presented in Figure 6. As expected, the cost of transfer would be overestimated if the instances of transfer is underestimated and if the instances of persistence is overestimated. In particular, α_4 would be overestimated if the coefficients on previous enrollment in four-year colleges in the community college enrollment logit are underestimated. α_4 would also be overestimated if the coefficients on previous non-selective four-year enrollment in the selective four-year enrollment logit is underestimated and if the coefficient on previous selective four-year enrollment in the selective four-year enrollment logit is overestimated. Unexpectedly, the coefficient on previous selective four-year enrollment in the non-selective four-year enrollment logit is positively correlated with the utility cost of transfer. However, since the size of the sensitivity is much smaller than with the other moments, this is likely due to noise in the estimation process and would not affect the overall validity of the identification strategy.

Chapter 3: Discrimination Against Community College Graduates — Evidence from a Labor Market Audit Study

Community colleges perform an integral role in the US higher education system by enrolling more than 40% of US undergraduates (Ma and Baum, 2015). These institutions have significantly lower tuitions compared to their four-year counterparts, and offer courseworks that can be transferred to four-year colleges. As a result, a large proportion of high school graduates choose to first enroll in a community college before later transferring to a four-year college to complete their bachelor's degree study. Moreover, students that choose this pathway to a bachelor's degree are more likely to be minorities and to be from lower socioeconomic backgrounds (Bailey, Jenkins, and Leinbach, 2005).

Despite the prevalence of community colleges and the popularity of the two-to-four pathway to bachelor's degrees, community college graduates have been subject to widespread discrimination (Huffington Post, 2017; Community College Review, 2019; Higher Education Today, 2019). Some of the most common misconceptions about community colleges are that they are less rigorous than their four-year counterparts, and that students attend community colleges because they cannot get into four-year colleges. The discrimination against community college graduates could have several important ramifications. First, it could reduce the motivation of community college students to devote to their coursework, and could also discourage attendance at two-year colleges in the first place. This could potentially lead to genuine differences between community college graduates and four-year college graduates, and hence creating a self-fulfilling prophecy (Bertrand and Duflo, 2016). Second, the over-representation of minorities and lower socioeconomic status students in two-year colleges implicate that unfair treatment towards community college students could significantly hinder upward socioeconomic mobility. Third, the discrimination could have general equilibrium effects, where high-skilled occupations become increasingly populated by individuals with bias against community coleges, leading to even more discrimination against community college graduates in these professions.

In this paper, I answer two questions related to the discrimination against community college graduates. First, do employers discriminate against community college graduates, conditional on the community college graduates having attained bachelor's degrees? Second, if there is discrimination against community college graduates, what is the nature of the discrimination? Is the discrimination based on rational expectation on individuals' qualification or irrational and biased prior on community college graduates' ability? If employers have biased prior, do they update their biased prior according to additional information on applicants' ability?

Before proceeding further, it is important to clarify some terminologies related to discrimination. An employer *discriminates* against individuals with a certain characteristic if, holding everything else constant, the employer systematically offers less opportunity to individuals with that characteristic. Discrimination could be due to *statistical discrimination*, in which case employers take a certain observable characteristic as a signal for applicants' productivity. In the setting of this paper, for example, employers may take previous enrollment in community college as a signal for lower academic accomplishment and/or lower socioeconomic background, which affect productivity. Discrimination could also be due to *biased prior*, in which case unequal treatment is due to employers' belief on a group of individuals' productivity that is not based on evidence. This is analogous to the taste-based discrimination in the classical dichotomy of discrimination (Becker, 1957).[29]

I implement a national labor market audit study to test the existence of discrimination against community college graduates. I generate a large pool of resumes that are representative of recent four-year college graduates seeking employment in either accounting positions or sales and marketing positions. I randomly assign community college experience, college GPA, and four-year college selectivity to each resume. Consequently, the only difference between applicants with and without community college experience is where they completed their first two years of college — all job applicants in the study have a bachelor's degree. These resumes are then sent to job openings on one of the largest online job platforms in the US. A total of 1350 accounting resumes and 2285 sales resumes are submitted, and the callback results of the applications are recorded.

Given the randomization design, difference in callback rates between students with and without community college experience is taken as evidence for the existence of discrimination against community college graduates. I find that the callback rate for accounting job applications is 7 percentage points lower for applicants with community college experience while the average callback rate for accounting job applications is

[29] I define discrimination that is not statistical as "biased prior" rather "taste-based discrimination" because taste-based discrimination is difficult to imagine in this paper's setting. It is hard to think of a person who innately dislikes community college graduates. On the other hand, it is easier to imagine individuals holding irrational bias against community college graduates' productivity due to their unfamiliarity with this population.

13%. Quantifying this result with the effect of GPA on callback rates, I calculate that the level of discrimination is equivalent to the effect of a 0.4 drop in college GPA. In contrast, I do not find evidence for similar discrimination in sales and marketing job applications.

Next, I take three approaches to identify the mechanism of the discrimination against community college graduates. First, I study the effect of applicants' four-year college selectivity on the gap in callback rates. I find that while non-transfer students receive higher callback rates if they graduate from selective four-year colleges, transfer students' callback rates do not depend on the selectivity of their four-year colleges. Unless all educational values of selective colleges are realized in the first two-years of college, this result points toward the discrimination being non-statistical and due to biased prior. This result also suggests that the discrimination against community college students interferes with employers' valuation of the candidates' other qualifications. Second, I corroborate the findings using the Education Longitudinal Study of 2002. I show that, conditional on bachelor's degree attainment, there is a significant income premium for not having earned a degree from community college, and that this income premium does not disapper as richer sets of controls on individuals' ability are included. Third, I conduct a small-scaled survey on community college transfer students that asks about their perception of the discrimination they have experienced. The survey highlights that bias against community college students are more severe in cognitive ability, which helps explain the difference in findings between the accounting industry and the sales industry.

