Do the SAT and ACT Limit Enrollment? Evidence from the Test-Optional Movement

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Abstract

As of 2019, about 250 four-year colleges and universities had adopted a test-optional application procedure that allowed students to apply for admission without submitting an SAT or ACT score. Many schools adopted this procedure to encourage greater racial and socioeconomic diversity among admitted students. Unfortunately, we know little about the impact of test-optional policies. In this paper, I use a difference-in-differences design to examine the impact of this reform on schools that adopted the policies between 2005 and 2014. Compared to schools that did not switch, test-optional schools witnessed an 18.2 log point increase in the number of minority enrollments and a 10.2 log point increase in the number of Pell Grant students. I also show that test-optional policies affect financial aid disbursements. After switching, schools experienced an increase in the number of students receiving institutional grant aid, but decreases in the average aid granted. Schools offset the decrease in grant aid by increasing the availability of institutional loans. These results take on heightened importance as nearly 800 additional four-year college and universities adopted these policies in response to the COVID-19 pandemic.

Keywords: higher education, test-optional, college access

JEL classifications: I22, I23, I24

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I Introduction

There are persistent disparities in college enrollment rates across racial and income groups. As a result, a vast economic literature examines the policies that promote college matriculation (Page and Scott-Clayton, 2016). Changes to the admissions process, specifically the removal of standardized testing requirements, are a policy of interest in this context. About one-half of high school students do not take a college admissions exam (National Center for Education Statistics, 2020a, 2019). Further, there is less access to these exams at schools that serve families of low socioeconomic status. The emergence of schools that allow students to apply without reporting a college entrance exam score has removed an immediate barrier for the group of students considering 4-year colleges and universities. Understanding the impact of these policies has taken on heightened importance as nearly 800 additional four-year colleges and universities have at least temporarily adopted the program in response to the COVID-19 pandemic.

It is unclear to what extent test-optional policies can help close existing gaps. Opponents argue that these policies could exacerbate differences in college enrollment[1]. Previous studies have shown that standardized tests help students signal their ability (Goodman, 2016; Hyman, 2017; Card and Giuliano, 2016) and there are concerns whether their removal could increase the reliance on subjective admissions criteria that favor affluent students (Wai et al., 2019; Gershenson, 2018; Snellman et al., 2015). However, others have documented that inequity in access to college entrance exams (Bulman, 2015; Buchmann et al., 2010) and their strong correlation with socioeconomic status (Bates News, 2005; Geiser and Santelices, 2007; Hiss and Franks, 2014) may provide two reasons test-optional policies could promote greater representation of low-income and minority students on college campuses. In this paper, I address this question by leveraging the differential timing of adoption of these policies to evaluate subsequent changes in enrollment and financial aid.

I construct a detailed panel dataset on selective, Title IV-eligible 4-year colleges and universities from the Integrated Postsecondary Education Data System (IPEDS) to get information on enrollment, financial aid, and graduation outcomes. I link this data to information on cohorts' performance in high school from the College Board's Annual Survey of Colleges and Pell Grant receipts from the Department of Education. I focus the analysis on the set of test optional policies adopted from 2005 to 2014 and employ a recent dynamic difference-in-differences model that avoids common biases in staggered two-way fixed-effects designs (Callaway and Sant'Anna, 2021).

I find that on average schools that switch to a test-optional policy saw an increase of 18.2 log points in their enrollment of first-time, full-time Black, Native American, and Hispanic (BNH)

[1] Moreover, opponents criticize test-optional policies for artificially increasing an institution's position by increasing the number of applications while keeping admissions the same (Epstein, 2009; Belasco et al., 2015).

students and an increase of 10.2 log points in the number of undergraduate students receiving a Pell Grant when compared to the set of control schools. However, changes in enrollment varied within racial groups. First-time, full-time (FTFT) Black and Hispanic Women witnessed the largest gains in enrollment with increases of 22.9 log points and 24.2 log points, respectively. There is some suggestive evidence that adopting schools expanded overall enrollment. However, the growth in total enrollment cannot fully explain the aforementioned results as the overall share of FTFT students identified as BNH increased by 1.5 percentage points (p.p.), and the share of undergraduates receiving a Pell Grant increased by 1.4 p.p.

