EdWorkingPaper No. 21-356

Admissions Policies, Cohort Composition, and Academic Success: Evidence from California

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VERSION: February 2021

Suggested citation: Grosz, Michel. (2021). Admissions Policies, Cohort Composition, and Academic Success: Evidence from California. (EdWorkingPaper: 21-356). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/y3xp-nt12

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February 23, 2021

Abstract

I study how postsecondary admission policies affect the composition and subsequent academic outcomes of new cohorts. I leverage the staggered replacement of lotteries and waitlists at California's community college nursing programs with admissions that rely on grades, work experience, and other measures. The change in admissions increased the average prior academic performance of incoming cohorts, but did not improve academic outcomes such as completion rates or pass rates on the national licensing exam. I find suggestive evidence that the change in admissions decreased the share of new students who were not White, but by small amounts. The change in admissions also substantially reduced the time students spent waiting between taking prerequisite coursework and entering the nursing programs.

1 Introduction

Why do colleges use merit-based admissions? There are other ways to allocate scarce resources in contexts where it is undesirable to raise prices, such as with lotteries (e.g. charter schools), waitlists (e.g. organ donation) and first-come-first-served processes (e.g. restaurant tables). Merit-based

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admissions allow colleges and universities to select applicants that will add value along various dimensions, potentially improving graduation rates, generating positive peer effects, and boosting rankings. But, basing admissions on merit has its drawbacks. The tools that institutions can use to screen applicants, such as test scores, produce measures of merit that are often correlated with other factors, contributing to disparities in access to higher education along racial, ethnic, and socioeconomic lines (Park and Becks, 2015; Buchmann, Condron and Roscigno, 2010). These tools are also often imperfect predictors of academic success (Rothstein, 2004; Bridgeman, McCamley-Jenkins and Ervin, 2000; Camara and Echternacht, 2000; Bulman, 2017; Geiser and Santelices, 2007).

There is relatively little evidence, however, on how admissions strategies affect the composition and outcomes of students. Many researchers have studied the effect of educational interventions on various outcomes by leveraging admissions rules such as test score cutoffs (e.g. Hoekstra, 2009; Anelli, 2020; Abdulkadroglu et al., 2017; Angrist and Rokkanen, 2015), and random lotteries (Dobbie and Fryer Jr, 2015; Ketel et al., 2016). Less is known about how changes in the admissions policies themselves affect the composition of applicant pools and cohorts. Recent work shows that colleges that make standardized tests optional do not see changes in the diversity of incoming cohorts (Belasco, Rosinger and Hearn, 2015). There is also a growing literature on the effects of affirmative action policies (Naven, 2017; Hinrichs, 2014; Arcidiacono et al., 2014). Other work has honed in on the effect of changes to application fees (Smith, Hurwitz and Howell, 2015) and entrance exams (Slonimczyk, Francesconi and Yurko, 2017). A challenge in this area of research is the endogenous selection of strategies.

In this paper I examine the effects of a large state-wide change in admissions rules. In 1991, the California Community Colleges restricted admissions for associate degree in nursing (ADN) programs to only "non-evaluative" policies, such as waitlists and random lotteries. In 2007 the state legislature reversed this policy and allowed colleges to base admissions decisions on predetermined criteria including prior grades, work history, essays, and personal references. I leverage the staggered adoption of these new admissions policies across the state's many ADN programs to provide causal estimates of the effect of the new admissions systems. The identification strategy relies on differences in the timing of the adoption of evaluative admissions, which I show are not correlated with observable program or local labor market characteristics. I study how the new evaluative admissions processes affected the composition of new nursing cohorts as well as their subsequent academic attainment.[2]

This paper provides new evidence on the effect of admissions strategies at a time of great policy interest in admissions. The "Varsity Blues" scandal at elite universities unleashed a torrent of commentary, including calls for doing away with merit-based admissions altogether (Kamenetz, 2019; Hess, 2019; Latinien, McCann and Fishman, 2019). This new interest in admissions has also manifested itself in policy changes. In the past few years many private colleges and universities have made standardized testing optional in order to increase student diversity, with scant empirical evidence to support these claims (Belasco, Rosinger and Hearn, 2015). A growing number of graduate programs have done away with requiring the GRE, making similar claims about diversity while also noting that the test is not a good predictor of a student's eventual success (Jaschik, 2019*a*,*b*). This paper investigates the validity of these types of claims, which have been understudied despite the policy changes implemented in their name.

Studying admissions policies among community college nursing students also leads to new insights. Career-technical programs like nursing teach students particular occupations and involve substantial hands-on training, making academic preparation not necessarily a good indicator of a student's potential success in the occupation. For this reason, an admissions process that is based on grades, even in relevant coursework, may also not predict success in the program. Thus, a change in admissions that resulted in new cohorts with better academic background may not lead to higher completion rates, higher licensing exam pass rates, or even better nurses.

I focus on outcomes in three broad areas. I first study whether the policy worked as intended: did programs that adopted these new admissions standards bring in cohorts with higher past academic performance? Of the measures I can observe in the data, I find that a switch to evaluative methods leads to increases in the GPA of incoming students, especially in math and biology, as well as a decrease in the share of students who had taken remedial coursework. This is evidence that the new admissions rules succeeded in bringing in cohorts of students with better academic preparation.

I then examine the demographic composition of new cohorts. The reason that the colleges

[2] For the rest of this article I use the term "evaluative" to refer to admissions strategies that use inputs like GPA, work history, life experiences, etc. I use the term "non-evaluative" to refer to other systems, such as lotteries and waitlists.

initially banned evaluative screening processes in the early 1990s was because of a lawsuit alleging that these policies unfairly excluded minority students. Examining the effects of instituting evaluative admissions on the demographics of incoming cohorts is a way to test the initial lawsuit's claim of these unintended consequences. In my main specification I do not find any evidence that colleges switching to an evaluative admissions rule increased their enrollment of any particular demographic at the expense of another. In robustness exercises using alternative models, I do find evidence that the share of White students and younger students increased, but both by small amounts. Thus, my results do not show much support for the initial lawsuit: evaluative admissions policies in ADN programs did not decrease the likelihood that underrepresented groups gain admissions, even if these policies do not explicitly target these groups. This type of result has been the basis for affirmative action legislation and lawsuits in recent years, and motivation of the test-optional movement at four-year colleges.

Finally, I examine academic outcomes. The purpose of the 2007 legislation undoing the ban on evaluative admissions was to increase completion rates and licensing exam pass rates. However, I find no evidence of improvements in first-year GPA, or completion rates for students in cohorts accepted under the new admissions regimes. I also do not find any changes in pass rates on the national licensing exam. These results add to growing discussion in research and policy circles about the optimal design of admissions at all levels of education. For this particular set of programs, the change in admissions does not seem to have achieved the policy goal, increased completion rates. This raises questions about what types of measures admissions policy designers should request from applicants, and how they should weight different components of the application.

I do, however, find a large decrease, of approximately one year, in the time between when a student first started taking community college coursework and first enrolled in an ADN program. This is because policies such as waitlists forced students to wait or reapply for years on end. Thus, the new policies, while not improving academic outcomes, may improve labor market outcomes by moving students back into the labor market or to other educational pursuits sooner.

Taken together, the findings show that evaluative admissions for ADN programs brought in better-prepared students, as intended, but did not change outcomes. In addition, evaluative admissions may have slightly reduced the racial and ethnic diversity of incoming cohorts, and skewed admissions towards younger students. However, the new admissions dramatically decreased the amount of time students had to wait before entering the cohorts, primarily for programs that previously used waitlists.

The paper proceeds as follows. Section 2 outlines the policy setting and other background. Section 3 describes the data. Section 4 includes the methodology, sampling, and discussion of causal inference. Section 5 discusses the results, and section 6 concludes.

2 Background

2.1 Admissions, Academic Outcomes, and Student Composition

The optimal design of postsecondary admissions is a controversial topic and an area of active research. For the purposes of this paper, it can be useful to frame this question using the interrelated concepts of merit and fairness (Carnevale and Rose, 2003; Zwick, 2017). On one hand institutions weigh the effectiveness of a strategy in achieving its goals, such as improved graduation rates, by identifying merit. On the other hand institutions often strive for fairness in how they administer their admissions policies. Each institution defines merit and fairness according to its own objective function, which incorporate a variety of institutional values (Carnevale and Rose, 2003). [3]

It is not necessarily the case that the concepts of merit and fairness are in opposition to each other, as the issue of testing makes clear. There is mixed evidence on whether commonly used screening mechanisms, such as high school grades and standardized test scores, can effectively predict academic success in college (Rothstein, 2004; Bridgeman, McCamley-Jenkins and Ervin, 2000; Camara and Echternacht, 2000; Bulman, 2017; Geiser and Santelices, 2007). And, of course, test scores are correlated with student attributes other than merit. Students from higher-income families are more likely to benefit from test preparation services and tutoring, for example. Admissions that rely heavily on these scores are thus more likely to produce socioeconomic, racial, and ethnic disparities in college attendance (Posselt et al., 2012; Park and Becks, 2015).

Other aspects of an admissions strategy that put merit and fairness at odds are its complexity and transparency: admissions strategies that are more transparent are not necessarily more fair or

[3] There are, of course, other additional values that admissions officers must weigh as well. In 1999, for example, the College Board identified nine distinct values that any admissions officer might consider (Perfetto et al., 1999). Some of these included social and economic mobility, as well as meeting its financial needs.

less complex. A relatively opaque approach such as "holistic review" has the potential to counteract some of the disadvantages to low socioeconomic status applicants common to other approaches (Bastedo et al., 2018). On the other hand, clearer and simpler admissions may prevent the types of behaviors that gained the spotlight in the "Varsity Blues scandal, and may also reduce the disadvantage certain families face from navigating such a complicated process (Dillon and Smith, 2013; Pallais, 2015).

The literature on the effects of admissions strategies focuses on the concepts of merit and fairness by studying how these strategies affect the composition and eventual outcomes of applicants and incoming students. A large and growing body of work studies affirmative action and other race conscious programs. Affirmative action programs seem to succeed in leading disadvantaged students to increasingly enroll in selective colleges and universities (Arcidiacono, 2005; Howell, 2010; Hinrichs, 2014), but there is still mixed evidence on whether they improve short-run educational outcomes like degree completion (Cortes, 2010; Bleemer, 2018; Arcidiacono et al., 2014). These types of policies are also controversial politically, in part because of the potential lack of transparency in how they are administered. Top-percent plans, which also seek to increase representation of disadvantaged groups and are less complex, may increase college-going and improve outcomes for disadvantaged students (Bleemer, 2018; Black, Denning and Rothstein, 2020).

In recent years the idea of lottery admissions, even at elite institutions, has gained popularity.[4] The basic argument for lotteries is that, by being transparent, they are more fair, cannot be gamed, and have less potential for bias from admissions officers. Moreover, advocates claim that, subject to the minimum qualifications an institution may set, it becomes difficult for an admissions officer to pick students from an applicant pool who would have better outcomes compared to a lottery. There is little empirical research on the effect of switching to a lottery, though. Simulations of the implementation of random lotteries show mixed results on the demographic composition and academic outcomes of new cohorts of students (Zwick, 2017; Carnevale and Rose, 2003; Baker, 2020). In this paper I contribute to this literature by studying how the replacement of these types

[4]Lotteries are by no means a new idea. As a response to how admissions to elite colleges increasingly correlated with race, ethnicity, and socioeconomic status, Astin (1970) proposed randomizing admissions subject to some screening standard.

of lotteries at many nursing programs affected the composition and outcomes of students.

Additional empirical work on admissions comes from studying the effect of other incremental changes in admissions. The growing test-optional movement, for example, seems not to substantially increase representation of disadvantaged groups, as intended (Belasco, Rosinger and Hearn, 2015; Bennett, 2020). Other interventions, such as providing students with additional information or college counselors, can also be effective (Gurantz et al., 2020; Hoxby, Turner et al., 2013; Castleman and Goodman, 2018).

The topic of the optimal design of admissions is also relevant in the literature on elementary and secondary education, where random lotteries are much more prevalent. Researchers using variation from these lotteries have shown that charter schools in urban settings improve academic achievement (Abdulkadiroglu et al., 2011; Dobbie and Fryer Jr, 2015). Less is known, however, on how these admissions strategies affect enrollment and mobility patterns (Walters, 2018; Avery and Pathak, 2021). Similarly, these issues of fairness and merit come up in efforts to redesign admissions to New York City's Specialized High Schools, which use a unique entrance exam, after reports that very few Black students are admitted (Abdulkadiroğlu, Pathak and Roth, 2009; Shapiro, 2019).

This paper sheds new light on these questions by examining how a particular policy, which completely overhauled admissions at participating programs, affected the composition of new cohorts as well as their eventual academic outcomes.

2.2 Admissions at California's Community College Nursing Programs

The California Community Colleges system consists of 116 campuses and is the largest public higher education system in the country, with more than 2.5 million students enrolled each year. Approximately half of all degrees and certificates are awarded in career-technical fields, of which by far the most popular is the associate's degree in nursing (ADN). The ADN is the minimum qualification to work as a registered nurse, which is one of the occupations with the highest projected demand (Bureau of Labor Statistics, 2015), yet the supply of new nurses has often struggled to keep pace.[5] These trends have led, nationwide, to nursing programs with more

[5] Appendix Figure A1 shows the number of ADN completions, along with other health programs and career-technical programs in general. Despite the large growth in demand for registered nurses, there has not been a noticeable increase in ADN completions over the past decade. The lack of expansion of ADN programs is due in large part to the high cost of expanding programs, as well as a lack of financial incentives based on how community colleges are funded (Grosz,

applicants than there are seats available. In turn, ADN administrators have had to institute admissions policies to ration seats.

ADN programs in California are regulated by the state's Board of Nursing, which establishes minimum standards for programs including guidelines on a program's courses. The prerequisites for application to a program are fairly uniform across the state's community college ADN programs. Each program administers its own admissions process, so the format and particular requirements of an application vary slightly across the state.[6]

In California, there were few restrictions on the types of admissions policies that community college nursing programs could use until the early 1990s. However, in a 1988 lawsuit, the Mexican American Legal Defense and Educational Fund (MALDEF) claimed that admissions and prerequisite policies unfairly excluded Latino students. To avoid litigation, in 1991 the California Community College Chancellor's Office (CCCCO) instituted regulations forcing programs to adopt non-evaluative admissions methods, though they could still require prerequisite coursework (Hill, 2007). Programs adopted different types of non-evaluative methods. Some colleges began waitlists, where students who completed application requirements would be added to the end of the list, and each term's new cohort would consist of the longest-waiting applicants. Other programs adopted "first-come first-served" regimes, where a program would accept applications for the following term's cohort during a pre-set window, admitting students in the order they submitted their applications. Finally, other colleges used random lotteries. Students could often reapply under lotteries and first-come-first-served regimes.

In October of 2007 the California Assembly, responding to concerns about low completion rates in ADN programs, passed AB-1559, which allowed community college ADN programs to adopt "multi-criteria screening" admissions policies, which I refer to in this article as "evaluative" admissions. Beginning January 1, 2008, ADN programs were now allowed to make admissions decisions based on a set list of criteria: prior academic degrees, diplomas, and certificates; GPA

2020; Stange, 2015).