To the best of my knowledge, this is the first study that attempts to credibly identify discrimination against community college graduates. Closely related is the empirical literature on the returns to a community college education. Studies in this literature generally find that community colleges improve the earnings potential of high school graduates (Kane and Rouse, 1995; Belfield and Bailey, 2011; Jepsen et al, 2014; Mountjoy, 2019). I contribute to this literature by examining the labor market returns (costs) to community college enrollment *conditional* on bachelor's degree being the highest degree earned. This distinction is critical for high school graduates' college enrollment decision, since more than 70% of community college freshmen plan to pursue a bachelor's degree via transfer (Hossler et al, 2012). As pointed out by Cecilia Rouse (1995), community colleges not only attract students who otherwise would not have attended college (democratization effect) but also attract students who otherwise would have directly attended four-year college (diversion effect). Findings of this paper is crucial for students deciding between directly enrolling in four-year colleges and first enrolling in community colleges.

Methodologically, this paper follows a long literature of using audit studies to identify discrimination

(Bertrand and Mullainathan, 2004; Oreopoulos, 2011). In another paper using audit study to examine employer perceptions on different types of degrees, Deming and coauthors find that graduates from for-profit institutions receive significantly fewer callbacks than those from non-profit non-selective institutions (Deming et al, 2016). Previous studies highlight the importance of distinguishing between different theories for the discrimination documented (Bertrand and Duflo, 2016). In this regard, I attempt to distinguish between statistical and bias-based discrimination by examining the interaction effect between community college experience and four-year college selectivity, and by combining results from publicly available data and insights from surveys.

In the remainder of the paper, I will discuss the experimental design, including the design of the resumes, the implementation of job applications, and the analysis using the ELS data, in section 2. I will present the main results in section 3. In section 4, I discuss the interpretations for the results and corroborate the interpretations with the survey responses. Finally, I offer conclusions in section 5.

1 Experimental Design

1.1 Study Setting: Degrees, Job Types, and Geographic Locations

In the design of the experiment, I require all applicants to have a bachelor's degree. This condition is imposed to ensure that the experiment is identifying the discrimination against community college, rather than the difference between returns to a four-year degree and to a two-year degree. Moreover, for the applicants who are randomly assigned community college experience, we restrict the two-year degrees to associate's degrees, and do not include resumes with certificate degrees. This is because certificate degree programs typically do not help prepare students for transfer to a four-year program, and is not equivalent to the first two years in a four-year program by design.

The study focuses on two specific types of occupations: sales/marketing and accounting/auditing. These two occupations ranked number 3 and 5 on LinkedIn's report on the most popular jobs recent college graduates entered. The occupations ranked higher than sales and accounting are software engineer, registered nurse, and teacher. I did not choose to apply to these occupations for two main reasons. First, entry level software engineer and teacher positions tend to have very structured application process, and tend not to hire on online job platforms. Second, software engineer positions and registered nurse positions typically require specific tests and/or credentials before an applicant can be considered for interviews, hindering the ability of audit studies to gain credible insights into their hiring process. The shares of LinkedIn's top 5 occupations in all job openings available on the online platform I used are presented in Table 1. Other occupations on the LinkedIn's list include project manager, administrative assistant, account executive, financial analyst, and account manager. All of these occupations bear similarity to sales and/or accounting positions, and I therefore do not add additional randomization arms to target each of the occupations.

I apply to jobs in a wide range of geographic locations to ensure the representativeness of the study's results. In particular, I apply to the northeast labor market (New York City, Boston, and Philadelphea), the midwest labor market (Chicago, Detroit, and Minneapolis), the Southern labor market (Houston, Dallas, and Atlanta), and the West Coast labor market (Los Angeles, San Francisco, and Seattle).

1.2 Resume Design: Randomization Arms and Resume Contents

Each resume used in this study consists of three main components: education, job experience, and skills and qualifications. I start by describing the construction of the education profiles, where the study's randomization takes place.

Each applicant is randomly assigned into one of the eight randomization arms, characterized by the selectivity of the four-year college, whether the applicant has previously enrolled in community college, and the applicant's GPA. The randomization arms are: 1. non-selective four-year, community college, 3.2 GPA; 2. non-selective four-year, community college, 3.4 GPA; 3. non-selective four-year, community college, 3.6 GPA; 4. selective four-year, community college, 3.2 GPA; 5. selective four-year, community college, 3.4 GPA; 6. selective four-year, community college, 3.6 GPA; 7. non-selective four-year, no community college, 3.2 GPA; 8. selective four-year, no community college, 3.2 GPA.

The random variation in the selectivity of four-year college is designed to capture potential evidence for rational (as opposed to irrational) statistical discrimination. I assume that being able to obtain a bachelor's degree from a selective four-year college (e.g. New York University) conveys stronger productivity attributes than graduating from a non-selective four-year college (e.g. Fordham University), either because the programs are more challenging and requires more human capital, or because competitive programs foster more interpersonal connections that are also correlated with productivity. Given these additional attributes, *rational* employers will update their beliefs on applicants and place less weight on the indirect evidence drawn from whether the applicant has previously enrolled in a community college, and therefore commit less discrimination against students graduating from selective four-year colleges. In contrary, however, irrational employers' discrimination and/or prejudice may not be easily affected by the additional information. As a result, taste-based and discrimination-based discriminations against community college graduates would not be sensitive to the selectivity of four-year colleges.

I vary the GPA of graduates with community college experience so that the level of discrimination can be anchored and quantified by the effect of GPA on callback rates. However, graduates with no community college experience are all assigned a GPA of 3.2 for two reasons. First, adding 4 additional randomization arms is extremely costly for statistical power, and would require a significantly larger sample size. This would not only increase the cost of the study, but also increase the risk of adverse effects on the real hiring process. Second, the additional randomization arms would not add much to our identification strategy. Although adding applicants with no community college experience and with GPAs of 3.4 and 3.6 would help corroborate the heterogeneity results from the random variation of college selectivity, the additional information would be mostly redundant.