I further explore the effects of test-optional policies by evaluating changes in student quality and performance. Schools that adopted a test-optional policy witnessed a 1.5 p.p. decrease in the percent of enrolled first-year students finishing in the top half of their high school class and a statistically insignificant decrease of 0.43 p.p. in the percent of enrolled students first-year students with a final high school grade point average above 3.0. However, these declines in college preparation measures did not translate into meaningful changes in college performance. Retention rates fell by 0.19 p.p, and 6-year graduation rates fell by 0.3 p.p.. When compared to the baseline mean (75.28 and 54.94, respectively), these results are small. Furthermore, they are statistically insignificant in all of my specifications and I can rule out any effect size larger than a 1 p.p decline in retention rates and any size larger than 1.3 p.p decline in the 6-year graduation rate.

Finally, I examine how the adoption of test-optional policies impacted financial aid disbursement. Financial aid packages not only influence college enrollment and completion for students (Bettinger et al., 2012; Dynarski et al., 2021), but can impact schools' decisions as well (Long, 2004; Turner, 2017). Schools that implemented a test-optional policy increased the number of FTFT students receiving any institutional grant aid by 7.4 log points, yet the average grant aid allotted to each student declined by 1,130.63 dollars. These results suggest that schools have had to respond to the change in financial need of their enrolled cohorts.[2] Interestingly, students somewhat offset this decrease in aid by taking out institutional loans. Adopting schools saw an increase of 8.3 log points in the number of FTFT students taking an institutional loan, with the average amount of the loan increasing by a marginally significant 248 dollars. Because the test-optional movement is recent and currently ongoing, it is too soon to know how these changes in average financial aid will affect students' long-term outcomes. Further implications of these findings are discussed in detail later.

I show that these results are robust to model specification choices, paying particular attention

[2]Interviews I conducted with different directors of admissions at adopting schools further bolster this claim. Karen Backs, the director of admissions at College of Saint Benedict & Saint John's University, who went test-optional in 2019, stated the school had to remove its need-blind policy to compensate for the changes in financial aid that were brought upon by the switch to test-optional.

to the possibility that other policies put into place when the schools became test-optional could be generating my findings. First, I use the Callaway and Sant'Anna (2021) design to create event-study figures that rule out the possibility that differential pre-trends are driving my results. I then show that the timing of adoption is not associated with changes in the number of new Black, Hispanic, or Native American faculty hired, changes in expenditures on public and academic services, or changes in application fees. Finally, I show that the results are robust to other difference-in-differences methods, including the standard two-way fixed-effect model, a mahabolonis-distance matching model, and a staggered synthetic control design.

To date, there have been relatively few studies assessing schools who have adopted a test-optional policy. Case studies focusing on individual institutions demonstrate that schools do receive more applications from students who would not have applied otherwise (Bates News, 2005) and receive applications from students who "underperformed" on the SAT (Robinson and Monks, 2005). These studies are limited methodologically in that they have no formal comparison group, and the racial and income diversity of the students are not a focus of the research, which has been cited as an important reason why schools become test-optional[3].

Two studies outside of economics have examined the impact of test-optional policies and have found mixed results. Most recently, Bennett (2021) evaluates changes in enrollment by race and Pell Grant receipt for the set of schools that adopted a test-optional policy between 2005 and 2015. The author finds that enrollment of first-time, full-time BNH students increased by 12 log points, and the number of undergraduates receiving a Pell Grant increased by 3 log points. The author does not measure changes in enrollment by quality, performance, or financial aid receipt, but the results by race and Pell Grant status are similar to the ones found in this paper. The differences in point estimates can be plausibly explained by changes in the sample and estimation method. By contrast, Belasco et al. (2015) find that the set of selective liberal arts schools that switched to test-optional between 1992 and 2010 saw no significant increases in the fraction of students receiving a Pell Grant or fraction of undergraduates identified as African American, Hispanic, or Native American. The reversal of their null result highlights the importance of evaluating the broader pool of test-optional institutions that have made the switch in recent years[4].

My results complement these papers in several ways, beginning with my empirical strategy. I use

[3]For example, when Wake Forest University went test-optional in 2009, Martha Allman, the Director of Admissions at the time, directly cited student diversity as the reason for the switch. Specifically, she stated: "By making the SAT and ACT optional, we hope to broaden the applicant pool and increase access at Wake Forest for groups of students who are currently underrepresented at selective universities."