[6]In general, programs require a C or C+ average (GPA of 2 to 2.5) in approximately 30 units—one year's worth of full-time work—in courses such as anatomy, physiology, chemistry, microbiology, psychology, and algebra. Some of the courses, such as algebra, might have their own prerequisites. Some programs have separate GPA requirements for a student's cumulative performance over all coursework and performance within the prerequisites themselves. Students may fulfill their prerequisites at certain other colleges within and outside the California Community College system, though the set of accepted courses varies by program. Programs also tend to require First Aid and CPR certifications, as well as other types of documentation such as high school diplomas and proof of eligibility to work.

in relevant courses; work and volunteer experience; life experiences; and proficiency in certain other languages. Nursing program administrators favored the change, believing that having more control on the set of students to their programs would lead to increases in completion rates and pass rates on the NCLEX-RN exam, which are key contributors to a program's funding.

Despite the new regulations, programs did not immediately institute the new policies. The first program to take advantage of this new policy changed its admissions process to admit the cohort of fall 2008. As shown in Figure 1, by 2018, 41 colleges had changed their policies, accounting for approximately half of the ADN programs in the state (California Community College Chancellor's Office, 2016). The gradual adoption of multi-criteria screening processes since 2008 allows me to identify how these policies affect the composition of the incoming student body relative to the other admissions regimes. As I discuss in more detail in section 4, the timing of each program's adoption of the new processes does not seem to be related to observable program and local labor market characteristics.

3 Data

I use detailed individual-level administrative records from the California Community College Chancellor's Office (CCCCO) from 1992 to 2019. These records include course-taking behavior, information on degrees and certificates earned, and demographics. The data also contain information on the content of each course. This includes the title of each course (e.g. Introduction to Nursing 101) as well as its classification according to the Classification of Instructional Programs (CIP) codes, used by the National Center for Education Statistics to identify the programmatic content of courses and programs.

I create a sample that consists of the students who begin an ADN program at any California community college since 1992. In California, all ADN programs are oversubscribed, necessitating admissions processes.[7] This means that any student who enrolls in a course associated with an ADN program is a student who was admitted to the program. I am not able to observe the set of applicants to nursing programs, since admissions processes are run by the individual programs

[7] To provide evidence for this claim I reviewed application instructions on program websites, which all suggested that there were fewer seats available than eligible candidates. Conversations with program deans and system-wide administrators provided further support.

rather than the CCCCO.

Table 1 shows summary statistics for new entrants to ADN programs in the fall of 2007, the last term before the California legislature passed the law allowing for evaluative screening processes. Over 80 percent of incoming students are women, and the racial and ethnic composition of new entrants are largely similar to that of California's community colleges in general. An important feature of ADN programs, and community college CTE in general, is that new students were 32 years old on average. A striking feature of the table is that, on average, students spent approximately six years between their first enrolled term at a community college to their first enrollment in a nursing program.[8] These are years that students spend enrolling in prerequisite coursework, and then waiting to win a lottery or have their name called off a waitlist. In general, students must enroll in 25-30 units worth of prerequisite coursework, or approximately a year of full-time coursework. This means that, on average, students who entered nursing programs spent five years longer than the minimum waiting to enter a program. Of course, because I cannot observe applications I cannot state with certainty that students spend all five of these years waiting; they may take time off from coursework in between or may take longer than one year to complete the prerequisite coursework. Table 1 also shows that half of all students earned some kind of degree or certificate within three years of starting the nursing program. Most completing students earned an ADN. Finally, although nursing programs usually consist of two years of full-time coursework, students who did earn an ADN took 2.9 years to complete their programs.

4 Methodology

4.1 Specification

I am interested in the causal effect of a program changing its admissions process. I leverage the fact that, while many programs eventually adopted evaluative admissions, there was variation in the timing of adoption. In this context the usual approach is to estimate a two-way fixed effects

[8] The median is five years.

(TWFE) model:

$$y_{ict} = \alpha_1 + \sum_{\substack{g=G_{min} \\ g\neq 0}}^{G_{max}} \beta_g \mathbf{I}(t - T_c = g) + \delta_c + \delta_t + \mu_c * t + e_{ict} \tag{1}$$

where y_{ict} is a characteristic or outcome for student i who starts in the ADN program at college c as a member of cohort t. The subscript g refers to the years since the program adopted its new evaluative screening admissions rules. The year where $g = 0$ is the year prior to the first one where a cohort would have been admitted based on the new rules, denoted as T_c. Omitting this event year ($g = 0$) means that the coefficient β_g is expressed relative to the year prior to the change. This specification also controls for college effects δ_c, cohort effects δ_t, and program-level linear time trends $\mu_c * t$..

Recent research has called into question the validity of the TWFE approach in differences-in-differences models with staggered adoption timing (Callaway and Sant'Anna, 2020; Sun and Abraham, 2020; Athey and Imbens, 2018; Goodman-Bacon, 2018*a*). The TWFE model estimates a weighted ATT, where units that are treated far from the endpoints of the time period—in this case the earliest and latest colleges to change their admissions— are given more weight. This means that if there is heterogeneity in treatment effects, TWFE is not estimating the ATT. Moreover, the presence of dynamic treatment effects could lead to significant bias in an estimate of the dynamic ATT.

To account for these issues I implement an estimator developed by Callaway and Sant'Anna (2020), hereafter CS. In the notation used by CS, $Y_{it}(0)$ is the untreated potential outcome, in this case some characteristic of the incoming cohort, for nursing program i in academic year t, for $t = 1, .., \tau$. Similarly, $Y_{it}(g)$ is the program's potential outcome in year t if the program first adopts the new admissions rules in year g. For programs that did adopt the new rules, the indicator G_i is the academic year in which they made the change.

For programs that never adopt the new rules, the observed outcome in year t is $Y_{it} = Y_{it}(0)$. For programs that do adopt the new rules, the observed outcome in year t is either the untreated potential outcome $Y_{it}(0)$ for years prior to adoption, or the treated potential outcome $Y_{it}(G_i)$ after adoption:

$$Y_{it} = 1\{G_i > t\}Y_{it}(0) + 1\{G_i \leq t\}Y_{it}(G_i)\} \tag{2}$$

There are two types of parallel trends assumptions that must hold in order for the estimate of the ATT to be causally identified. First, the average potential outcomes for colleges that never switched and colleges that switched in year g would have followed parallel paths for every year following g. Second, the average potential outcomes for colleges that switched in year g would have followed parallel paths following the year g as those of colleges that switched later, had these colleges also switched in year g. In other words, this is an assumption that colleges that have not yet switched are a valid comparison for colleges that have already switched. If these assumptions hold, then the group-time average treatment effects are:

$$ATT(g,t) = E[Y_t(g) - Y_t(0)|G = g] \tag{3}$$

CS estimate the overall ATT, θ_S^O, as a combination of the group-specific ATT, $\theta_S(g)$, defined for each group G_i, as follows:

$$\theta_S(g) = \frac{1}{\tau - g + 1} \sum_{t=2}^{\tau} 1\{g \leq t\} ATT(g,t) \tag{4}$$

$$\theta_S^O = \sum_{g=2}^{\tau} \theta_S(g)(G = g) \tag{5}$$

The dynamics of the treatment effect are captured in estimates of the ATT at different numbers of elapsed treatment years e:

$$\theta_D(e) = \sum_{g=2}^{\tau} 1\{g + e \leq \tau\} ATT(g, g+e) P(G = g|G + e \leq \tau) \tag{6}$$

For this analysis, I present and plot estimates of θ_S^O and of $\theta_D(e)$ for each outcome.[9]

In the appendix I also explore two other approaches to estimating these effects. The first is the standard TWFE, which I include in order to provide a comparison to CS. As this is still an active area of research, the comparison between the two results can be informative. The second is a so-called "stacking" approach by Cengiz et al. (2019) that previous research has found to perform

[9]To actually implement the estimator proposed by Callaway and Sant'Anna (2020) I use their "did" package in R, available at https://bcallaway11.github.io/did/.

similarly to the CS estimators. The three estimators yield qualitatively similar results, though with a few differences that I explain further in section 5.5.

4.2 Dependent Variables

The outcomes I study correspond to the three research questions. First, I am interested in whether the academic preparation of the incoming cohorts changed. I examine credit accumulation and GPA in the first two years a student is in the community college, before entering the ADN program. I can also observe the number of basic skills courses in which they enroll in that same period. Changes in the academic preparation of new cohorts could be brought about by three mechanisms. Students could change their application patterns in response to the announcement of the new admissions rules. By changing the policy, admissions committees would also switch which applications they select. And finally, the change in policy might also affect matriculation rates among accepted students if the newly accepted students place a higher or lower value on the program than previously accepted students. I cannot differentiate between these mechanisms; nevertheless, the composition of the new cohorts is the policy-relevant outcome.

Next, because the original legal impetus came from a group claiming that evaluative screening admissions adversely affected Latino applicants, I am particularly interested in the racial and ethnic composition of the incoming students. I also look at the gender and age composition.

I next observe academic outcomes. The rationale espoused by supporters of the 2008 law change was that completion rates would increase and attrition rates would drop (Hill, 2007). I examine student GPA in the first year of the program as an intermediate outcome. I then look at two different completion outcomes. The first is whether a student completed an ADN and the second is whether a student completed any community college degree or certificate. I define both completion outcomes as occurring at any of the community colleges in the state. I also include the share of students who pass the national licensing exam, the NCLEX-RN, two years after the cohort began.[10]

The last type of outcome I study concerns the delay students experience between deciding to

[10] I am not able to identify which students sit for the exam. However, nursing programs require approximately two years of full-time coursework, so I assume that students taking the exam would be two years removed from their start of their program, if they finish on time. .

embark upon a nursing degree and actually entering a program. I have already shown that wait times for students entering nursing cohorts were high, requiring several years beyond the expected amount of time needed to complete prerequisite coursework. Replacing waitlists and other lengthy application systems with evaluative admissions might make the entire process more efficient and allow admitted students to more quickly complete the program and enter the labor force quicker.

4.3 Threats to Identification

There are two main potential threats to the identification strategy. First, there would need to be college-level shocks that are more likely to occur in the same event year. Because of the staggered adoption of the new admissions rules, any kind of calendar-year shock is accounted for. On the other hand, if colleges were hit with a particular type of shock in the second year following their adoption of the new rule, this would create a problem. It is unlikely that this type of coincidence would occur, however.

More worrisome is the potential that the timing of the adoption of the new criteria is endogenous. Certain types of colleges may be more likely to adopt earlier relative to others, which could lead to biased results. From conversations with program administrators and local news reports, I found that the reasons for the timing of the change are likely idiosyncratic and fall into two categories. The first consists of administrative hurdles to surpass in order to adopt the evaluative screening process allowed by the new policy. These include establishing reporting processes to state administrators and assembling working groups in order to choose what criteria to use for admissions. Second, some colleges with waitlists decided to wait a few years so that their list length would shorten, before implementing the new admissions. Not all colleges with waitlists did this, though. Some gradually did away with their waitlists. For both these reasons, it is not clear that these decisions, and their relationships to the timing of the admissions process switch, would be correlated with the quality of the program or the outcomes I am interested in.

To empirically understand the heterogeneity in the timing of adoption of new admissions approaches, I regressed the semester of adoption on a variety of college and county characteristics, measured in 2007. This exercise is illustrated in Figure A2, which plots the month of adoption on the vertical axis and the 2007 program-level mean on the horizontal axis. The size of each marker represents the size of the program. Appendix Table A1 shows the coefficient estimates. There does not seem to be a systematic pattern between the timing of adoption to these characteristics, and some are likely statistically significant merely by chance. Thus, there is less concern that the timing of the adoption of the new rules is endogenously determined.

Finally, as discussed in an earlier section, the assumption of parallel trends is crucial. In the sections that follow I plot estimates of $\theta_D(e)$ in years prior to and following a college's switch. The pre-treatment estimates show relatively parallel trends prior to treatment. This is not proof that the parallel trends assumption holds following treatment, of course, but it is evidence of parallel trends prior to treatment. Together with the previous tests of the endogeneity of treatment, these exercises give credibility to the strength of this research design.

5 Results

Figures 2-4 plot event study coefficients for characteristics and outcomes of incoming ADN cohorts. Tables 2-4 display the post-treatment coefficients for each of these specifications. These are estimates of $\theta_D(e)$ for $e > 0$. Tables 2-4 also show the overall ATT estimate θ_S^O, effectively an average of the year-by-year treatment effects for the first four years of the post-implementation period.

5.1 Academic Preparation and Background

As discussed previously, the impetus for the 2008 legislation that allowed evaluative admissions was college administrators worrying that systems like lotteries and "first come first served" admissions do not admit the students most likely to succeed in the program. Since funding is often tied to completion rates and NCLEX-RN pass rates, student academic preparation is of primary concern to administrators. At face value, changing admissions policies to accept students based on a set of agreed-upon variables should result in new cohorts of students having higher average composition along those variables. However, this might not occur for a few reasons. First, take-up rates of admissions offers at nursing programs are relatively low, around 50 percent, and there may be selection in takeup that counteracts the effects of the admissions decision (Grosz, 2020). Certain non-evaluative admissions policies do in fact serve to select students on certain attributes. "First come first served" policies, for example, reward the most proactive students, who apply the earliest. Waitlists reward applicants who are willing to wait. A final reason why the adoption of evaluative screening processes may not result in substantial changes in cohort quality is that there may be little variation to begin with. ADN program applicants must meet a high GPA standard in extensive prerequisites in order to be eligible to be considered for admissions.

Table 2 and Figure 2 show the event study estimates for a set of academic background variables. Overall GPA is flat in the pre-period, providing evidence of parallel trends necessary for the validity of the research design. In the first year following implementation, GPA jumps by 6.7 points, with an upward trend in the following years. Overall, the average effect of changing admissions on new cohort GPA is 7.3 points. There is also a large and positive effect on GPA in subjects that are explicitly targeted by evaluative admissions processes. For math coursework, GPA rose by 6 points, and it rose by 7 points for biology. For both these topics, there is a clear upward trend following the initial jump.

While GPA increased, the number of units that students entered with decreased. Students who enrolled after the change to evaluative admissions had attempted approximately 2 fewer units. This difference is not statistically significant, but the coefficients for some of the post-period are. Still, this is a relatively small effect, comprising less than one full-time course credit and just a 3.7 percent decrease.

Another informative measure of a student's academic preparation prior to application in an ADN program is whether they took remediation coursework. Students must typically take these courses, primarily in math and English, before being able to proceed to college-level coursework. Table 2 and Figure 2 show that the share of new nursing students who had taken one of these courses in their first year of community college dropped, by 3.3 percentage points, following the adoption of the new admissions rules. This amounts to an approximately 7 percent decrease in the share of students who had taken remedial coursework.

Taken together, these results show that the change in admissions procedures does seem to lead to cohorts of incoming students with better academic preparation, as intended by the legislation.

5.2 Demographics

The purpose of the original ban on evaluative admissions processes was, as claimed by the group that brought forth a lawsuit, to counteract adverse acceptance rates for minority students. Figure 3 and Table 3 examine the effects of switching to an evaluative admissions policy on demographic characteristics.

Recruitment of male nurses has been a priority in recent years in an occupation that has historically been primarily female (Evans, 2013), and one way to increase the number of male nurses would be to accept more men into nursing programs.[11] Panel a) of Figure 3 shows no discernible effect on the share of male students following implementation of evaluative admissions.

There does not seem to be an effect, either, on the racial and ethnic composition of the new cohorts. The coefficients on the effect of implementing evaluative admissions on the share of a program's students that were White, Black, and Latino students are all small and not statistically significant. Moreover, in all three cases the confidence intervals allow me to rule out modest impacts. For example, while the share of White students might be expected to rise as a result of the new admissions, the estimates allow me to rule out an increase of more than 3 percentage points, or approximately 7 percent. Similarly, the results allow me to rule out decreases in the Black and Latino student body of 1.6 percentage points and 2.8 percentage points, respectively. These amount to small percent decreases. Thus, this analysis does not support the argument that multi-criteria screening processes do crowd out minority students. In a robustness exercise explained in a later section, where I use two different estimation approaches, I do find some evidence of small statistically significant increases in the share of White students.