To avoid peculiar resumes, where, for example, a graduate from UCLA applies to a position in Boston, the institutions that the applicants attended depends on their assigned labor market. In each geographic labor market, I choose a community college, a non-selective four-year institution, and a selective four-year institution with large enough name recognition. Our criteria for non-selective versus selective four-year institution is based on Barron's ranking. The institutions for each geographic labor market are presented in Table 8.

The major of the applicants were chosen to match the targeted occupations, and so we assign either a Bachelor in Business Administration or an Associate in Business Administration degree depending on the type of institution. Where these two particular degrees are not offered, we substitute the degree with a closely related major. For example, since NYU does not offer a Bachelor in Business Administration degree, applicants graduating from NYU are assigned Bachelor of Arts in Economics degrees. Relevant course works were randomly selected from a list of courses relevant for sales occupations and accounting occupations offered at the applicants' institution.

GPAs are assigned according to randomization. However, I allow the precise GPA to be 0.01 or 0.02

higher than the assigned GPA, so that applicants assigned with a GPA of 3.2 could have a 3.21 or 3.22 on their resume, while applicants assigned with a GPA of 3.6 could actually have a 3.61 or 3.62. The purpose of this adjustment is to make the resumes look as natural as possible.

All applicants are assigned to be recent graduates of the class of 2019, and so there are no variation in the age of applicants. All applicants' names are intentionally chosen to be common names for non-hispanic white American, so that there are no perceived variation in the ethnicity of applicants. To the extent that is controllable, we avoid names that may raise concerns of being artificial, e.g. Jack Smith.

Now I turn to the construction of the job experience section of the resume. To ensure that the job experience match the job requirements both in terms of quality and content, the pool of internship experience is pulled from the job advertisements for internships found on the same job platform under the same keywords. For example, for applicants applying to full-time sales positions, we construct the pool of internship experience by searching for sales and marketing internship positions on the job platform. The company names and job titles of the search results are compiled for the internship job titles, and the job descriptions of the internship advertisements are compiled to construct the resumes' bulletin points explaining the applicants' internship experiences. The job descriptions are rephrased manually to ensure variations and the final internship experience sections have been checked manually to avoid repetition of bulletin points and other inconsistencies.

Finally, in the skills and qualifications section, occupation specific lists of computer skills and personal qualifications are used to randomly generate the skills and qualifications of each resume. Importantly, since eligibility for the CPA exam is virtually required for accounting jobs targeting college graduates, the following sentence has been added to every resume submitted to accounting job applications: "150 credit hours completed. Eligible to sit for CPA Exam."

The resumes were randomly generated using the random resume generator developed by Lahey and Beasley (2009). The program can be found on the NBER website and is publicly available.

1.3 Job Application and callback Recording

All generated resumes in a geographic location are then ranked in a random order, and sent to job openings posted on one of the largest job search platforms in the country. According to statistics listed on the platform's website, the platform has more than 10 million job listings from more than 1 million employers. The job applications were conducted between August and October, 2019, so that the job applications reflect those made by recent college graduates. Accounting jobs were located by searching at the assigned geographic area and inputing key words "accounting", "accountant", or "audit". Sales jobs were located by searching at the assigned geographic area and inputing key words "sales" or "marketing". Job queries were restricted to full-time entry-level positions that required no more than 3 years of work experience. This criteria was set to match the type of jobs that recent college graduates apply to. I also do not apply to jobs that request information beyond what is available in the resume. An exception to this requirement is made for questions asking whether the applicant has legal permission to work in the US, since all applicants are characterized as US citizens who are authorized to work full-time in the country.

I follow the standard for labor market audit studies in not accepting any interview invitation and/or job offers. Only callbacks, defined as a personalized email and/or phone call inviting the applicant to a phone or in-person interview, are recorded. Moreover, the job search platform has a 0-5 score for individual employers. The score is determined by recent employee feedback and computed using the platform's proprietary ratings algorithm, which place higher weights on more recent reviews. The majority of job openings applied to have an employer score, and I use this measure as a summary statistic to control for employer quality in the preferred specifications. As shown in Figure 1, there is sufficient variation in the employer rating for both accounting and sales job openings

1.4 Empirical Specifications Using Audit Study Data and ELS 2002

The main empirical specification using the audit study data is:

$$\text{CallBack}_i = \beta_0 + \beta_1 \text{Transfer}_i + \beta_2 \text{GPA}_i + \beta_3 \text{Selective}_i + \boldsymbol{\beta X_i} \tag{34}$$

where Transfer_i is an indicator for whether a job applicant has previously enrolled in a community college, Selective_i is an indicator for whether a job applicant graduated from a selective four-year college, and $\boldsymbol{X_i}$ is a vector of control variables that include the employer ratings, and a vector of indicators for each labor market in which we have applied to. Given the randomization of Transfer_i, β_1 identifies the level of discrimination against community college graduates.

To examine the mechanisms of the discrimination against community college graduates, I further study how the level discrimination changes with the selectivity of the applicants' four-year college. The empirical specification is:

$$\text{CallBack}_i = \beta_0 + \beta_1 \text{Transfer}_i + \beta_2 \text{GPA}_i + \beta_3 \text{Selective}_i + \beta_4 \text{Transfer}_i \times \text{Selective}_i + \boldsymbol{\beta X_i} \tag{35}$$

β_4 identifies the effect of four-year-college selectivity on the level of discrimination.

Given the limited amount of variation that can be built into a resume audit study and the experiment's inability to capture job applicants' wage offer, I supplement the study with an analysis using the Education Longitudinal Study of 2002 (ELS). The ELS is a nationally representative longitudinal survey of over 15,000 10th grade students in 2002. The study follows up participants in 2004 (12th grade), 2006 (during college), and in 2012, when students, under standard academic progress, would have completed their undergraduate study. The data contains information on students' educational history, employment history, as well as a rich set of demographic backgrounds. Throughout the analysis using the ELS data, I focus on the subset of students who have attained a bachelor's degree by the 2012 survey.