[4]Using a limited version of the data from the Belasco et al. (2015) study, I can replicate their results to support this hypothesis. Results are available from the author upon request

new econometric methods that address the concerns of possible heterogeneous treatment effects that could bias the results from a standard two way fixed-effect design. Second, I separate the analysis on enrollment by race and gender to get a more complete picture of the effects of test-optional policies on enrollment. Third, I examine whether adopting schools saw changes in the quality of students enrolled both in terms of high school performance and retention rates. My findings address the concerns that test-optional policies would reduce the academic standing of admitted cohorts. Lastly, I include an analysis on changes in financial aid, which has been shown to have long-term effects on individuals' outcomes.

This paper also speaks to a growing empirical literature evaluating policies that reduce the inequity of college admissions exam-taking through greater access. One set of papers focuses on the causal effects of statewide legislation that mandates universal testing (Klasik, 2013; Hurwitz et al., 2015; Hyman, 2017; Goodman, 2016). These papers find that statewide testing programs have meaningful impacts on college enrollment. My findings add to this literature by evaluating a policy that serves as an alternative to offering college entrance exams for all students. Test-optional policies may be less expansive than universal testing programs, but they are also less costly for states. Furthermore, my results serve as a counterargument to the conclusions made from this literature. Goodman (2016) states that her results are explained by a large number of high-ability students underestimating their candidacy for selective colleges. However, if it was only the case that students need to take a college entrance exam to reveal they are prepared for selective colleges, we would not expect to see any significant changes in enrollment following the adoption of a test-optional policy. The results of this paper show that access is not the only limiting factor when it comes to applying to selective schools.

The rest of this paper is organized as follows. In Section II, we provide background information on the test-optional movement. Section III summarizes the data used in this paper. Section IV describes the empirical strategy and lays out the regression specifications. Section V contains the main results which includes my analysis on applications, enrollment, quality and financial aid. Section VI discusses possible threats to validity. Section VII offers conclusions from this research.

II The Test-Optional Movement

The use of standardized testing for college admissions began in the 20th century as an alternative to admitting students via institution-specific examinations or from preapproved high schools (Syverson, 2007). Although the first SAT was administered in 1926, it's widespread use in college admissions was not seen until the 1940s after the passage of the G.I. Bill for U.S. veterans of World War II. The large increase in the applicant pool led to an increase in SAT test-takers by a factor of eight during the 1940s and an additional factor of 10 increase during the 1950s (College Entrance Examination Board, 1975). By 1959, the ACT had emerged as the first large-scale competitor to the SAT.

As the use of standardized testing has become commonplace, concerns over their weight in college admissions has grown for two main reasons. First, there is a question of what information college entrance exams provide. In his 2004 paper, Jesse Rosthein decomposes the predictive power of the SAT by testing whether a predicted test score, based on student and school characteristics, could account for the relationship between SAT scores and first-year GPA (Rothstein, 2004). The author found that the orthogonal portion of SAT scores could not predict student success and that SAT scores appear to be a more effective measure of the demographic characteristics that predict first-year GPAs than they are of variations in student preparedness (conditional on background). This sentiment was bolstered by further research in the 2000s, which contended that test scores have low predictive validity and a high correlation with socioeconomic status (Bates News, 2005; Geiser and Santelices, 2007; Hiss and Franks, 2014). Second, there is the concern of inequity in access to college entrance exams. Bulman (2015) shows that only about half of high schools, in SAT dominated states, have an available testing center and these testing centers are concentrated in high schools with fewer subsidized lunch eligible students, and fewer shares of Black, Native American and Hispanic students. Buchmann et al. (2010) shows that there also exists inequity in access to college entrance exam preparation. The author finds that students from higher income households have greater engagement with preparation material, influencing both test performance and selective college enrollment. Together, these concerns at least partly spurred a number of institutions to become test-optional.