The final column of Table 3 and panel d) of Figure 3 show the effect on mean age of new ADN students upon admission. The figure shows no discernible effect in positive event years. Appendix Table A2 and Figure A3 examine subcategories of age. Although not precisely estimated, the results suggest that, potentially, the share of younger students increased somewhat, along with declines in the shares of students between 30 and 40. In additional robustness exercises I find some stronger support for these effects. But, on the whole, I do not find systematic evidence that the age of new students changed much.

Overall, my results do not support the claims made to the CCCCO in the early 1990s. Relative to non-evaluative admissions, evaluative admissions seem to select similar students in terms of their gender, race, ethnicity, and age.

[11] According to the legislation, of course, admissions committees are not allowed to use gender as a basis for admissions.

5.3 Academic Outcomes

The demographic effects of evaluative admissions policies were the basis for the original ban. However, the potential benefit of evaluative admissions, in the form of improved academic outcomes, was the reason for their reinstatement. Although I find substantial effects on measures of academic preparation, evaluative admissions also involved essays and other components, which committees could be using to choose students better posed for academic success. To analyze whether evaluative admissions as a whole led to improved outcomes I observe the effect of the admissions rule changes on cohort academic performance.

The first column of Table 4 shows the number of years between a student's first course in a community college and first semester in an ADN program. There is a pronounced decline following the implementation of the new admissions policies. Over the four years, the average student under the new regime waited 0.38 years less, a six percent decline. In fact, later cohorts face even larger declines, of more than half a year. This incremental change likely comes from the fact that some colleges with waitlists did not do away with their waitlists immediately. Instead, some colleges allowed a smaller proportion of students to be called off the waitlist, while not allowing any new students to be added to the waitlist. In a later analysis I show that that, in fact, the entire reduction in wait time comes from programs with waitlists. Although I cannot observe the behavior of rejected applicants, the shortening of their wait likely leads them to return to the labor market sooner or to apply to other programs.

Earlier I showed evidence of whether the new policies affect student academic background: the mean overall GPA of incoming students increases, and fewer take remediation classes for example. Table 4 and Figure 4 show that academic outcomes did not improve following the policy change. There is a positive coefficient on student first-year GPA, but it is not statistically significant. The share of students who completed any degree is unchanged, and the coefficients even have a negative sign. Likewise, the share of students who complete an ADN is unchanged.

The final subfigure in Figure 4 shows the effect of changing admissions on the college's pass rates on the National Council Licensure Examination (NCLEX), which is required of nursing program graduates in order to work as nurses. Data on program-level pass rates are available at the program level, but not at the individual level, going back to the 2003-2004 academic year. I cannot match individual students to their NCLEX score, nor do I know for certain how many students from each cohort take the exam each year. Because ADN require approximately 2 years of full-time coursework, I match the NCLEX pass rates to the cohort two years prior. Pass rates are already high, 88 percent, meaning that there is little room for upward movement. I find no change in the pass rate due to the policy. In fact, the coefficient is negative, and is statistically significant in the first year after implementation.

A lingering question is whether the negligible effects I find of the policy on outcomes are merely due to an underpowered study design. On the other hand, the measures of academic quality used for admissions, especially grades, may not be good predictors of student success in the program. Community college CTE students are more likely to be older, work part-time, and have families to take care of, so failure to complete is often a result of "life getting in the way" (Goldrick-Rab, 2010) rather than a lack of academic preparation. One way to answer this question is to ask what increase in completion rates an admissions officer designing an evaluative admissions policy would expect given the changes in the composition of the new cohorts. That is, if these characteristics predict success in the program, then they can be used to estimate the potential effects of the new admissions policy on completion.

Table 5 shows this relationship between the observable inputs of new cohorts prior to 2008—the first year of evaluative admissions—and their eventual ADN and overall degree completion. These are linear probability models that control for college, calendar year, and college-specific trends, and are not meant to have a causal interpretation. Rather, Table 5 demonstrates that these characteristics, which are observable to the admissions officer, strongly predict completion; they can help relate the main estimates of the effects of changing admissions strategy on inputs to those on outcomes.[12]

In particular, Table 2 showed that changing admissions policy increased the GPA of students by 7.3 points. According to the results in Table 5 this increase in GPA would imply that ADN completion rates would increase by 0.7 percentage points. This is a small effect. A similar calculation shows that the increase in Biology GPA of incoming students would only increase completion rates by 0.5 percentage points. Students who took remedial coursework were 4.13

[12] Appendix Table A3 shows that these results are robust to the inclusion of college fixed effects and trends.

percentage points less likely to complete the program; combined with the 3.3 percentage point decline in the share of these students in incoming cohorts, this amounts to an implied increase in the completion rate of just 0.1 percentage points.[13]

These estimates of the effect of the policy on completion rates are within the confidence intervals shown in Table 4, where I explicitly estimate the effect of the policy on completion rates, so I cannot rule them out. Still, these are small effect sizes for a policy expressly intended to boost completion.

In sum, I find no evidence that changing admissions policies improved completion rates. This lack of effect comes in spite of increases in measures of student ability, which suggests that the screening variables used by these programs may not be ones that predict student success.

A limitation of using course performance and degree completion as outcomes is that these measures are subjective: if institutions base course grades on relative comparisons within cohorts then grades would be unchanged. The state's Board of Nursing dictates which courses form the curriculum, but it does not standardize grade performance within those courses. I cannot actually observe whether program administrators, or individual faculty, change grading procedures as the composition of the student body changes. But, I believe this behavior to be unlikely. The California legislature's stated purpose in allowing evaluative admissions was to increase completion rates; similarly, individual programs adopted the new measures in order to increase their own completion rates and thus be eligible for additional funding.[14] It would seem counterproductive for these programs to have then hampered their own efforts by making courses more difficult. Appendix Figure A12 shows that, prior to 2008, there was significant variation in the annual change in GPA for first-year nursing students. The within-program standard deviation was 0.20 GPA points. This is evidence that, at least prior to the policy change, there did not seem to be rules keeping average grades constant over time. If, however, colleges had adjusted their grading then this would only provide more evidence of how blunt a tool admissions can be for the purposes of improving completion rates in this context.

[13] Using the upper bound of the confidence intervals for these estimates would only give an increase in the completion rate of 1.0 percentage point in the case of overall GPA, and a similar number in the case of biology GPA. In the case of remedial coursework, taking the upper bound would imply just a 0.25 percentage point increase in completion rates.

[14] An ADN or a bachelor's degree in nursing is a requirement for sitting for the national licensing exam. A drop in completion rates would then make fewer students eligible to take the exam.

5.4 Previous Admissions Type

The main results may differ by the type of admissions the programs used prior to the change. The effects of evaluative admissions might be most pronounced at programs with random lotteries, where endogenous selection on student characteristics would be the least pronounced. On the other hand, the improvement in wait time might be concentrated among programs with waitlists. Of the 44 programs in the sample that switched their admissions, 12 had waitlists, 28 had lotteries, and 4 had first-come-first-served. The collection of the admission type data is complicated, though, by the fact that the available archival college catalogs do not fully describe the lottery process: some lotteries are not purely random and give additional weight to certain applicants. For these reasons, I separate the programs into the 12 waitlist programs and the 32 other programs that include some amount of randomness. Tables A4-A6 show these results.

The main results are almost all the same, qualitatively, though I lose power for some analyses. The most notable difference is that waitlist programs that switched to evaluative admissions had the most pronounced drops in wait time between starting community college and enrolling in an ADN program. By eliminating waitlists, these programs reduced the wait time by almost a full year. Lottery and first-come-first-served programs, on the other hand, had much smaller drops in wait time, if at all. The overall drop of almost a full year for waitlist programs hides the fact that the declines in wait time were gradual. Appendix Figure A4 shows the event study results for wait time. There is a sudden, small, and statistically insignificant drop in wait times for programs with lotteries and first-come-first-served admissions. On the other hand, the decline for waitlist programs is large and monotonic: by the third and fourth years following the change the wait times had declined by more than 1.5 years. This evidence is consistent with programs gradually doing away with their waitlists, and potentially represents a benefit to admitted and rejected students.

5.5 Alternative Specifications

In this subsection I present results using different methodological approaches used by researchers studying staggered rollouts.

First I present results from the standard two-way fixed effects (TWFE) approach, which is commonly used in studies with staggered timing of policy adoption and was described in equation

1. New econometric research has shown that this method is prone to bias in estimates of the ATT, particularly in the presence of dynamic treatment effects, which is why I implement the Callaway and Sant'Anna (2020) approach. Nevertheless, because of the prevalence of the TWFE in studies of this kind, I find it useful to include these estimates as a comparison.

Following Goodman-Bacon (2018*b*) and Naven (2017), I identify the maximum set of event years possible when using a balanced panel. This corresponds to the number of pre-event years for the earliest adopting college and the number of post-event years for the latest adopting college.[15] Then, I "bin" the additional years by including all available earlier and later years of data that would not be included in the balanced panel, using these to estimate one coefficient prior to the first event year and one coefficient after the last event year. This creates an unbalanced panel which has still has the property that the coefficients of interest are identified with a balanced panel of colleges.[16]

Appendix Figures A6-A8 and Appendix Tables A7-A9 show results from this exercise. These are analogous to the tables and figures presented in the main results section. For the most part, the results are similar to the estimates using the CS method. However, there is one main source of difference in the results on the demographic composition of new students. Using the TWFE approach, I find that students in cohorts after the admissions change were 2.5 percentage points more likely to be White. Using the CS approach, I estimated this effect as a precise zero. Nevertheless, this effect, while positive, is small in magnitude. The coefficient estimates on the effect of changing admissions on the share of students who were Black or Latino are negative but not statistically significant.

I also implement a method used by Cengiz et al. (2019), whose main concern with TWFE, similar to CS, is the bias due to differential weighting of treated units based on their timing. Cengiz

[15] In other words the minimum number of event years for which each college is represented the same number of times.

[16] In the context I study, the first adopting college changed its rules in the Fall of 2008. Because the data go back to 1992, this means that the maximum number of pre-event years of data in a balanced panel is 16. Later adopters, like a college that changed its rules in the Fall of 2009, will have 17 or more years of pre-event data available. To create the "loaded" panel I code all event years greater than 16 as the 17th year of pre-event data. This allows me to use more years of data for some colleges to identify the trends, though the main coefficients are still identified with a balanced panel of colleges. I use a similar approach for the post-event years. Because programs are still in the process of announcing plans to adopt the new admissions policies, there is no "last" adopter. Instead, I estimate up to four years of post-event coefficients given the data I have, which go up to 2017, and drop all adopters after 2017 from the analysis. By doing this, I can then "load" the fifth post-event coefficient with all event years after the fourth, in an analogous fashion as with the pre-event loading.

et al. (2019) treat each program that changed its admissions as a separate event, and estimatesan individual effect for each event. The overall effect is the average of all the event-specific effects.[17] I estimate the following equation:

$$y_{cth} = \alpha_0 + \sum_{\substack{g=G_{min} \\ g\neq 0}}^{G_{max}} \gamma_g \mathbf{I}(t - T_c = g) + \xi_{ch} + \zeta_{th} + u_{cth} \tag{7}$$

where y_{cth} is the mean characteristic or outcome for students in the ADN program at college c as members of cohort t in the sub-dataset h. The subscript g refers to the years since the program adopted its new evaluative screening admissions rules. The year where $g = 0$ is the year prior to the first one where a cohort would have been admitted based on the new rules, denoted as T_c. I omit this event year ($g = 0$), meaning that the coefficient γ_g is expressed relative to the year prior to the change. The coefficients γ_g give the difference between the outcome in event years g and event year 0. I also control for college-dataset effects δ_{ch} and cohort-dataset effects δ_{th}.

Appendix Figures A9-A11 and Appendix Tables A10-A12 show results from estimates of equation 7. For the most part, the results are similar to the main results, as well as those described using the Cengiz et al. (2019) stacked approach. The sole exception, as with the TWFE, is in the results on the demographics changes brought about by the adoption of evaulative admissions. I find small increases in the share of students who were White.

5.6 Changes relative to non-adopting colleges

The event study design only uses data from colleges that eventually adopted the new admissions policies within the time period. However, non-adopting colleges may also see the composition of their new cohorts change if other colleges change their admissions. Colleges implementing new evaluative admissions might attract better students by drawing them away from colleges with lotteries. Or, the new admissions might just be a way for colleges to change the selection from among their own pool of applicants. Without information on the applicant pool, however, I cannot

[17] To implement, I create h individual datasets, each including data for the focal program as well as all other never-treated programs and not-yet-treated programs that would not change their admissions policies until at least four years after the focal program. I then pare the years of data to include just 10 years prior to the year the focal program changed its admissions, and four years after. Finally, I stack each of these h individual datasets together to form a long analysis dataset.

answer these questions in a causal way.

Figure A5 shows mean characteristics of incoming students, net of college fixed effects and college-specific linear time trends, at colleges that did not have evaluative admissions. In none of the figures is there a clear break in trend before and after 2008. Of course, this analysis is limited by being just a pre-post design, but still does not show evidence of any changes in the composition of the incoming cohorts and non-adopting colleges.

As another test, I ask whether a college sees its cohort composition change if nearby colleges change their admissions. The intuition is that the market for nursing school applicants might be local to smaller geographic areas. So, if changing admissions merely reshuffles the applicant pool across colleges, then colleges that did not change their admissions should see declines in white students and declines in academic preparation. Specifically, I implement an event study similar to that in equation 1. The event, in this case, is the earliest year that a nearby college switched its admissions, and I limit the sample to colleges that never changed their admissions. I define the set of local colleges in three ways: colleges in the same county, colleges within a 25 mile radius, and colleges within a 100 mile radius. Approximately 75 percent of colleges had at least one other college within 25 miles, and all colleges had at least one college within 100 miles.[18]

Table 6 shows the results. If colleges that changed their admissions were diverting students from non-adopting colleges, then the results in the table would have the opposite sign as in the main results in previous tables. Instead, I find results that are not statistically significant, but of the same sign. The results are robust to the three different definitions of the local area. Thus, this is additional support for the idea that colleges are not diverting students from other colleges, but instead are changing selection patterns from within their own set of applicants.

6 Conclusion

In this paper I measure how a change in a community college ADN program's admissions policy affects the composition of its incoming cohorts as well as the eventual academic outcomes of its students. I leverage a statewide policy in California that led many of the states ADN programs to

[18] Results are not sensitive to different lengths of the radius between 25 and 100 miles. Definitions of a smaller radius drop many colleges. For example, only half the colleges had another college within 10 miles.

switch their admissions from lotteries and waitlists to ones that relied on evaluative measures such as grades and work experience. I find that admissions strategies at ADN programs that are based on evaluative measures do not lead to improved completion rates for students.

There are several potential explanations for this result. The allowable types of measures may not contain valuable information for screening applicants to ADN programs. Nursing programs involve difficult coursework, but the most demanding aspects of the program are often the field placements at local hospitals, the clinical labs on site, and outside responsibilities.[19] For this reason, GPA in biology or math may not be a useful way to determine who will successfully complete an ADN program. Second, even if the components included in the new admissions policies were useful in predicting success among applicants, there may not be enough variation among applicants for these components to be useful. This is the argument made by Kamenetz (2019) and others calling for a reassessment of admissions at elite colleges in response to the Varsity Blues admissions scandal.