Using the ELS data, I first examine whether there is evidence on discrimination against students with community college backgrounds when income is the outcome variable. The regression specificaion using the ELS data is:

$$w_i = \beta_0 + \beta_1 \text{AA}_i + \beta_2 \text{GPA}_i + \beta_3 \text{Selective}_i + \boldsymbol{\beta X_i} \tag{36}$$

where w_i is the students' 2011 income, and $\boldsymbol{X_i}$ is a vector of control variables. In the baseline model, $\boldsymbol{X_i}$ includes gender, ethnicity, college major, and a gender-specific quadratic on work experience.

If employers statistically discriminate against community college graduates, then they are inferring hard-to-observe characteristics of employees through the easy-to-observe community college background. If this is the case, the employees' income should rely less on their community college background as the hard-to-observe characteristics are taking into account (Farber and Gibbons, 1996; Altonji and Pierret, 2001). To examine whether this is the case, I leverage the ELS's rich information on student backgrounds, and sequentially include richer controls in specification (3), including students' age at 2011, highest parental education, parental inome, high-school GPA, and high-school extracurricular involvement.

2 Results

2.1 Main Effects of discrimination

A total of 3635 job applications were submitted as a part of the audit study. Among these applications, 1350 were for accounting jobs, and 2285 were for sales jobs. Overall, accounting job applications have an average callback rate of 12.8% and sales job applications have an average callback rate of 26.0%. The callback rates by industry and by labor market is reported in Table 2.

As shown in column 1 of Table 3, a simple comparison between callback rates for those with and without community college experience suggests that community college experience reduces callback rates by 2 percentage points and that the effect is not statistically significant. However, this simple comparison hides significant heterogeneity across the two types of occupations. As I report in column 2 of Table 3, community college experience reduces callback rates by more than 6 percentage points for accounting positions and the effect is statistically significant at the one percent level. On the contrary, column 3 of Table 3 shows that community college experience has no significant effect on callback rates for sales positions. If anything, the point estimate of the effect is positive.

In the main empirical specification — equation (1) — I include labor market fixed effects to absorb geographic variation in callback rates, and the job platform rating to control for employment quality. Results for this analysis are presented in Table 4. The results in column 2 confirm the conclusion of Table 3, and suggest that the callback rate for applicants with community college experience is 7 percentage point lower than those without community college experience. The difference is statistically significant at the 5% level. In comparison, the results also suggest that an one point increase in GPA leads to a 24 percentage point increase in callback rate. Since the study only assigns three discrete GPA values, I also report the average callback rates for each discrete GPA level in Table 5. The reported averages indicate that improving GPA from 3.2 to 3.6 improves accounting callback rates by 8.1 percentage points, suggesting that the discrimination against community college graduates is similar in magnitude to the effect of reducing GPA from 3.6 to 3.2.[30]

In comparison to the results for accounting job applications, the results for sales job applications (columns

[30] The average effect of GPA on callback rate I find — 5.5 percentage point increase in callback by increasing GPA from 3.2 to 3.6 — is very close to that found in Quadlin (2018). She finds that an increase in GPA from the range [2.84,3.20] to [3.21,3.59] increases callback rate by 4 percentage points.

3 of Table 4) suggest that there is no significant discrimination against community college graduates in sales occupations. The point estimates are close to zero and statistically insignificant.

2.2 Interaction Effects and ELS Results

Next, I run regressions that include interaction effects of community college experience with four-year program selectivity — equation (2). The results are presented in Table 6. As shown in column 2, for accounting job applicants, non-transfer students from selective four-year colleges receive callbacks 10 percentage points more frequent than non-transfer students from non-selective four-year colleges. However, transfer students from selective four-year colleges do not enjoy similar callback-rate premium. In fact, the point estimates suggest that transfer students from selective four-year colleges receive callbacks 1 percentage points less frequent that transfer students from selective four-year colleges. In comparison, results presented in column 3 suggest that selectivity of four-year college generally does not matter for callback decisions for sales job openings.

Table 7 presents the results from the analyses using the ELS data and following equation (3). Column 1 of panel 1 shows that BA holders with community-college backgrounds earn $3932.36 less in business and finance related jobs compared to BA holders without community-college backgrounds. Columns 2-6 show that the gap in earnins responds only slightly to the inclusion of hard-to-observe characteristics, including age, parental education, parental income, high-school GPA, and high-school extracurricular involvement in the regressions. By including all of these control variables, the earnings gap only decreases from $3932.36 to $3453.21. In comparison, the results shown in panel 2 show that community-college graduates do not experience similar discrimination in the sales and marketing related occupations.

3 Interpretations and Survey Results

The results of the audit study presented above show that while community college experience significantly reduces the callback rate for accounting job applicants (Table 4, column 2), it does not seem to harm the callback rate for sales job applicants (Table 4, column 3). Results from the ELS data supports this finding, and show that students with community college background earn around $3900 less annually in the finance and business occupations. I do not find similar earnings gap in the sales and marketing occupations.

Is the discrimination in the accounting industry based on rational expectation on community-college students' productivity or biased prior? Results from additional empirical analyses suggest the latter case is more likely. First, results in Table 6 suggest that only non-transfer students from selective colleges receives higher callback rates compared to their non-selective college peers. Such preference of the employers can only be rationalized if the value of selective colleges are exclusively generated in the first two years of college, during which the transfer students were enrolled in community colleges. This, however, is highly unlikely. Second, results in Table 7 panel 1 suggest that the earnings gap between transfer and non-transfer students are not sensitive to the inclusion of age, parental education, parental income, high-school GPA, and high-school extracurricular involvement. If employers commit statistical discrimination, they discriminate based on community college backgrounds because it is easily observable and correlated with hard-to-observe characteristics that affect productivity. As these hard-to-observe characteristics are included in equation (3), the community college backgrounds of students should matter less forearnings. The results in Table 7 panel 1 do no square up with this hypothesis.