Test-optional policies have existed for some time but have become increasingly popular in the last two decades. Bowdoin College was the first to adopt a test-optional policy in 1969, but it was not until the mid-2000's that there was a sharp increase in the number of schools following suit. The number of schools that have switched to a test-optional policy increased from 10 in 2001 to over 250 in 2019.[5] The types of schools switching to test-optional admissions have also changed over time. The test-optional "movement", which began with a group of selective liberal arts colleges, has expanded to include both public research institutions and 5 of the U.S. News Top 50 Universities[6]. Schools often cite the desire to increase representation within their student body as the reason behind the change in policy[7] and the message that there are greater barriers to access to college entrance

[5]See Figure 1

[6]See FairTest (2022) for the list of schools

[7]For example, when Wake Forest University went test-optional in 2009, Martha Allman, the Director of Admissions at the time, directly cited student diversity as the reason for the switch. Specifically, she stated: "By making the SAT

exams for underrepresented groups of students has been carried forward in the set of schools that have gone test-optional because of the COVID-19 pandemic.[8]

At test-optional institutions, it is not required for a student to submit the SAT or ACT to be considered for admissions. The exact policy varies slightly across institutions. In some cases, students may be required to submit additional application materials, or scores from other standardized tests such as Advanced Placement or International Baccalaureate exams. While students are told they are not penalized for omitting their test scores, schools must rely more heavily on the other aspects of a student's applications (e.g. class rank, etc.) on a scale that is unknown to them.

The onset of the COVID-19 pandemic has greatly accelerated the number of institutions adopting a test-optional policy. From Spring 2020 to Fall 2021, nearly 800 institutions decided to allow students to temporarily apply without the inclusion of a college entrance exam score. Whether these institutions will remain test-optional is unknown. The University of California Board of Regents was the first large, state college system to announce they will permanently move to a test-optional admissions system (Nietzel, 2021). However, other institutions such as the Massachusetts Institute for Technology announced they will be moving back to a system that relies on use of college entrance exams (Schmill, 2022).

III Data

The data for this study come from multiple postsecondary data sources, including the Integrated Postsecondary Education Data System (IPEDS), the College Board's *Annual Survey of Colleges*, and the U.S. Department of Education.

The largest source of data comes from IPEDS. IPEDS has institutional-level data on every college, university, and technical/vocational institution that participates in the federal student financial aid programs (Title IV-eligible institutions) as required by the Higher Education Act of 1965 (IPEDS, 2020).[9] The dataset is a series of 12 interrelated survey components covering 9 major areas: Academic Libraries, Admissions, Completions, Enrollment (Fall), Finance, Graduation Rates and Outcome Measures, Human Resources, Institutional Characteristics and Student Financial Aid. The scope of the data has grown over time and as a result, there is often inconsistent time coverage

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[8]Harvard University released a statement in June of 2020 reading "We understand that the COVID-19 pandemic has created insurmountable challenges in scheduling tests for all students, particularly those from modest economic backgrounds, and we believe this temporary change addresses these challenges." (Fu and Kim, 2020)

[9]IPEDS also collects data on non-Title IV schools, but reporting from these schools are not required.

of each of the variables[10]. Similarly, some data are only required in alternate years. To combat these issues, I place requirements on the reporting behavior of each school I have in the sample. In order to be in the sample, a school cannot be missing more than 1 year of data for each of the following variables: Enrollment by Race, Number of Applications, Tuition and Total Enrollment.

The other sources of data come from the *Annual Survey of Colleges* and the Department of Education. The Annual Survey contains much of the same information as available through IPEDS, but also includes data on students' performance in high school. Specifically, I use this dataset to collect information on the percent of freshmen that had a final high school grade point average (GPA) above 3.0 and the percent of freshmen who graduated in the top half of their class. The Department of Education has institutional-level data on the total amount and number of students receiving a Pell Grant since 2000 (Department of Education, 2020). The difference between the data collected from IPEDS and the DOE is that the information is only calculated for total undergraduates rather than for first-time, full-time students which I have for all other variables.

My sample focuses on the years 2001-2018 and includes 1,042 colleges and universities. Schools that adopt a test-optional policy before 2005 or between 2015-2018 are not included in the sample[11]. Data on which schools switched to test-optional come from the National Center of Fair & Open Testing (FairTest, 2020) and includes those that have additional requirements for non-submitters. For every school, I assign the year of adoption as the fall semester in which students would have been affected by the test-optional policy[12]. I focus on schools adopting the test-optional policy between the years of 2005-2014 which gives me at least four years of data before and after the policy was implemented. In total, I identify 100 schools that adopt a test-optional policy between 2005-2014, of which, 85 meet the data requirements previously outlined.[13]

Table 1 documents summary statistics for several outcome variables in the dataset. Specifically, Table 1 compares the differences in means between schools that will adopt a test-optional policy to those that maintain a test requirement in the year 2004 (the year before any school adopted a test-optional policy). Test-optional schools are different from test-requiring schools on several

[10]For example, information on retention rate is available from 2002 through 2018, but information on the number of students retained is only available from 2006 onward. For situations like these, I use the variable with the largest sample year coverage and include results for the other variables in the appendix.