For these nursing programs, admissions strategies that rely on evaluative measures do not seem to be an improvement over lotteries or waitlists in terms of the academic outcomes of the students and the diversity of the incoming cohorts. More broadly, the findings from this paper serve to highlight some of the key issues that policymakers must grapple with when designing or redesigning admissions policies. First, what is the program or institution's goal? The legislation that reinstated evaluative admissions at California's community college nursing programs sought to specifically increase completion rates. However, the demographic and socioeconomic composition of nursing school students is another important consideration, as is the time students spend engaging in the process of taking prerequisites and waiting to eventually be accepted into a program.

This paper also suggests that changing admissions may not be the most effective tool for institutions to improve their completion rates. In this case even a large increase in the average prior academic performance of new nursing cohorts did not budge completion rates. In fact, as shown earlier, the correlation between completion rates and prior academic performance yields an a priori back of the envelope prediction that even sizeable increase in prior GPA would have

[19] I gained this information from a focus group and semi-structured interviews with program administrators and new students, performed in 2015.

had minimal effects on completion. In addition, evaluative admissions are more expensive than maintaining a waitlist or running a random lottery, and they are more politically fraught.

There are other ways to increase the number of completions, which may be more expensive per student yet may also be more effective. One way is along the extensive margin: in prior work I have shown that the benefits of adding seats to nursing programs, in terms of labor market outcomes and spillover effects, vastly outweigh the high costs (Grosz, 2020). Another way is to improve completion rates for students who are already enrolled. Increases in wraparound services such as mentoring and career counseling have positive effects in many community college programs (Scrivener et al., 2015).

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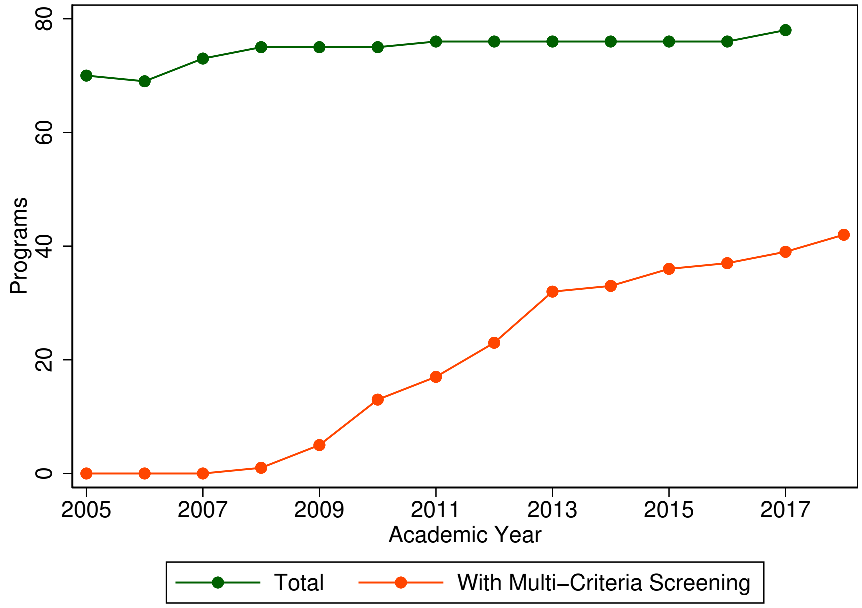
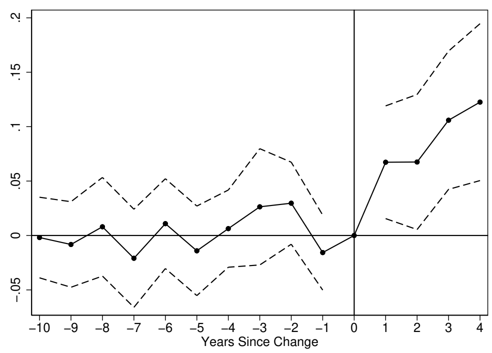
Figure 1: Adoption of Multi-Criteria Screening Processes

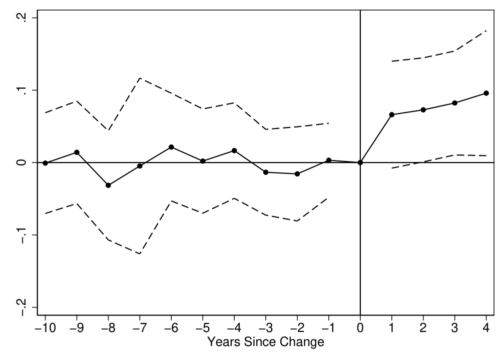


Notes. This figure shows the number of total ADN programs as well as the number of programs with multi-criteria screening processes. The number of ADN programs is based on data from the California Community College Chancellors Office (CCCCO) up to the 2015-2016 academic year. The number of programs with multi-criteria screening processes is based on publications of the CCCCO, including for years in which there are not yet data.

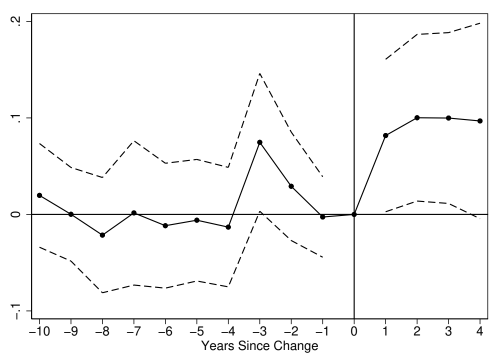
Figure 2: Main Results, Academic Background



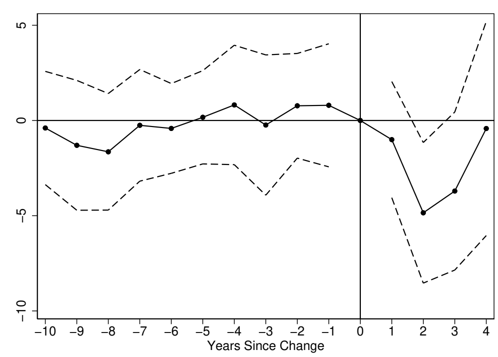
a) GPA



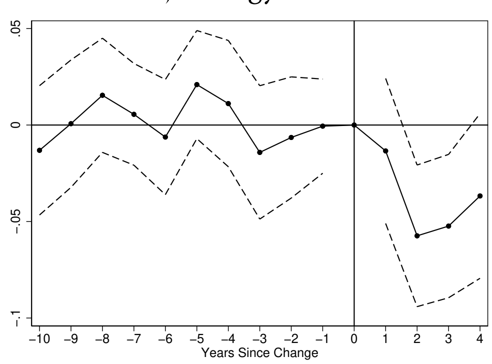
b)Math GPA



c) Biology GPA



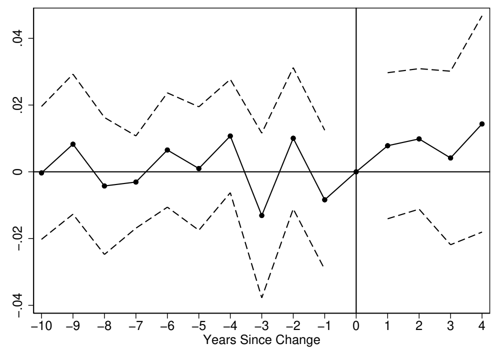
d) Units Attempted



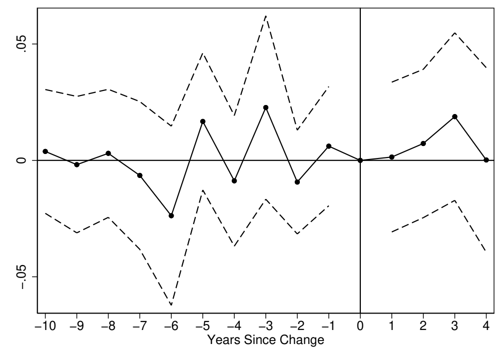
e) Basic Skills Courses

Notes. Figures show point estimates and 95 percent confidence intervals of results from regressions of equation 6. Standard errors clustered at the program level.

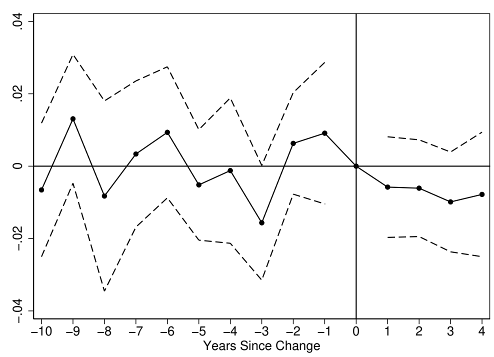
Figure 3: Main Results, Demographics



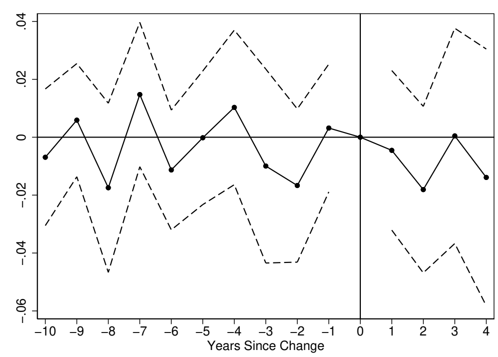
a) Male



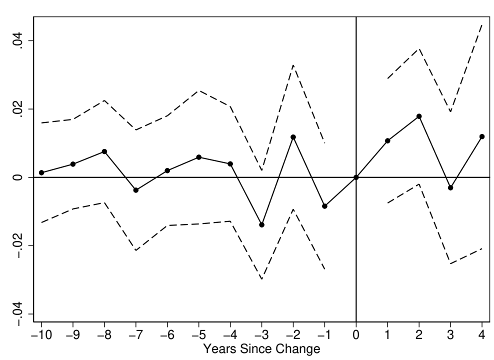
b)White



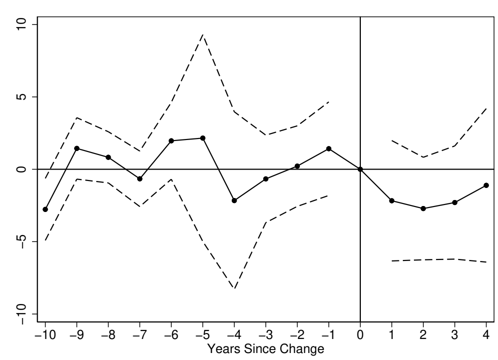
c) Black



d) Latino



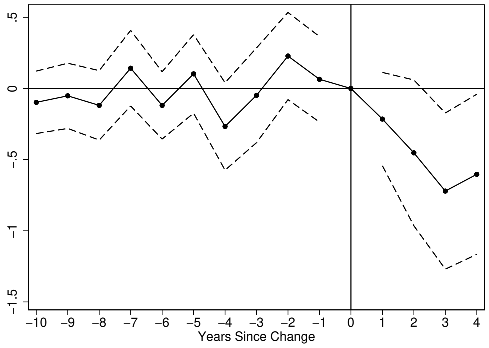
e) Asian



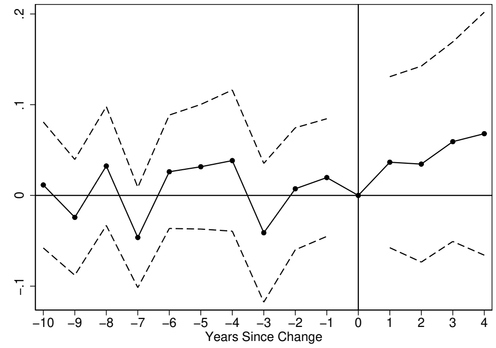
f) Age

Notes. Figures show point estimates and 95 percent confidence intervals of results from regressions of equation 6. Standard errors clustered at the program level.

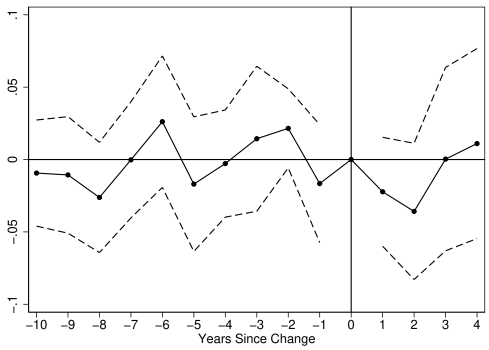
Figure 4: Main Results, Outcomes



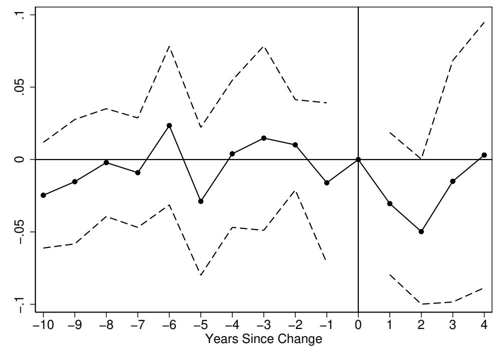
a) Time to Start



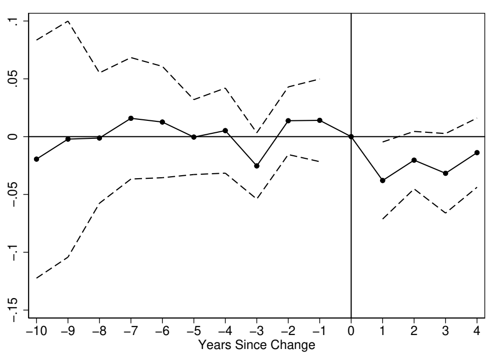
b) First-Year GPA



c) Any Degree



d) Complete ADN



e) NCLEX-RN Pass Rates

Notes. Figures show point estimates and 95 percent confidence intervals of results from regressions of equation 6. Standard errors clustered at the program level.