What explains the difference in results between the accounting occupations and the sales occupations? Do the two occupations have different perceptions on community college students? Or do the two occupations have the same perception on community college students, but look for different dimensions of human capital in job applicants? I argue that the latter case is more likely. First, using the ELS data, I calculate that 8.61% of sales and marketing workers have an associate's degree while 8.53% of business and financial occupation workers have an associate's degree. It is unlikely for two industries in which community college graduates composes of similar shares of the population hold starkly different perception on community college graduates. Second, there is evidence that the two industries care about different dimensions of human capital. Results in Table 5 show that GPA and four-year college selectivity is significantly less important for sales occupations compared to accounting occupations. According to O*Net's importance of specific skills in occupations, the importance of mathematics ability is 72 for accountants, but only 40 for sales occupations. In contrary, the importance of social perceptiveness is 50 for accountants, and 57 for sales and marketing occupations. These results suggest that the biased prior against community college graduates may be focused on cognitive abilities, as opposed to noncognitive abilities.

To corroborate this interpretation, I conducted a small-scale anonymous online survey on community college transfers at a highly selective private four-year institution in the Northeast.[31] The respondents were invited from the pool of all community college transfer students in the institution, and the respondents were not briefed on the design or results of the audit study. The survey asks respondents for their impressions on the existence and nature of the discrimination against community college students. Although the sample size of the survey is small (N=16), the sample represents 50% of the transfer student population at this institution, and the responses overwhelmingly support the hypothesized mechanism.

All of the survey respondents believe that there is a discrimination against community college students, and more than 80% of the respondents believe that the discrimination is sizable as opposed to being minor. Figure 2 presents a word bubble for the responses to the open-end question "what type of discrimination, if any, do you think is associated with community colleges?" As can be seen in the graph, while there are some responses mentioning discrimination on non-cognitive skills (e.g. lazy, less-commitment, or not-motivated), most of the highest mentioned phrases point to weaker intelligence and lower academic preparedness (e.g. poorly-educated, not-smart or not-academic). More directly, I ask respondents whether an employer would prefer a four-year graduate who have previously enrolled in a community college or a four-year graduate who have not previously enrolled in a community college. I ask the respondents to consider this question separately for cognitive and non-cognitive skills. The responses to this question is reported in Figure 3. The results also support the hypothesis that discrimination attached to community college graduates is largely focused on their cognitive skills. Interestingly, when asked the question "Suppose there are two hiring managers, one from an accounting firm, and another from a sales firm. Which hiring manager has a better perception of a 2-4 transfer student?" 50% answered responded the sales firm HR, 25% answered the accounting firm HR, and the other 25% responded no difference in perception.

4 Conclusions

In this paper, I use a national labor market audit study to examine the existence and the nature of discrimination against community colleges in the labor market. The negative impact of community college experience on callback rate is particularly salient for accounting job applications, where the 7 percentage point decrease in callback rate is equivalent to a 0.4 point drop in college GPA. On the contrary, community college ex-

[31]The institution asked to remain anonymous for the purpose of this study.

perience does not seem to harm the callback rate for sales job applications. Moreover, I find suggestive evidence that the discrimination is driven by irrational and bias-based discrimination against specific human capital attributes of community college students. A small-scaled survey on community college graduates corroborates the results of the study, and suggests that the discrimination attached to community college graduates is likely associated to their cognitive (as opposed to noncognitive) skills. This is perhaps due to the public perception that students choose to enroll in community colleges only because they cannot gain admission to four-year colleges and that community college courses are less demanding.

The existence of discrimination-based discrimination against community college graduates not only could deepen socioeconomic inequality, but could also significantly harm the efficacy of public investment in higher education. Fortunately, studies have shown that intergroup contact can be effective in reducing discrimination and prejudice (Allport, 1954; Pettigrew and Tropp, 2008). This suggests that the "bad equilibrium" where biased hiring behavior exacerbates pre-existing discrimination could be effectively negated if efforts are made to encourage and assist more community college graduates to enter these traditionally elite occupations. An information campaign that educates employers (or the general public) of the prevalence and legitimacy of community colleges may also be effective. The effectiveness of these measures in combating discrimination against community college graduates deserves further research by future studies.

5 Tables and Figures in Chapter 3

Table 1: Share of Job Categories in Entry-Level Job Openings by Labor Market

	Share in Entry-Level Job Openings				
Job Category	Accounting	Sales	Software	Teacher	Nurse
Atlanta	5.6%	8.5%	4.8%	0.8%	9.5%
Boston	3.5%	4.3%	4.3%	2.8%	15.5%
Chicago	4.7%	7.9%	0.1%	2.4%	10.9%
Dallas	6.5%	9.1%	5.7%	0.5%	4.7%
Detroit	3.3%	8.7%	4.8%	0.7%	21.3%
Houston	6.3%	8.8%	3.9%	0.8%	9.3%
Los Angeles	5.3%	6.5%	0.0%	1.4%	12.2%
Minneapolis	5.3%	7.0%	5.1%	2.6%	4.6%
New York	6.8%	7.7%	2.4%	2.5%	11.3%
Philadelphia	6.1%	10.6%	3.6%	1.4%	9.3%
San Francisco	4.2%	3.8%	3.6%	1.5%	12.1%
Seattle	2.8%	4.3%	6.4%	0.1%	14.1%
Average	5.2%	7.2%	3.3%	1.6%	11.2%

Notes: this table reports the share of accounting, sales, software, teaching, and nursing jobs among entry-level job openings posted on the online job platform. Shares are reported for the 12 cities in which job applications were submitted. The overall average shares are also reported. Number of entry-level job openings in each job category is computed by searching the respective key words and the geographic location, and by restricting to entry-level job openings. Total number of entry-level job openings in each geographic location is computed by searching the geographic location, and by restricting to entry-level job openings.