[11]I also conduct the analysis including only schools that adopt test-optional policy from 2001 to 2018. In this specification, I use the method developed by Callaway and Sant'Anna (2021) where the comparison group is the set of not-yet treated schools. See Appendix Table A1 for those results

[12]For example, several schools are noted to have adopted the policy in the spring or summer season. I assign treatment to the following fall as all of my variables report information based off fall enrollment.

[13]Bennett (2021) use a hand-coded system to identify test-optional schools. I identified few differences in my coding and that found in Bennett (2021), but the results are not sensitive to these differences.

fronts. Adopting schools on average have a lower percentage of first-time, full-time BNH enrollment, a smaller undergraduate population, have higher tuition (based on 2010 dollars), a lower percentage of students receiving a Pell Grant, and overall higher student quality measures. However, level differences between the adopting and control school are not of concern in a difference-in-differences model, so long as the two groups of school are not following differential trends. I visually test this assumption using the event-study specification described in the following section.

IV Empirical Strategy

I identify the effect of switching to a test-optional admissions policy using a difference-in-differences and event study design developed by Callaway and Sant'Anna (2021). I use their estimation procedure to identify group-specific average treatment effects on the treated (denoted as $ATT(g,t)$) which reflect the average treatment effects on the treated for group g at time period t. In this context, each group g represents the set of schools who adopt a test-optional policy in the same year. For example, $g = 2005$ represents the set of schools that adopted the policy in 2005, $g = 2006$ represents the set of schools that adopted the policy in 2006 and this continues until the final adoption year of 2014. Time periods, t, include years leading up to and following adoption of the policy. $C = 1$ indicates the schools that are in the control group.

Callaway and Sant'Anna (2021) formally shows that under the assumption of conditional parallel trends between the control and treatment groups, the group-specific average treatment effects can be represented by

$$ATT(g,t) = E[Y_t - Y_{g-1}|G_g = 1] - E[Y_t - Y_{g-1}|C = 1] \quad (1)$$

where the average effect of adoption for units in group g is identified by taking the evolution of the outcome variable actually experienced by that group (the first term in Equation (1)) and adjusting it by the evolution of the outcome variable experienced by the control group (the second term in Equation (1)). Under the parallel trends assumption, this second term is the path of outcomes that units in group g would have experienced if they had not adopted the policy. Both terms in Equation (1) are easily calculated as simple averages from the data. Once the $ATT(g,t)$ has been calculated for each treatment g and time period t, I combine the estimates to form the aggregated causal parameters.

I use Callaway and Sant'Anna's dynamic aggregation approach to assess the validity of the parallel trends assumption and to examine the effects of adoption as a function of years relative to the treatment period g. For each event-time e relative to a treatment date (e.g., 2 years after g), I find the relevant ATT(g,t) for each treatment group that corresponds to the relative time period. For example, $e = 2$ for treatment group $g = 2005$ corresponds to the year 2007. I then take an average of the $ATT(g,t)s$ across groups (weighting by the group size) to retrieve a single average treatment effect estimate for each event-time e. These estimates can include time periods before the treatment occurs ($e < 0$). I then plot these averages to represent the typical event-studies seen in standard difference-in-differences designs. To create a single, overall point estimate, I take the average all of the identified group-time average treatment effects together[14]. For inference, I use Callaway and Sant'Anna's recommended bootstrapping procedure and cluster at the school-level.

Identification relies on the assumption that had the adoption of the policy not occurred, treatment and control schools would have followed parallel trends in the outcome variables. In this setting, I assume that outcomes in schools that adopted a test-optional policy would have followed parallel paths as the outcomes in schools that did not adopt a test-optional policy, if adopting schools had not switched. This is ultimately an untestable assumption. I gauge the plausibility of this assumption by testing (1) whether trends in outcome variables are parallel across treatment and control units in the years leading up to the year of adoption, and (2) whether other observable characteristics between treatment and control units were parallel before and after the treatment. This second test helps mitigate the concern that other characteristics of the schools that could affect outcomes changed at the same time as switch to test-optional. I find that the results are robust to both tests, bolstering the plausibility of our estimates.