Table 1: Summary Statistics for Students of Fall 2007 Cohort

A. Demographics	
Male	0.198
	(0.399)
White	0.415
	(0.493)
Black	0.0780
	(0.268)
Hispanic	0.245
	(0.430)
Asian	0.135
	(0.341)
Other Race	0.127
	(0.333)
Age	32.36
	(51.97)
B. Academic Preparation	
GPA	2.979
	(0.750)
GPA in Math	2.791
	(1.001)
GPA in Biology	2.930
	(0.823)
Units Attempted	57.72
	(40.87)
Basic Skills Courses	0.468
	(0.499)
Time to Enrollment	6.331
	(4.871)
C. Academic Outcomes	
Any Degree, 3 Years	0.555
	(0.497)
Any Nursing Degree, 3 Years	0.398
	(0.490)
Time to degree	1.108
	(3.630)
Time to Nursing Degree	2.873
	(1.301)
First-Year GPA	3.052
	(0.878)
Observations	6221

Table 2: Main Results, Academic Background

	(1) GPA	(2) Math GPA	(3) Bio GPA	(4) Units	(5) Basic Skills
A. Event Study					
Event Year +1	0.0673*	0.0662	0.0818*	-1.01	-0.0134
	(0.0264)	(0.0377)	(0.0403)	(1.56)	(0.0192)
Event Year +2	0.0675*	0.0729*	0.1*	-4.85*	-0.0574**
	(0.0317)	(0.0367)	(0.0441)	(1.88)	(0.0187)
Event Year +3	0.106**	0.0823*	0.0999*	-3.71	-0.0524**
	(0.0324)	(0.0366)	(0.0452)	(2.12)	(0.0189)
Event Year +4	0.123***	0.0959*	0.0969	-0.421	-0.0367
	(0.0368)	(0.0441)	(0.0517)	(2.87)	(0.0218)
	.	.	.	.	.
Y-mean	3.03	2.82	2.9	59.3	0.466
N	1340	1334	1335	1352	1352
B. Differences in Differences					
Post Change	0.0733***	0.0594*	0.0692	-2.26	-0.0334*
	(0.0216)	(0.028)	(0.036)	(1.54)	(0.0155)
	.	.	.	.	.
Y-mean	3.03	2.82	2.9	59.3	0.466
N	1340	1334	1335	1352	1352

Notes. Both panels shows estimates of equation 6. Panel A estimates each post-event change separately, while Panel B is the ATT for all post-event coefficients. Standard errors clustered at the program level. $*p < 0.05, **p < 0.01, ***p < 0.001$

Table 3: Main Results, Demographics

	(1)	(2)	(3)	(4)	(5)	(6)
	Male	White	Black	Latino	Asian	Mean Age
A. Event Study						
Event Year +1	0.00782	0.00144	-0.00578	-0.00453	0.0107	-2.17
	(0.0112)	(0.0164)	(0.00709)	(0.0141)	(0.0093)	(2.12)
Event Year +2	0.00986	0.00726	-0.00608	-0.0181	0.0179	-2.71
	(0.0107)	(0.0163)	(0.00682)	(0.0147)	(0.0101)	(1.81)
Event Year +3	0.00415	0.0188	-0.00988	0.00046	-0.00304	-2.3
	(0.0133)	(0.0184)	(0.00704)	(0.019)	(0.0113)	(2)
Event Year +4	0.0143	0.00018	-0.00781	-0.0139	0.0119	-1.11
	(0.0165)	(0.0202)	(0.00878)	(0.0226)	(0.0167)	(2.71)
	.	.	.	.	.	.
Y-mean	0.172	0.429	0.0917	0.239	0.108	34.3
N	1352	1352	1352	1352	1352	1352
B. Differences in Differences						
Post Change	0.00606	0.00161	-0.00505	-0.00453	0.00684	-2.05
	(0.00983)	(0.0138)	(0.00568)	(0.0123)	(0.00757)	(1.76)
	.	.	.	.	.	.
Y-mean	0.172	0.429	0.0917	0.239	0.108	34.3
N	1352	1352	1352	1352	1352	1352

Notes. Both panels shows estimates of equation 6. Panel A estimates each post-event change separately, while Panel B is the ATT for all post-event coefficients. Standard errors clustered at the program level. $*p < 0.05, **p < 0.01, ***p < 0.001$

Table 4: Main Results, Outcomes

	(1) Wait Time	(2) First-Year GPA	(3) Any Degree	(4) Finish ADN	(5) Pass NCLEX
A. Event Study					
Event Year +1	-0.215	0.0366	-0.0223	-0.0304	-0.0379*
	(0.167)	(0.0481)	(0.0192)	(0.0251)	(0.017)
Event Year +2	-0.452	0.0345	-0.0358	-0.0498	-0.0203
	(0.261)	(0.0551)	(0.024)	(0.0256)	(0.0127)
Event Year +3	-0.721*	0.0592	0.000314	-0.015	-0.0317
	(0.28)	(0.0561)	(0.0323)	(0.0426)	(0.0175)
Event Year +4	-0.603*	0.068	0.011	0.00312	-0.0138
	(0.287)	(0.0683)	(0.0335)	(0.0468)	(0.0153)
	.	.	.	.	.
Y-mean	5.95	3.05	0.608	0.424	0.879
N	1352	1344	1352	1352	964
B. Differences in Differences					
Post Change	-0.382*	0.0458	-0.0123	-0.0226	-0.0209
	(0.191)	(0.045)	(0.0201)	(0.0257)	(0.0126)
	.	.	.	.	.
Y-mean	5.95	3.05	0.608	0.424	0.879
N	1352	1344	1352	1352	964

Notes. Both panels shows estimates of equation 6. Panel A estimates each post-event change separately, while Panel B is the ATT for all post-event coefficients. Standard errors clustered at the program level. $*p < 0.05, **p < 0.01, ***p < 0.001$

Table 5: Student Background Characteristics and Eventual Degree Attainment, pre-2008 Cohorts

	(1)	(2)
	Complete ADN Program	Complete Any Degree
Male	-0.0430***	-0.0398***
	(0.00839)	(0.00515)
White	0.0548***	0.0382***
	(0.00851)	(0.00625)
Black	-0.00375	0.0251**
	(0.00906)	(0.00841)
Hispanic	0.0149*	0.0240***
	(0.00626)	(0.00663)
Age	-0.00320***	-0.00160***
	(0.000662)	(0.000454)
GPA	0.0912***	0.165***
	(0.0111)	(0.00784)
GPA in Math	0.00829**	-0.00315
	(0.00277)	(0.00260)
GPA in Biology	0.0689***	0.0290***
	(0.00722)	(0.00524)
Units Taken	0.00109***	0.00436***
	(0.000157)	(0.000218)
Basic Skills Courses	-0.0413***	-0.0333***
	(0.00622)	(0.00441)
Time to Enrollment	0.00522***	0.00251*
	(0.00114)	(0.00107)
N	62423	62423
Y-Mean	0.401	0.643
R-Squared	0.316	0.261
Cohort FE	X	X
College FE	X	X
College Trends	X	X

Notes. Table shows results of OLS regressions of student characteristics on completion of an ADN or of any community college program. Regressions also control for the square of age. Standard errors clustered at the program level.
$*p < 0.05, **p < 0.01, ***p < 0.001$

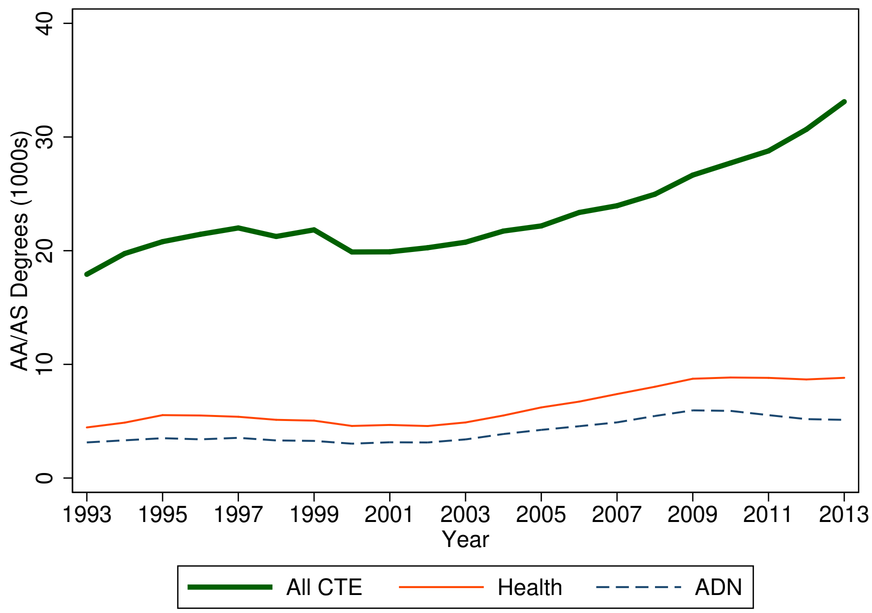
Table 6: Effect of Switches at Other Colleges on Non-Adopting Colleges

	(1) Male	(2) White	(3) Latino	(4) Black	(5) Mean Age	(6) GPA	(7) Bio GPA
A. In Same County							
Post Change	0.0172	0.0272	-0.0261	0.00701	1.255	-0.00754	0.00401
	(0.0152)	(0.0175)	(0.0180)	(0.0118)	(1.929)	(0.0386)	(0.0445)
Y-mean	0.213	0.263	0.322	0.116	33.025	2.913	2.867
N	115515	115722	115722	115722	115521	104233	66971
B. Within 25 Miles							
Post Change	0.0203	0.0295	-0.0251	0.00938	-1.054	-0.0396	-0.0193
	(0.0146)	(0.0163)	(0.0172)	(0.0111)	(2.080)	(0.0384)	(0.0420)
Y-mean	0.208	0.243	0.285	0.120	32.380	2.940	2.889
N	115515	115722	115722	115722	115521	104233	66971
C. Within 100 Miles							
Post Change	-0.00362	0.0222	-0.0118	0.00701	0.144	-0.0386	-0.0171
	(0.0126)	(0.0158)	(0.0118)	(0.00943)	(1.306)	(0.0317)	(0.0356)
Y-mean	0.182	0.351	0.256	0.117	31.456	2.857	2.784
N	115515	115722	115722	115722	115521	104233	66971

Notes. Panels shows estimates from regressions estimating 16 years prior and four year post the earliest year that other colleges in the specified geographic area changed admissions policy. Regressions control for calendar year, program, and program-specific linear time trends. Standard errors clustered at the program level. $*p < 0.05, **p < 0.01, ***p < 0.001$

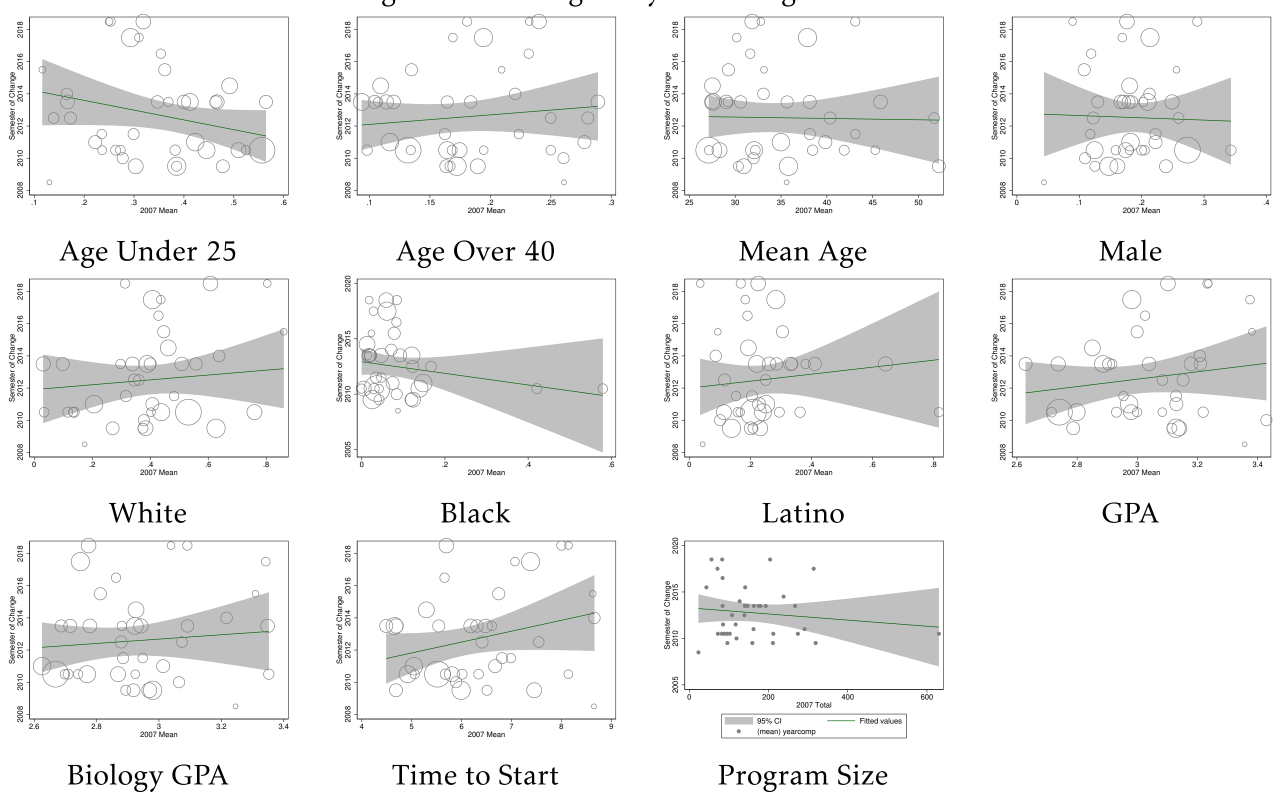
A1 Appendix Tables and Figures

Figure A1: CTE, Health, and ADN Completions Since 1993



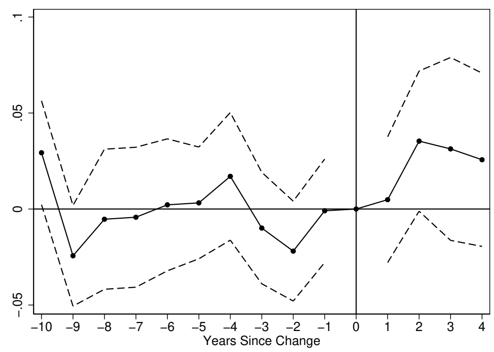
Notes. This figure shows the total number of associate degrees in career-technical education (CTE) programs since the 1992-1993 academic year; the number of associate degrees in all health programs; and the number of associate degrees in nursing (ADN).

Figure A2: Endogeneity of Timing

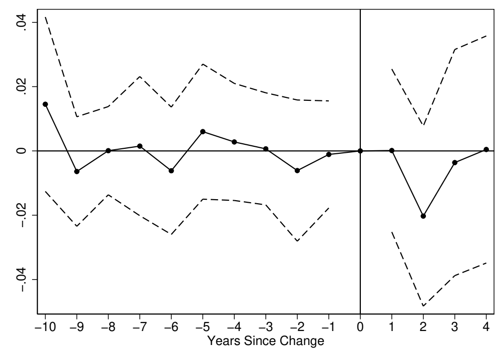


Notes. These figures show scatterplots of mean characteristics for each program in 2007 (horizontal axis) and year of implementing evaluative admission (vertical axis). The size of each bubble is proportional to the number of students in the 2007 cohorts. The figures also show a best fit line, weighted by the number of students, and associate 95 percent confidence interval.

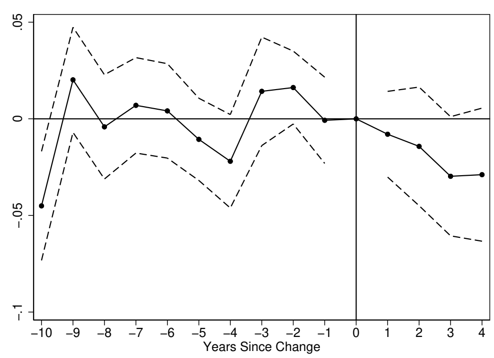
Figure A3: Main Results, Age Categories



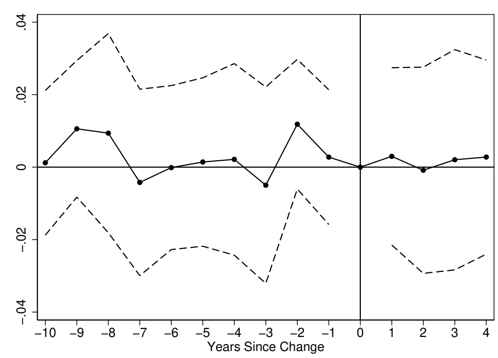
a) Under 25



b) 25-30



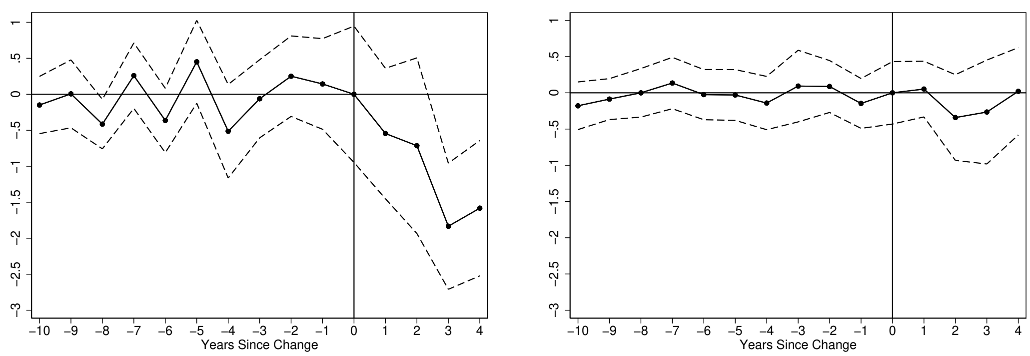
c) 30-40



d) 40 and over

Notes. Figures show point estimates and 95 percent confidence intervals of results from regressions of equation 6. Standard errors clustered at the program level.