Table 2: Callback Rates by Industry and Labor Market

	Call Back Rates	
Job Category	Accounting	Sales
Atlanta	6.3%	44.0%
Boston	17.5%	23.8%
Chicago	11.6%	29.5%
Dallas	9.8%	37.3%
Detroit	9.6%	24.7%
Houston	14.3%	23.9%
Los Angeles	17.3%	20.5%
Minneapolis	12.5%	23.8%
New York	10.3%	26.3%
Philadelphia	9.6%	30.4%
San Francisco	6.8%	19.4%
Seattle	10.5%	30.0%
Average	12.8%	26.0%

Notes: callback rates by city, and by occupation type are reported. Average callback rates over all cities are also reported. Callback rate is defined as the share of applications that received a personalized email or phone call inviting the applicant to an interview.

Table 3: Simple Comparison of callback Rates by Industry

	(1)	(2)	(3)
	Callback		
Industry	All	Accounting	Sales
No Community College	0.216	0.157	0.251
Community College	0.196	0.090	0.264
Difference	-0.020	-0.067***	0.013
	(0.019)	(0.025)	(0.027)
Observations	3635	1350	2285

Notes: the average callback rates for applicants with and without community college experience are reported for each job category. Samples are restricted to applicants with 3.2 GPA, because applicants without community college experience are only assigned 3.2 GPA. The last row reports the callback differences between applicants without prior community college enrollment and those with community college degrees. Standard errors of the differences are reported in parentheses. Standard significance levels are reported: * 0.10, ** 0.05, *** 0.01.

Table 4: Callback Rate Regressions — Main Regressions

	(1)	(2)	(3)
	Callback		
Industry	All	Accounting	Sales
AA Degree	-0.033	-0.070**	-0.002
	(0.024)	(0.034)	(0.032)
College GPA	0.142**	0.245***	0.084
	(0.061)	(0.086)	(0.082)
Selective College	-0.014	0.002	-0.041*
	(0.017)	(0.024)	(0.022)
Observations	3635	1350	2285

Notes: this table reports estimates from the main effect regressions for the audit study callback rates. All specifications control for employer rating, and labor market fixed effects. Robust standard errors are reported in parenthesis. Standard significance levels are reported: * 0.10, ** 0.05, *** 0.01.

Table 5: callback Rates by Discrete GPA Levels

	Callback Rate		
Industry	All	Accounting	Sales
3.2 GPA	19.2%	9.3%	25.4%
3.4 GPA	19.3%	11.6%	25.3%
3.6 GPA	24.7%	17.4%	27.9%

Notes: Average callback rates for each discrete GPA level are reported by industry. Samples are restricted to those with community college experience, because only applicants with community college experience are assigned 3.4 or 3.6 GPAs.

Table 6: Callback Rate Regressions — With Interaction Effect

	(1)	(2)	(3)
	Callback		
Industry	All	Accounting	Sales
AA Degree	-0.015	-0.053	0.004
	(0.030)	(0.041)	(0.041)
College GPA	0.144**	0.255***	0.084
	(0.061)	(0.086)	(0.082)
Selective College	0.015	0.099*	-0.032
	(0.033)	(0.059)	(0.040)
Rating	0.040***	0.041***	0.030**
	(0.009)	(0.009)	(0.014)
AA * Selective	-0.040	-0.110*	-0.012
	(0.029)	(0.068)	(0.049)
F-Statistic	2.93	7.94	0.18
(Prob>F)	(0.09)	(0.00)	(0.67)
Observations	3635	1350	2285

Notes: this table reports estimates from the main effect regressions for the audit study callback rates. All specifications control for employer rating, and labor market fixed effects. The variable "AA*Selective" is the interaction indicator for applicants with community college experience and from a selective four-year college. The variable "AA*Rating" is the interaction term between prior community college experience and the online platform's employer rating. Robust standard errors are reported in parenthesis. Standard significance levels are reported: * 0.10, ** 0.05, *** 0.01.