In my preferred specification, I incorporate pre-treatment covariates using Callaway and Sant'Anna's procedure to create propensity-score-based matches between treatment and control units. This adjustment is needed if one believes that the parallel trend assumption only holds conditional on covariates. The results of most outcome variables are not sensitive to this specification choice, but help mitigate concerns about the baseline differences between the two groups. Equation (1) is augmented with propensity score weights for each group so that control schools are weighted more if they are similar to members of the treatment group across included covariates[15]. Formally, this is represented by[16]

[14]For a full discussion and proof of this method, see of Callaway and Sant'Anna (2021). All treatment effects are calculated using Fernando Rios-Avila, Pedro H. C. Sant'Anna and Brantly Callaway's stata command, csdid. See `https://github.com/friosavila/csdid_drdid` for more information on this package.

[15]This procedure does not adjust for any time-varying covariates that are orthogonal to pre-treatment observables.

[16]Specifically, this method incorporates coviarates using inverse probability weighting. Callaway and Sant'Anna (2021) outlines several methods to incorporate covariates. The results are not sensitive to specification choice as seen in Appendix Table A7

$$ATT(g,t) = E\left[\left(\frac{G_g}{E[G_g]} - \frac{\frac{p_g(x)C}{1-p_g(x)}}{E[\frac{p_g(x)C}{1-p_g(x)}]}\right)(Y_t - Y_{g-1})\right] \tag{2}$$

An additional assumption is needed with this specification. Specifically, there must be enough common support across treatment and control group covariates to create reasonable propensity score matches. This is a common assumption in the matching literature.

V Effects of Test-Optional Policies

In this section, I describe the school-level outcome results. I will present the results in three sections. In Section V.A, I document the effect of switching to a test-optional policy on applications, admissions and total enrollment. I then extend the analysis in Section V.B to examine the effects on enrollment by race, Pell Grant status and then by race and gender. Finally, in Section V.C, I present the effects of test-optional policies on the quality of students and their financial aid receipt.

V.A Applications, Admissions and Enrollment

V.A.1 Applications and Admissions

Test-optional policies have received the criticism that they are programs that artificially increase institutional position through increasing the number of applications while keeping admissions the same (Epstein, 2009; Belasco et al., 2015). I, therefore, begin by presenting the results on applications and admissions using the Callaway and Sant'Anna (2021) approach described in the previous section. Figure 2 shows the results of the event-study analysis for the log number of applications (Panel A), the admissions rate (Panel B), the matriculation rate (Panel C), and the percent of enrolled first-year students that submitted either the SAT or ACT (Panel D)[17].

The results presented in Panels A-C suggest that switching to a test-optional policy did not significantly impact the application or admissions behavior of adopting schools. I find no evidence that schools are trying to increase their selectivity by deflating their admission rate. Given the relatively flat results of the admissions rate, it must be the case that adopting schools are increasing the number of students they admit when they see increases in applications[18]. On average, schools that switched to a test-optional policy saw a statistically insignificant increase of around 2 log points in the number of applications, a 1.6 p.p increase in the admissions rate, and a 0.35 p.p decrease in the matriculation rate.

[17]Appendix Table A2 presents the results with and without covariates.

[18]The aggregated result for the log number of applications is a statistically insignificant increase of 2 log points and 4 log points for the log number of admissions, which supports this claim.

Despite statistically insignificant results on the number of applications, adopting schools saw sharp and significant changes with regards to the submission of standardized test scores of their enrolled first-year students. Panel D shows that adopting schools saw a 15.69 p.p. decrease in the percentage of enrolled first-year students who submitted the SAT or ACT after switching to a test-optional policy. The decline immediately followed the switch and then grew as the policy remained. These results suggest that test-optional schools enrolled students that did not submit their standardized test scores, resembling the "first-stage" effects of the policy.

V.A.2 Overall Enrollment

I next present the results on overall enrollment in Table 2. Each cell presents the single, aggregated average treatment on the treated effect (ATT) for the separate estimations. Columns (1) and (2) present the results for first-time, full-time (FTFT) enrollment, while Columns (3) and (4) present the results for overall full-time (FT) enrollment. I separate the table to show the results when pre-treatment covariates are included (Panel A) and when they are not (Panel B).