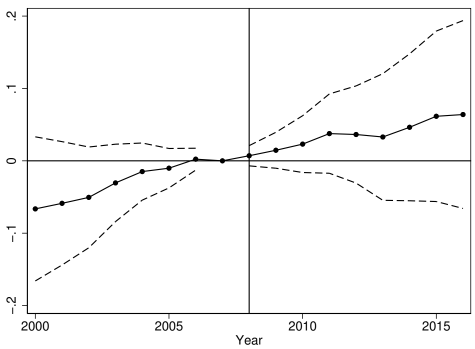
Figure A4: Wait Time, by Previous Admission Type



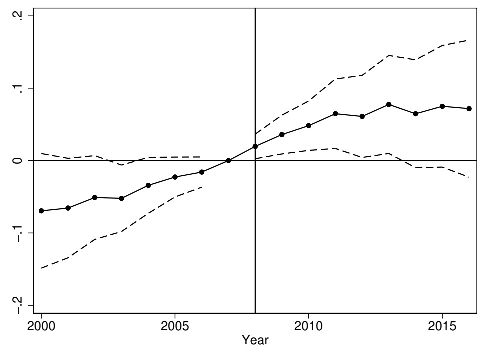
a) Waitlist b) Lottery and FCFS

Notes. Figures show point estimates and 95 percent confidence intervals of results from regressions of equation 6. Panel a) consists of programs that had waitlists prior to changing their admissions, while Panel b) consists of programs that had various types of lotteries and first-come-first-served regimes. Standard errors clustered at the program level.

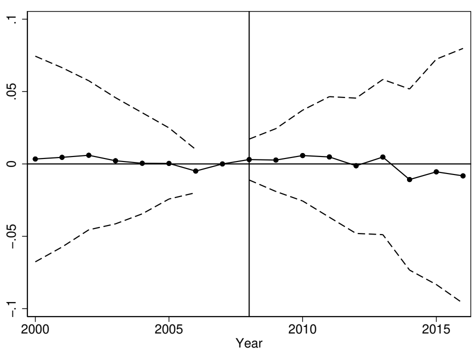
Figure A5: Cohort Characteristics at Colleges with Non-Evaluative Admissions



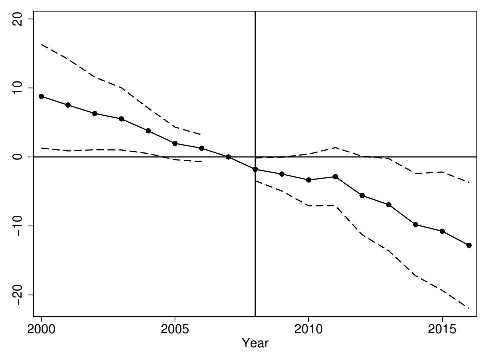
a) Male



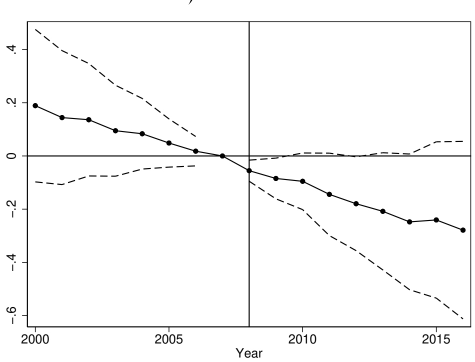
b) White



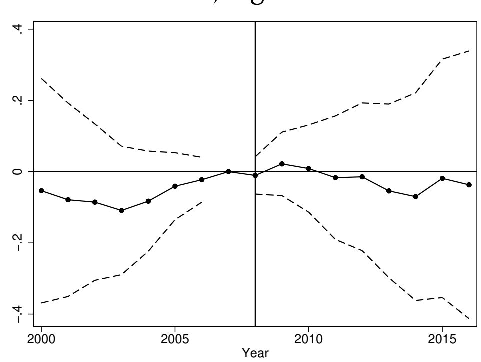
c) Latino



d) Age



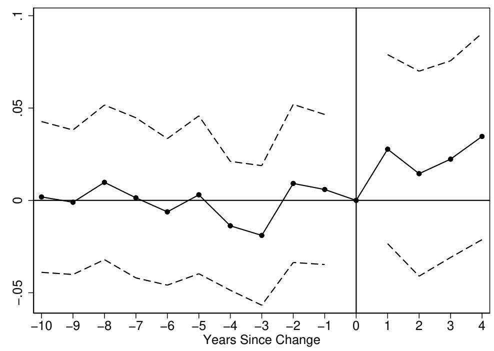
e) GPA



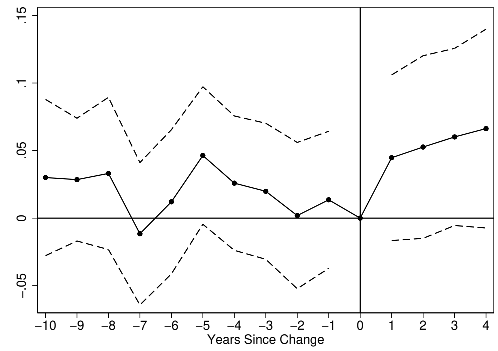
f) Bio GPA

Notes. Figures show point estimates and 95 percent confidence intervals of results from regressions estimating 16 years prior and four year post change of admissions policy. Regressions control for calendar year, program, and program-specific linear time trends. Standard errors clustered at the program level.

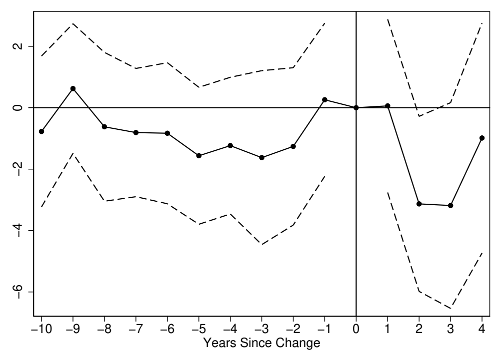
Figure A6: Results Using Two Way Fixed Effects, Academic Background



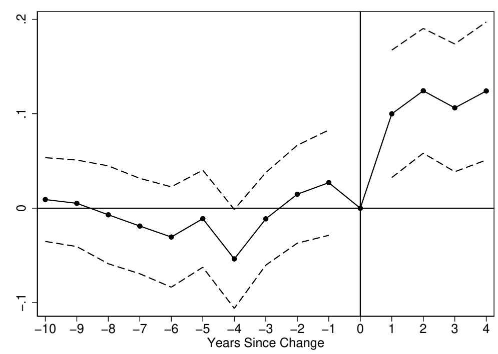
a) GPA



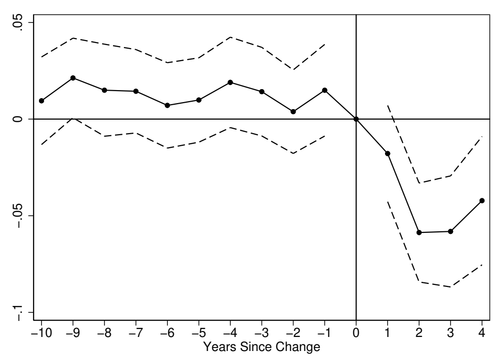
b)Math GPA



c) Units Attempted



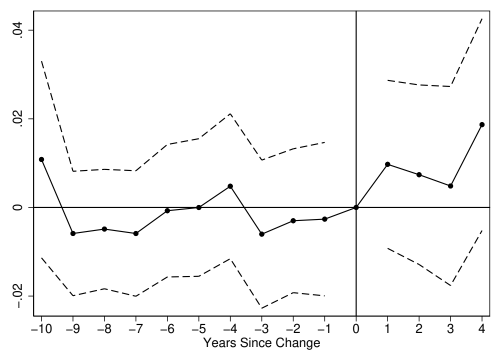
d) Biology GPA



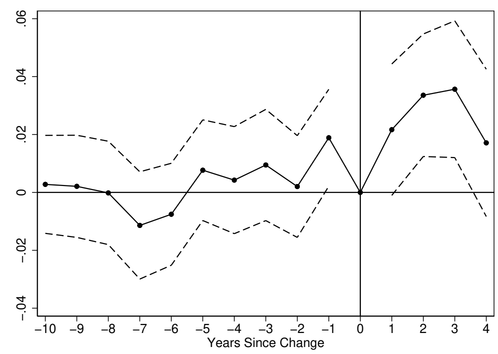
e) Basic Skills Courses

Notes. Figures show point estimates and 95 percent confidence intervals of results from regressions estimating 16 years prior and four year post change of admissions policy. Regressions control for calendar year, program, and program-specific linear time trends. Standard errors clustered at the program level.

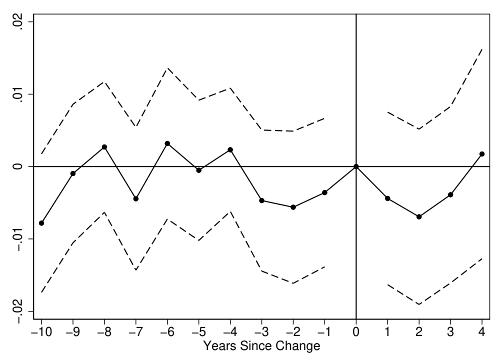
Figure A7: Results Using Two Way Fixed Effects, Demographics



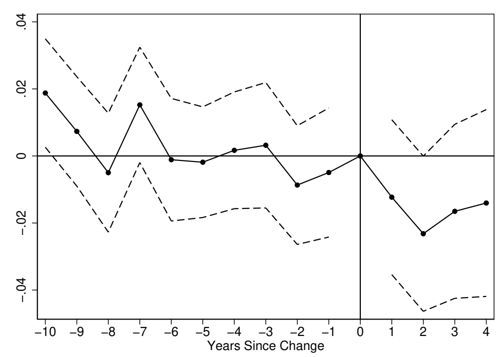
a) Male



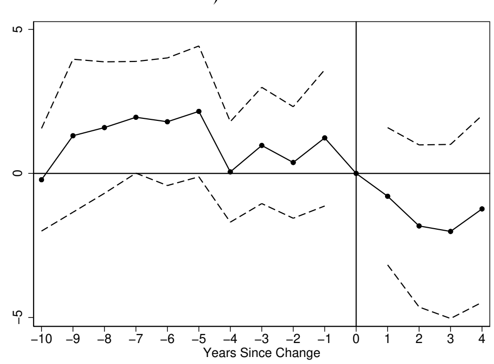
b)White



c) Black



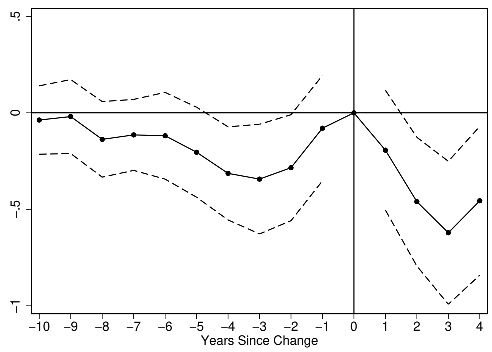
d) Latino



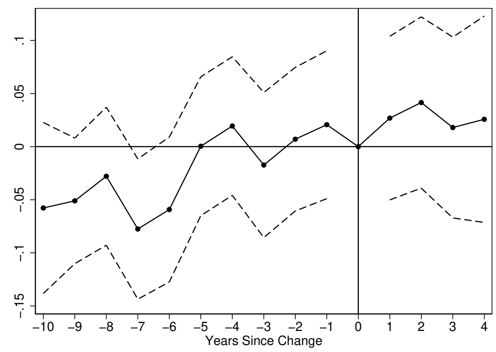
e) Age

Notes. Figures show point estimates and 95 percent confidence intervals of results from regressions estimating 16 years prior and four year post change of admissions policy. Regressions control for calendar year, program, and program-specific linear time trends. Standard errors clustered at the program level.

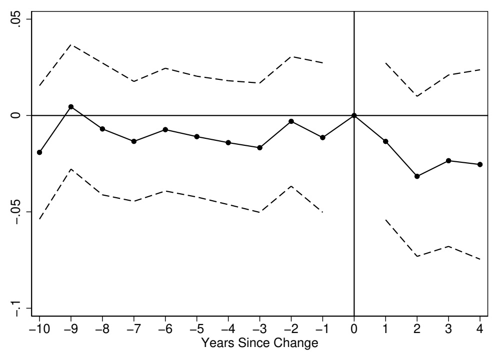
Figure A8: Results Using Two Way Fixed Effects, Outcomes



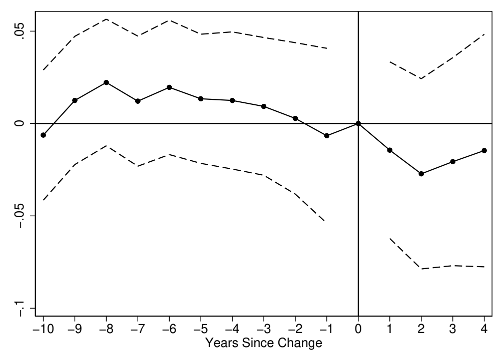
a) Time to Start



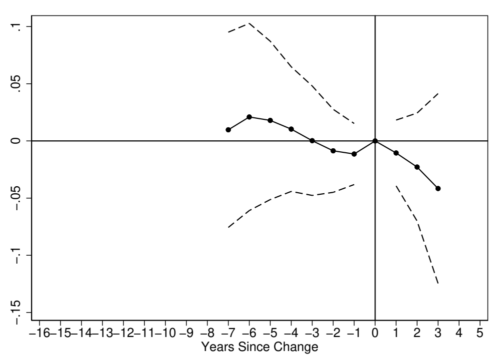
b) First-Year GPA



c) Any Degree



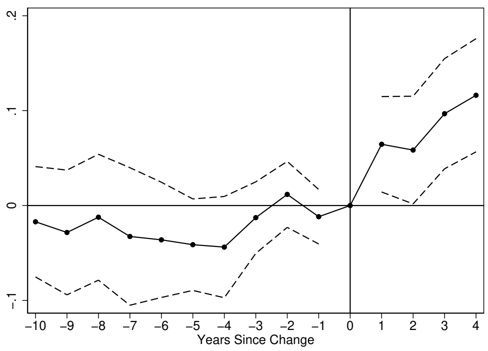
d) Complete ADN



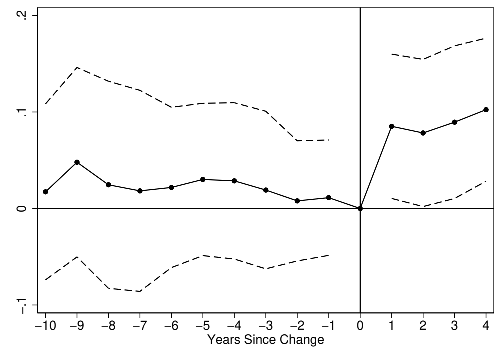
e) NCLEX-RN Pass Rates

Notes. Figures show point estimates and 95 percent confidence intervals of results from regressions estimating 16 years prior and four year post change of admissions policy. Regressions control for calendar year, program, and program-specific linear time trends. Standard errors clustered at the program level.