Table 7: Wage Regressions Using ELS 2002 Data

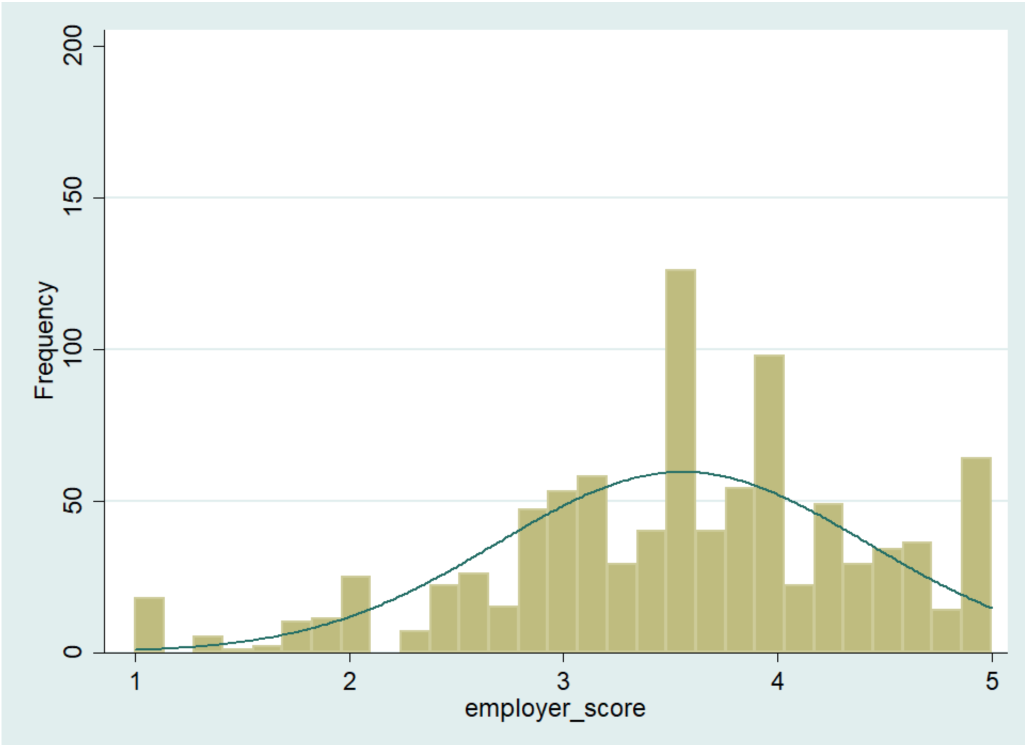
	Panel 1					
	(1)	(2)	(3)	(4)	(5)	(6)
	2012 Wage					
CC Enrollment	-3932.36**	-3924.34**	-3841.54**	-3786.06**	-3625.98*	-3453.21*
	(1948.0)	(1951.3)	(1946.7)	(1944.6)	(1953.0)	(1994.0)
College GPA	4503.55*	4464.84*	4504.64*	4611.08*	3832.62	3910.63
	(2383.0)	(2381.0)	(2383.8)	(2394.8)	(2586.1)	(2583.6)
Selective College	6478.79***	6502.95***	5764.35***	5539.99***	5326.41***	5398.83***
	(1699.0)	(1705.1)	(1713.2)	(1693.2)	(1651.9)	(1652.0)
Age	-	-1302.1	-1335.29	-1361.73	-1335.80	-1260.38
		(2583.7)	(2584.4)	(2576.2)	(2586.8)	(2595.5)
Parent Education	-	-	933.91**	673.49	680.72	677.30
			(454.9)	(501.2)	(502.7)	(503.6)
Parent Income	-	-	-	692.25	743.25	679.74
				(541.1)	(554.3)	(553.0)
HS GPA	-	-	-	-	864.01	758.31
					(1248.6)	(1234.8)
HS Extracurricular	-	-	-	-	-	909.46
						(997.7)
Observations	758					

	Panel 2					
	(1)	(2)	(3)	(4)	(5)	(6)
	2012 Wage					
CC Enrollment	-2469.55	-2493.50	-2447.09	-2393.36	-2527.13	-2459.58
	(2302.5)	(2307.2)	(2309.1)	(2354.5)	(2356.2)	(2372.4)
College GPA	2046.23	2080.16	1946.64	2062.20	2508.82	2807.13
	(2996.8)	(3006.2)	(2988.8)	(2926.7)	(2807.4)	(2790.2)
Selective College	6661.45***	6640.85***	6112.29**	6020.29**	6150.55**	6313.19**
	(2543.1)	(2544.0)	(2538.1)	(2488.5)	(2515.4)	(2548.9)
Age	-	2365.87	2290.64	2306.44	2386.08	2204.39
		(2902.4)	(2844.7)	(2852.3)	(2878.5)	(2875.3)
Parent Education	-	-	975.48	856.36	849.12	831.72
			(809.3)	(749.0)	(748.6)	(756.3)
Parent Income	-	-	-	287.94	268.12	135.96
				(672.0)	(666.2)	(664.7)
HS GPA	-	-	-	-	-560.94	-690.39
					(1081.1)	(1078.4)
HS Extracurricular	-	-	-	-	-	2250.14*
						(1186.2)
Observations	546					

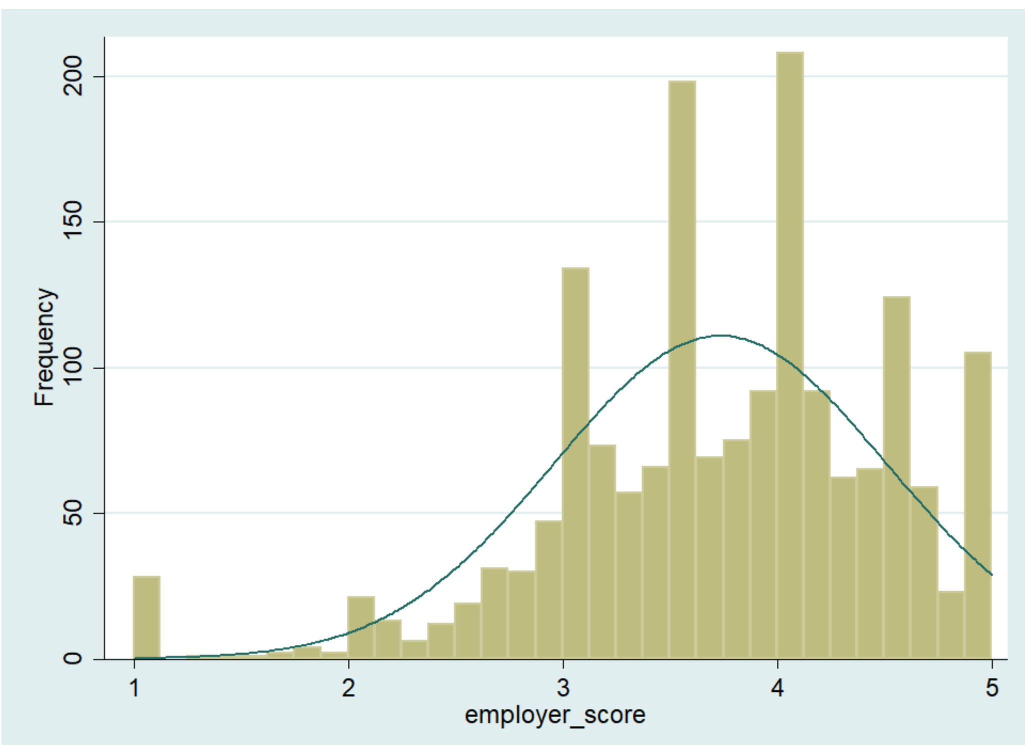
Notes: these tables report wage regressions using the ELS 2002 data. The regressions are restricted to the subset of students who have attained BA degrees by 2012. CC Enollment is an indicator for previous enrollment in community college. Robust standard errors are reported in parenthesis. Standard significance levels are reported: * 0.10, ** 0.05, *** 0.01.