The results in Panel A suggest that after switching to a test-optional policy, adopting schools saw a statistically significant increase of 31 FTFT (or 4.5 log points) students, and a statistically insignificant increase of 166 (or 3.2 log points) total FT students when compared to the control group. This may suggest that these schools are growing as a result of the policy. However, in the specifications without pre-treatment covariates. (Panel B), the results do not hold in levels. In Panel B, the results in columns (1) and (3) suggest that test-optional schools are decreasing the number of FTFT students by 19 and the number of FT students by 43. The sensitivity of the results to the inclusion of the pre-treatment covariates is only apparent for the set of results that rely on the levels of the outcome variables. In columns (2) and (4) of Panel B, we see the results are almost identical to what is seen in Panel A. In total, I take the results of Table 2 as evidence that adopting schools are slightly increasing their enrollment as a result of the policy. Furthermore, the changes in the coefficients from levels to logs simply suggests that my results are driven by smaller schools.

V.B Enrollment by Race, Pell Grant Status and Gender

V.B.1 BNH Enrollment and Pell Grant Receipients

Schools often cite the desire to increase representation within their student body as a motivating factor behind the switch to a test-optional policy. Therefore, in this section I examine whether adopting schools see changes in the enrollment of Black, Native American and Hispanic students and Pell Grant recipients. Table 3 shows the effect of adopting a test-optional policy on the level (column

1), log number (column 2), and percentage of BNH or Pell Grant students enrolled. Panel A focuses the analysis on first-time, full-time BNH students, while Panel B presents the results on all full-time BNH students. Panel C shows the results for all Pell Grant recipients. Each specification includes the use of pre-treatment covariates[19]. The results show consistent and statistically significant effects of the adoption of test-optional policy on these outcome variables of interest.

The results of Panel A suggest that adopting a test-optional policy is associated with increased enrollment of FTFT BNH students. Columns (1) and (2) show that after the switch to a test-optional policy, adopting schools saw an increase of 18.76 (or 18.2 log points) FTFT BNH students enrolled. Column (3) confirms the increase in overall enrollment presented in Table 2 does not drive the results on FTFT BNH enrollment. Specifically, after implementing the policy, adopting schools increased the percentage of FTFT students who identify as BNH by 1.5 percentage points, suggesting that test-optional schools are changing the composition of students on their campus rather than just expanding. When I look at the results on full-time enrolled BNH students, I find a similar pattern holds. On average, schools that adopted the policy saw a statistically significant increase of 76.23 FT (or 18.4 log points) BNH students enrolled, which translated into a rise of 1.3 p.p. in the percent of FT students identified as Black, Native American, or Hispanic. Panel C examines the effects of test-optional policies on the enrollment of students that receive a Pell Grant. After the switch to a test-optional policy, adopting schools saw an average increase of 213.66 (or 10.2 log points) Pell Grant recipients, which translated to a statistically significant increase of 1.4 p.p in the percent of students receiving a Pell Grant.

Together, these results suggest that over the entire post-period, adopting schools saw significant changes in the enrollment composition of their student body. Figure 3[20] examines the timing of these effects to give a sense of when enrollment patterns changed and if preexisting trends are driving the results. For FTFT (Panel A) and FT BNH enrollment (Panel B), there is an immediate jump following the adoption of the policy, and this effect continues to grow as the policy remains in place. For Pell Grant enrollment (Panel C), the effect of the policy becomes statistically significant after two years. The delay in the increase of Pell Grant students may be due to the fact that data available focuses only on the total number of undergraduates receiving a Pell Grant. We may not expect to see statistically significant results until students admitted under the test-optional policy make up a larger proportion of the student body. For each of our outcomes of interest, the differences between adopting and test-requiring schools are relatively flat before adoption, lending to the credibility of this design. A further discussion on the validity of the empirical strategy is in the following section.

[19] Appendix Table A3 presents the results without the use of pre-treatment covariates

[20] Appendix Figure A1 shows the results using levels and Appendix Figure A2 shows the results using percentages.