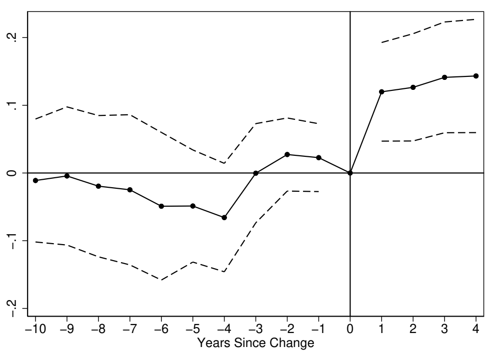
Figure A9: Results Using “Stacked” Approach, Academic Background



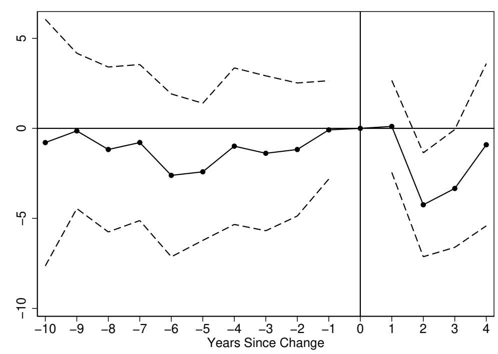
a) GPA



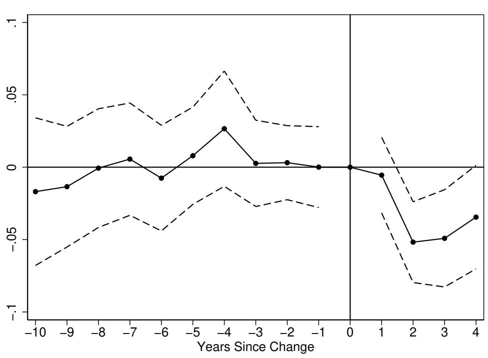
b)Math GPA



c) Biology GPA



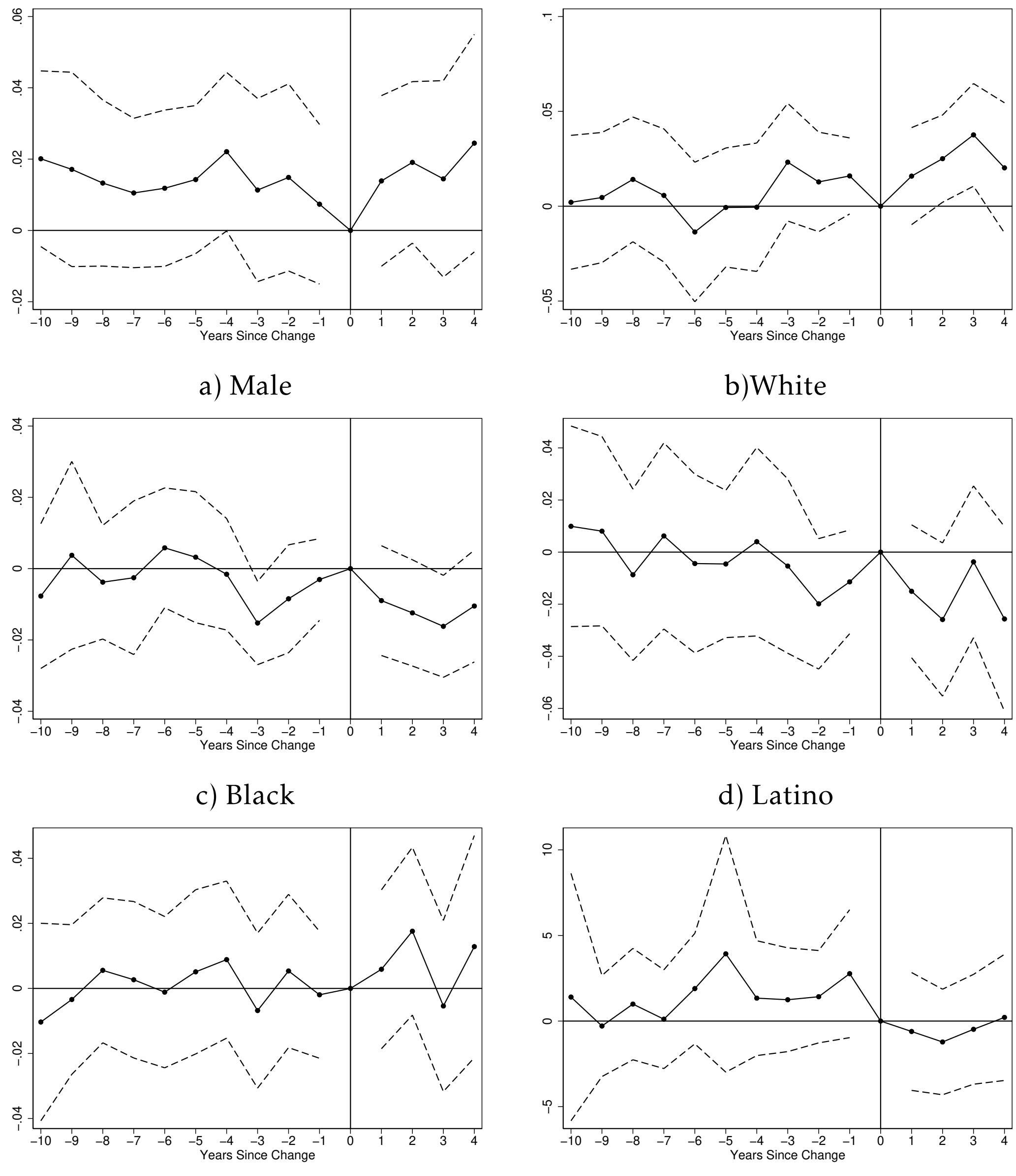
d) Units Attempted



e) Basic Skills Courses

Notes. Figures show point estimates and 95 percent confidence intervals of results from regressions estimating equation 7. Regressions control for calendar year-event and program-event interactions. Standard errors clustered at the program level.

Figure A10: Results Using "Stacked" Approach, Demographics



a) Male

b)White

c) Black

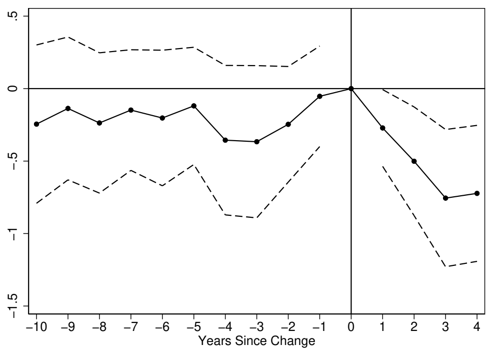
d) Latino

e) Asian

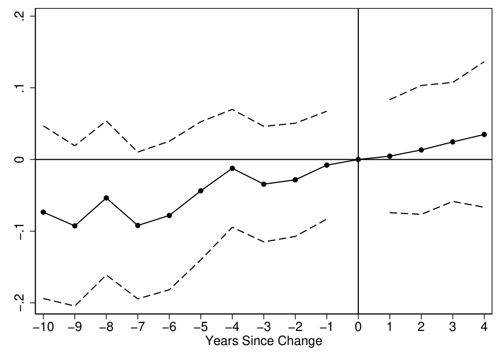
f) Age

Notes. Figures show point estimates and 95 percent confidence intervals of results from regressions estimating equation 7. Regressions control for calendar year-event and program-event interactions. Standard errors clustered at the program level.

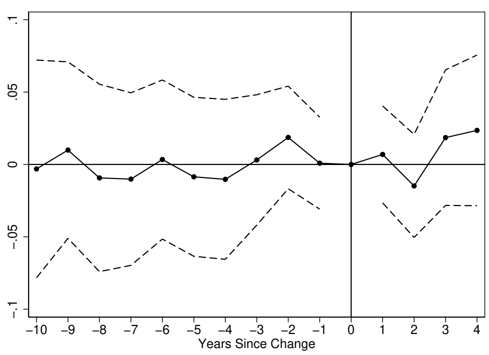
Figure A11: Results Using “Stacked” Approach, Outcomes



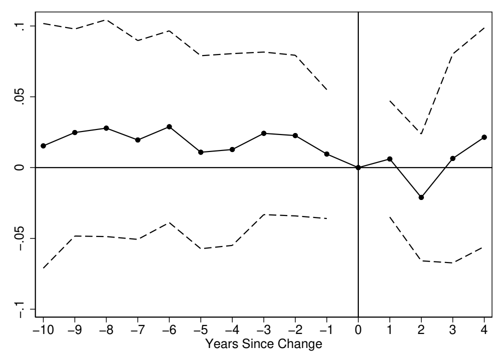
a) Time to Start



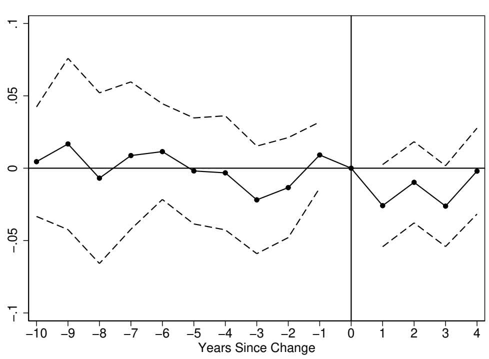
b) First-Year GPA



c) Any Degree



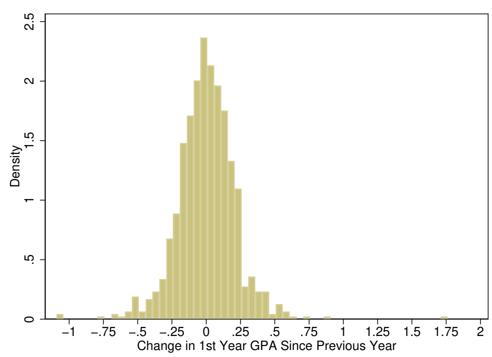
d) Complete ADN



e) NCLEX-RN Pass Rates

Notes. Figures show point estimates and 95 percent confidence intervals of results from regressions estimating equation 7. Regressions control for calendar year-event and program-event interactions. Standard errors clustered at the program level.

Figure A12: Distribution of Year-to-Year Changes in First Year Nursing GPA, pre-2007



Notes. Figure shows distribution of college-level differences from one year to the next in mean GPA of first year nursing courses. Data are included from the 1993 academic year through the 2007 academic year, the year prior to the first change in admissions.

Table A1: Test for Endogeneity of Timing

	(1) Unweighted	(2) Weighted
Male	0.250	-0.459
	(1.880)	(1.742)
White	0.545	0.811
	(0.888)	(0.905)
Black	-0.648	2.091
	(2.336)	(2.332)
Hispanic	-0.868	-1.402
	(1.001)	(1.056)
Asian	-0.648	0.353
	(0.895)	(0.662)
Other Race	1.424	-0.126
	(1.610)	(1.438)
Age	-0.00359***	-0.00348***
	(0.000551)	(0.000590)
GPA	1.268*	1.381*
	(0.468)	(0.528)
GPA in Math	0.447	0.590
	(0.355)	(0.371)
GPA in Biology	0.488	-0.398
	(0.958)	(0.924)
Units Attempted	-0.00864	-0.0122
	(0.0128)	(0.0143)
Basic Skills Courses	0.225	0.759
	(1.038)	(1.028)
Time to Enrollment	-0.0213	0.0344
	(0.140)	(0.170)
Any Degree, 3 Years	-0.171	-0.369
	(0.993)	(1.093)
Any Nursing Degree, 3 Years	0.355	0.297
	(0.931)	(0.946)
Unemployment Rate	-2.679	5.635
	(15.73)	(22.56)
UI Benefits (log)	-0.978*	-0.689
	(0.376)	(0.504)

Notes. Each cell shows results from a regression of year of adoption of evaluative measures on college mean characteristics in 2007, the year prior to the policy change. Column 1 does not weight, while Column 2 weights by the size of the incoming cohort. Standard errors clustered at the program level. $*p < 0.05, **p < 0.01, ***p < 0.001$

Table A2: Main Results, Age Detail

	(1) Under 25	(2) 25-29	(3) 30-39	(4) Over 40
A. Event Study				
Event Year +1	0.00487	0.000127	-0.00797	0.00297
	(0.0167)	(0.0129)	(0.0113)	(0.0125)
Event Year +2	0.0354	-0.0203	-0.0143	-0.000856
	(0.0186)	(0.0143)	(0.0157)	(0.0145)
Event Year +3	0.0313	-0.00362	-0.0297	0.00204
	(0.0243)	(0.0179)	(0.0157)	(0.0155)
Event Year +4	0.0257	0.000444	-0.0289	0.00278
	(0.023)	(0.018)	(0.0176)	(0.0137)
	.	.	.	.
Y-mean	0.328	0.222	0.269	0.181
N	1352	1352	1352	1352
B. Differences in Differences				
Post Change	0.0196	-0.00572	-0.0166	0.00271
	(0.0161)	(0.0127)	(0.0102)	(0.0109)
	.	.	.	.
Y-mean	0.328	0.222	0.269	0.181
N	1352	1352	1352	1352
Y-Mean	0.375	0.223	0.246	0.157
N	254002	254002	254002	254002

Notes. Both panels shows estimates of equation 6. Panel A estimates each post-event change separately, while Panel B is the ATT for all post-event coefficients. Standard errors clustered at the program level. $*p < 0.05, **p < 0.01, ***p < 0.001$

Table A3: Student Background Characteristics and Eventual Degree Attainment, pre-2008 Cohorts

	(1)	(2)	(3)	(4)	(5)	(6)
	Complete ADN Program			Complete Any Degree		
Male	-0.0558***	-0.0438***	-0.0430***	-0.0469***	-0.0403***	-0.0398***
	(0.00965)	(0.00841)	(0.00839)	(0.00659)	(0.00511)	(0.00515)
White	0.0918***	0.0560***	0.0548***	0.0652***	0.0390***	0.0382***
	(0.0180)	(0.00834)	(0.00851)	(0.0117)	(0.00607)	(0.00625)
Black	-0.0226	-0.00488	-0.00375	-0.00824	0.0239**	0.0251**
	(0.0241)	(0.00970)	(0.00906)	(0.0136)	(0.00852)	(0.00841)
Hispanic	0.0477**	0.0147*	0.0149*	0.0372**	0.0232***	0.0240***
	(0.0152)	(0.00621)	(0.00626)	(0.0111)	(0.00670)	(0.00663)
Age	-0.000365	-0.00308***	-0.00320***	0.0000240	-0.00157***	-0.00160***
	(0.000890)	(0.000657)	(0.000662)	(0.000606)	(0.000453)	(0.000454)
GPA	0.0966***	0.0909***	0.0912***	0.163***	0.167***	0.165***
	(0.0157)	(0.0116)	(0.0111)	(0.0118)	(0.00804)	(0.00784)
GPA in Math	0.0209***	0.00990**	0.00829**	0.00491	-0.00290	-0.00315
	(0.00558)	(0.00308)	(0.00277)	(0.00464)	(0.00264)	(0.00260)
GPA in Biology	0.0847***	0.0700***	0.0689***	0.0389***	0.0287***	0.0290***
	(0.00916)	(0.00771)	(0.00722)	(0.00639)	(0.00543)	(0.00524)
Units Taken	0.00267***	0.00121***	0.00109***	0.00528***	0.00442***	0.00436***
	(0.000251)	(0.000170)	(0.000157)	(0.000244)	(0.000221)	(0.000218)
Basic Skills Courses	-0.0470**	-0.0409***	-0.0413***	-0.0354**	-0.0309***	-0.0333***
	(0.0155)	(0.00610)	(0.00622)	(0.0107)	(0.00437)	(0.00441)
Time to Enrollment	0.00624**	0.00545***	0.00522***	0.00272	0.00265*	0.00251*
	(0.00185)	(0.00121)	(0.00114)	(0.00137)	(0.00107)	(0.00107)
N	62423	62423	62423	62423	62423	62423
Y-Mean	0.401	0.401	0.401	0.643	0.643	0.643
R-Squared	0.126	0.295	0.316	0.183	0.254	0.261
Cohort FE	X	X	X	X	X	X
College FE		X	X		X	X
College Trends			X			X