Table 8: Labor Market and Corresponding Colleges

Labor Market	Selectivity	Selective BA School
North-East	Selective BA	New York University
	Non-Selective BA	Fordham University
	AA School	CUNY Borough of Manhattan Community College
West Coast	Selective BA	University of California - Los Angeles
	Non-Selective BA	University of California - Riverside
	AA School	Los Angeles City College
Mid-West	Selective BA	University of Michigan - Ann Arbor
	Non-Selective BA	University of Illinois - Chicago
	AA School	City Colleges of Chicago - Wilbur Wright College
South-West	Selective BA	University of Texas - Austin
	Non-Selective BA	University of Houston
	AA School	Houston Community College



(a) Accounting Positions



(b) Sales Positions

Figure 1: Distribution of Employer Ratings

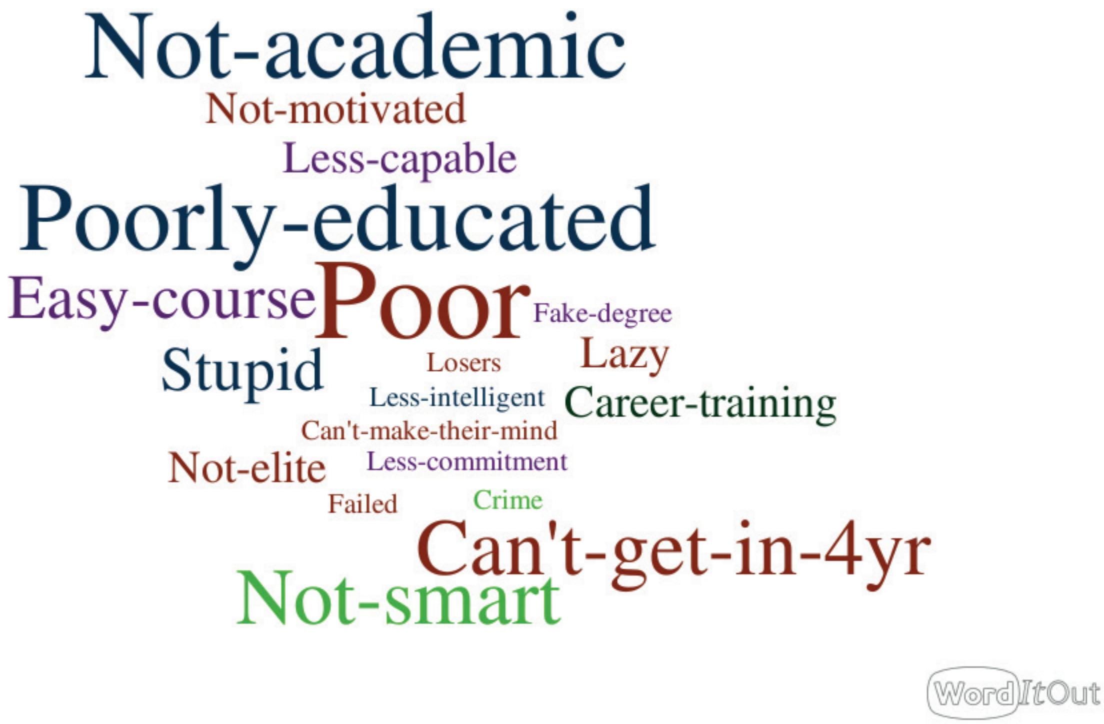


Figure 2: discrimination Word Bubble

Note: the figure reports the frequently appearing phrases in survey participants' response to the question "If you think there is a negative discrimination against community colleges, list as many negative discriminations you think are attached to community colleges." Phrases with larger fonts appear more frequently, and the color of the phrases has no substantial meaning.

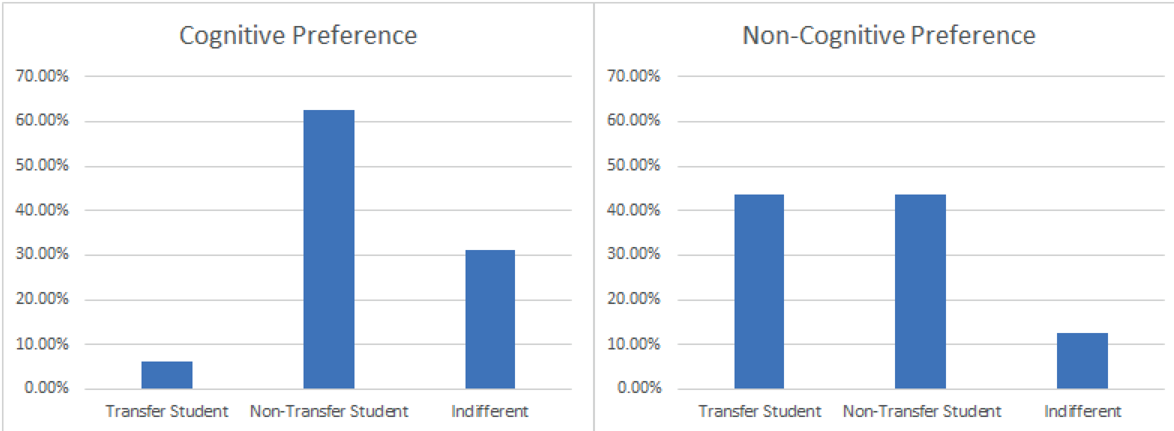


Figure 3: Survey Response

Notes: the left panel reports survey participants' response to the question "The hiring manager believes that the individual with higher cognitive skills is" and the right panel reports survey participants' response to the question "The hiring manager believes that the individual with higher noncognitive skills is."

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