Notes. Table shows results of OLS regressions of student characteristics on completion of an ADN or of any community college program. Regressions also control for the square of age. Standard errors clustered at the program level.
$*p < 0.05, **p < 0.01, ***p < 0.001$

Table A4: Main Results, Academic Background, by Previous Admissions Type

	(1) GPA	(2) Math GPA	(3) Bio GPA	(4) Units	(5) Basic Skills
A. Waitlist					
Post Change	0.0799	0.0366	0.0366	-5.29	-0.0264
	(0.0449)	(0.0581)	(0.0581)	(3.88)	(0.0303)
	.	.	.	.	.
Y-mean	3.03	2.79	2.87	58.7	0.436
N	386	383	382	391	391
B. Lottery and FCFS					
Post Change	0.06*	0.0573	0.0573	-0.672	-0.0306
	(0.0268)	(0.0317)	(0.0317)	(1.73)	(0.0201)
	.	.	.	.	.
Y-mean	3.03	2.83	2.92	59.5	0.479
N	954	951	953	961	961

Notes. All panels shows estimates from regressions estimating the ATT according to equation 5. Panel A consists of programs that had waitlists prior to changing their admissions, while Panel B consists of programs that had various types of lotteries and first-come-first-served regimes. Standard errors clustered at the program level.
$*p < 0.05, **p < 0.01, ***p < 0.001$

Table A5: Main Results, Demographics, by Previous Admissions Type

	(1) Male	(2) White	(3) Black	(4) Latino	(5) Asian	(6) Mean Age
A. Waitlist						
Post Change	0.00101	0.000584	0.0018	0.0271	-0.0206	-3.99
	(0.0175)	(0.0286)	(0.0102)	(0.0213)	(0.0163)	(3.98)
	.	.	.	.	.	.
Y-mean	0.172	0.464	0.0869	0.203	0.101	35.8
N	391	391	391	391	391	391
B. Lottery and FCFS						
Post Change	0.00605	0.00141	-0.00741	-0.0184	0.00933	0.292
	(0.0128)	(0.0183)	(0.00799)	(0.0165)	(0.0102)	(2)
	.	.	.	.	.	.
Y-mean	0.173	0.415	0.0936	0.253	0.11	33.7
N	961	961	961	961	961	961

Notes. All panels shows estimates from regressions estimating the ATT according to equation 5. Panel A consists of programs that had waitlists prior to changing their admissions, while Panel B consists of programs that had various types of lotteries and first-come-first-served regimes. Standard errors clustered at the program level.
$*p < 0.05, **p < 0.01, ***p < 0.001$

Table A6: Main Results, Outcomes, by Previous Admissions Type

	(1) Wait Time	(2) First-Year GPA	(3) Any Degree	(4) Finish ADN	(5) Pass NCLEX
A. Waitlist					
Post Change	-0.959**	0.0668	-0.0559	-0.0875	-0.0122
	(0.351)	(0.0822)	(0.0448)	(0.0448)	(0.0231)
	.	.	.	.	.
Y-mean	6.36	3.05	0.622	0.463	0.875
N	391	385	391	391	257
B. Lottery and FCFS					
Post Change	-0.0683	0.0386	0.00388	-0.00478	-0.0216
	(0.222)	(0.0553)	(0.0227)	(0.0317)	(0.0155)
	.	.	.	.	.
Y-mean	5.78	3.05	0.603	0.409	0.881
N	961	959	961	961	693

Notes. All panels shows estimates from regressions estimating the ATT according to equation 5. Panel A consists of programs that had waitlists prior to changing their admissions, while Panel B consists of programs that had various types of lotteries and first-come-first-served regimes. Standard errors clustered at the program level.
$*p < 0.05, **p < 0.01, ***p < 0.001$

Table A7: Results using Two-Way Fixed Effects, Academic Background

	(1) GPA	(2) Math GPA	(3) Bio GPA	(4) Units	(5) Basic Skills
A. Event Study					
Event Year +1	0.0250	0.0356	0.100**	-0.210	-0.0218
	(0.0258)	(0.0304)	(0.0336)	(1.398)	(0.0125)
Event Year +2	0.0123	0.0453	0.127***	-3.398*	-0.0620***
	(0.0279)	(0.0335)	(0.0330)	(1.414)	(0.0129)
Event Year +3	0.0215	0.0549	0.113***	-3.503*	-0.0610***
	(0.0269)	(0.0325)	(0.0339)	(1.670)	(0.0144)
Event Year +4	0.0361	0.0640	0.136***	-1.276	-0.0446**
	(0.0281)	(0.0365)	(0.0363)	(1.864)	(0.0166)
F-test: pre-years	0.485	0.545	1.643	0.779	0.440
p-value: pre-years	0.901	0.859	0.088	0.649	0.927
F-test: post-years	0.489	0.886	4.523	2.502	7.285
p-value: post-years	0.744	0.471	0.001	0.040	0.000
B. Differences in Differences					
Post Change	0.0231	0.0424	0.115***	-1.916	-0.0430***
	(0.0219)	(0.0270)	(0.0284)	(1.215)	(0.0110)
Y-Mean	3.006	2.762	2.958	63.290	0.490
N	232372	143125	158185	254335	254335

Notes. Both panels shows estimates from regressions estimating 16 years prior and four year post change of admissions policy. Regressions control for calendar year, program, and program-specific linear time trends. Panel A estimates each post-event change separately, while Panel B constrains all post-event coefficients to be equivalent. The F-tests are a test that all the pre-event or post-event coefficients are jointly zero. Standard errors clustered at the program level.
$*p < 0.05, **p < 0.01, ***p < 0.001$

Table A8: Results using Two-Way Fixed Effects, Demographics

	(1) Male	(2) White	(3) Black	(4) Latino	(5) Mean Age
A. Event Study					
Event Year +1	0.00675	0.0187	-0.00508	-0.0126	-0.511
	(0.00941)	(0.0107)	(0.00573)	(0.0114)	(1.212)
Event Year +2	0.00808	0.0269*	-0.00652	-0.0208	-1.558
	(0.0101)	(0.0106)	(0.00564)	(0.0114)	(1.428)
Event Year +3	0.00435	0.0351**	-0.00449	-0.0145	-1.784
	(0.0110)	(0.0119)	(0.00610)	(0.0128)	(1.526)
Event Year +4	0.0240*	0.0141	-0.00163	-0.00543	-1.028
	(0.0117)	(0.0122)	(0.00664)	(0.0147)	(1.627)
F-test: pre-years	0.694	1.057	0.859	1.546	1.508
p-value: pre-years	0.731	0.392	0.571	0.117	0.130
F-test: post-years	1.279	2.833	0.482	1.005	0.485
p-value: post-years	0.276	0.023	0.749	0.404	0.747
B. Differences in Differences					
Post Change	0.00883	0.0234**	-0.00520	-0.0149	-1.029
	(0.00836)	(0.00863)	(0.00484)	(0.00971)	(1.110)
Y-Mean	0.199	0.362	0.073	0.249	33.677
N	254335	254335	254335	254335	254002

Notes. Both panels shows estimates from regressions estimating 16 years prior and four year post change of admissions policy. Regressions control for calendar year, program, and program-specific linear time trends. Panel A estimates each post-event change separately, while Panel B constrains all post-event coefficients to be equivalent. The F-tests are a test that all the pre-event or post-event coefficients are jointly zero. Standard errors clustered at the program level.
$*p < 0.05, **p < 0.01, ***p < 0.001$

Table A9: Results using Two-Way Fixed Effects, Outcomes

	(1) Wait Time	(2) First-Year GPA	(3) Any Degree	(4) Finish ADN
A. Event Study				
Event Year +1	-0.186	0.0284	-0.0124	-0.0176
	(0.152)	(0.0381)	(0.0206)	(0.0241)
Event Year +2	-0.453**	0.0423	-0.0301	-0.0306
	(0.164)	(0.0399)	(0.0209)	(0.0259)
Event Year +3	-0.617***	0.0173	-0.0217	-0.0246
	(0.183)	(0.0421)	(0.0224)	(0.0283)
Event Year +4	-0.445*	0.0235	-0.0214	-0.0179
	(0.190)	(0.0483)	(0.0248)	(0.0315)
F-test: pre-years	1.339	1.472	0.418	0.403
p-value: pre-years	0.203	0.143	0.939	0.946
F-test: post-years	3.444	0.335	0.537	0.380
p-value: post-years	0.008	0.855	0.708	0.823
B. Differences in Differences				
Post Change	-0.370**	0.0295	-0.0204	-0.0228
	(0.133)	(0.0337)	(0.0177)	(0.0214)
Y-Mean	7.015	3.055	0.547	0.351
N	254002	183400	254335	254335

Notes. Both panels shows estimates from regressions estimating 16 years prior and four year post change of admissions policy. Regressions control for calendar year, program, and program-specific linear time trends. Panel A estimates each post-event change separately, while Panel B constrains all post-event coefficients to be equivalent. The F-tests are a test that all the pre-event or post-event coefficients are jointly zero. Standard errors clustered at the program level.
$*p < 0.05, **p < 0.01, ***p < 0.001$

Table A10: Results Using "Stacked" Approach, Academic Background

	(1) GPA	(2) Math GPA	(3) Bio GPA	(4) Units	(5) Basic Skills
A. Event Study					
Event Year +1	0.0646*	0.0852*	0.120**	0.107	-0.00550
	(0.0251)	(0.0374)	(0.0364)	(1.277)	(0.0131)
Event Year +2	0.0585*	0.0783*	0.127**	-4.245**	-0.0518***
	(0.0284)	(0.0381)	(0.0396)	(1.443)	(0.0140)
Event Year +3	0.0968***	0.0895*	0.141***	-3.338*	-0.0491**
	(0.0290)	(0.0395)	(0.0409)	(1.636)	(0.0168)
Event Year +4	0.116***	0.102**	0.143***	-0.910	-0.0345
	(0.0298)	(0.0371)	(0.0418)	(2.253)	(0.0179)
F-test: pre-years	1.428	0.330	0.988	0.571	0.975
p-value: pre-years	0.162	0.973	0.452	0.838	0.463
F-test: post-years	3.915	2.326	3.715	5.696	4.600
p-value: post-years	0.004	0.054	0.005	0.000	0.001
N	19467	19457	19455	19481	19481
B. Differences in Differences					
Post Change	0.0825***	0.0883**	0.132***	-2.094	-0.0347**
	(0.0229)	(0.0317)	(0.0344)	(1.243)	(0.0117)
N	19467	19457	19455	19481	19481
Y-mean	3.047	2.806	2.996	70.448	0.507

Notes. Both panels shows estimates from regressions estimating equation 7. Regressions control for calendar year-event and program-event interactions. Panel A estimates each post-event change separately, while Panel B constrains all post-event coefficients to be equivalent. Standard errors clustered at the program level.
$*p < 0.05, **p < 0.01, ***p < 0.001$

Table A11: Results Using "Stacked" Approach, Demographics

	(1) Male	(2) White	(3) Black	(4) Latino	(5) Asian	(6) Other Race	Mean Ag
A. Event Study							
Event Year +1	0.0139	0.0159	-0.00897	-0.0150	0.00589	0.00227	-0.608
	(0.0120)	(0.0128)	(0.00769)	(0.0128)	(0.0122)	(0.0169)	(1.721)
Event Year +2	0.0191	0.0251*	-0.0124	-0.0259	0.0176	-0.00436	-1.223
	(0.0113)	(0.0115)	(0.00745)	(0.0147)	(0.0129)	(0.0124)	(1.541)
Event Year +3	0.0145	0.0376**	-0.0162*	-0.00374	-0.00540	-0.0123	-0.482
	(0.0138)	(0.0135)	(0.00715)	(0.0146)	(0.0132)	(0.0138)	(1.604)
Event Year +4	0.0245	0.0202	-0.0105	-0.0257	0.0129	0.00307	0.214
	(0.0153)	(0.0171)	(0.00784)	(0.0177)	(0.0171)	(0.0168)	(1.842)
F-test: pre-years	0.610	0.982	1.846	1.347	1.112	1.992	0.736
p-value: pre-years	0.806	0.457	0.049	0.200	0.349	0.031	0.691
F-test: post-years	0.878	2.692	1.495	1.122	1.182	1.072	0.233
p-value: post-years	0.476	0.030	0.201	0.344	0.317	0.369	0.920
N	19481	19481	19481	19481	19481	19481	19481
B. Differences in Differences							
Post Change	0.0178	0.0245*	-0.0120	-0.0175	0.00777	-0.00281	-0.551
	(0.0108)	(0.00983)	(0.00667)	(0.0121)	(0.0108)	(0.0130)	(1.253)
N	19481	19481	19481	19481	19481	19481	19481
Y-mean	0.190	0.385	0.079	0.288	0.128	0.121	35.110

Notes. Both panels shows estimates from regressions estimating equation 7. Regressions control for calendar year-event and program-event interactions. Panel A estimates each post-event change separately, while Panel B constrains all post-event coefficients to be equivalent. Standard errors clustered at the program level. $*p < 0.05, **p < 0.01, ***p < 0.001$

Table A12: Results Using "Stacked" Approach, Outcomes

	(1) Wait Time	(2) First-Year GPA	(3) Any Degree	(4) Finish ADN	(5) Pass NCLEX
A. Event Study					
Event Year +1	-0.272*	0.00469	0.00693	0.00609	-0.0259
	(0.132)	(0.0394)	(0.0167)	(0.0205)	(0.0142)
Event Year +2	-0.501**	0.0134	-0.0148	-0.0211	-0.00975
	(0.187)	(0.0450)	(0.0178)	(0.0224)	(0.0140)
Event Year +3	-0.755**	0.0245	0.0186	0.00646	-0.0262
	(0.237)	(0.0415)	(0.0234)	(0.0369)	(0.0140)
Event Year +4	-0.723**	0.0349	0.0235	0.0214	-0.00201
	(0.235)	(0.0508)	(0.0260)	(0.0386)	(0.0148)
F-test: pre-years	0.659	1.153	0.677	0.401	0.956
p-value: pre-years	0.763	0.318	0.747	0.947	0.480
F-test: post-years	3.294	0.159	1.052	0.896	2.052
p-value: post-years	0.011	0.959	0.379	0.466	0.085
N	19481	19372	19481	19481	15931
B. Differences in Differences					
Post Change	-0.551***	0.0186	0.00787	0.00260	-0.0162
	(0.158)	(0.0357)	(0.0163)	(0.0222)	(0.0120)
N	19481	19372	19481	19481	15931
Y-mean	7.651	3.011	0.641	0.457	0.888

Notes. Both panels shows estimates from regressions estimating equation 7. Regressions control for calendar year-event and program-event interactions. Panel A estimates each post-event change separately, while Panel B constrains all post-event coefficients to be equivalent. Standard errors clustered at the program level. $*p < 0.05, **p < 0.01, ***p < 0.001$