V.B.2 Enrollment by Race and Gender

In the previous section, I presented evidence that schools saw modest increases in the enrollment of FTFT BNH students following the adoption of a test-optional policy. However, these average estimates can hide variation in the impact of this policy across subgroups. Therefore, I disaggregated the results by race and gender. The analysis also includes the results for non-BNH students. I calculated these estimates using school-level measures of the number of FTFT students enrolled by each race and gender combination.

Figure 4 displays the results of this analysis for the log number of FTFT students enrolled[21]. Each panel of the figure plots, for a given race, the simple, aggregated average treatment on the treated effect separately for men and women. Black and Hispanic women saw the largest changes in enrollment. On average, schools that adopted a test-optional policy saw a 22.9 log point increase in the number of FTFT Black women and a 24.2 log point increase in the number of FTFT Hispanic women. There is suggestive evidence that adopting schools are also seeing increases in enrollment of Black, Native American and Hispanic men, but most estimates are not statistically significant. We might see differences across gender within these racial groups for two reasons. First, women enroll in college at a higher rate than men within each racial group (National Center of Education Statistics, 2021). Second, while females tend to perform worse on college entrance exams when compared to males (National Center for Education Statistics, 2020a,b), they often outperform them on other metrics (Conger, 2015; Goldin et al., 2006). Together, we might expect female students to have a greater advantage as test-optional policies are adopted.

Figure 4 also displays the results for FTFT White and Asian enrollment. The enrollment of these groups of students is not often stressed in the test-optional literature but are included here to get a more complete picture of how enrollments are changing as a result of the policy. I find no evidence that switching to a test-optional policy increases the number of FTFT White women or men enrolled, however the point estimate in the bottom panel of the figure suggests that FTFT Asian Women enrollment increased by 18 log points following the adoption of the policy. While this estimate is statistically significant, it is not consistent across variable definition (levels vs. logs vs. percentages) and may be a result of a significant number of school-year observations recording zero enrollments of FTFT Asian students[22]. Therefore, I refrain from interpreting this result as a definite increase in the enrollment of first-time, full-time Asian women.

[21] Appendix Figure A3 plots the coefficients when the estimation is run on levels and percentages.

[22] Appendix Table A5 shows the results for all variable definitions and illustrates the issue of missing values for log FTFT Asian enrollment.

V.C Results on Quality and Financial Aid Receipt

V.C.1 Quality and College Performance

In this section, I further explore the effects of test-optional policies on student composition by examining school-level changes in the quality of students enrolled and the subsequent retention and graduation rates. Table 4 presents the results of the Callaway and Sant'Anna (2021) estimation procedure on the percent of enrolled freshmen with a high school grade point average (GPA) above a 3.0 (column 1), the percent of enrolled freshmen with a high school class rank in the top half (column 2), retention rates (column 3) and 6-year graduation rates (column 4)[23].

Performance in high school classes is consistently ranked as one of the most important factors in college admissions decisions[24]. I, therefore, begin with the analysis on the percent of enrolled freshmen with a high school GPA above 3.0. Adopting a test-optional policy decreases the percent of enrolled freshmen with a high school GPA above 3.0 by a statistically insignificant 0.43 p.p., or by 0.6 percent. The estimates that control for pre-treatment school-level covariates are nearly identical to those that do not. Figure 5(a) presents the change in freshmen high school GPA relative to the year the school adopted a test-optional policy. Freshmen GPA remains relatively stable prior to the adoption of the policy and in the years after. Together, the results suggest that after the adoption of the policy, the percent of freshmen with a high school GPA above 3.0 is not systematically changing.

However, schools that adopted a test-optional policy did see a decrease in the percent of enrolled freshmen with a high school class rank in the top half when compared to the control group. Following the switch in the admissions policy, schools saw the percent of enrolled freshmen with a high school class rank in the top half fall by a statistically significant 1.557 p.p., or by 1.9 percent. Figure 5(b) allows for the examination of the timing of these effects. While none of the individual event-time estimates are statistically significant, we see the largest drop in the our outcome variable of interest occur in the first year schools went test-optional. In the years following, the estimates almost return back to their pre-policy levels. This result suggests that after adopting a test-optional policy schools may have had to undergo an adjustment period as they made changes to their admissions formula[25].

While schools may have seen some decreases in the high school performance of their enrolled freshmen, this did not translate into meaningful differences in their college performance. Adopting

[23] Appendix Table A6 presents the results in levels and logs for retention and graduation. This analysis requires the use of a more limited version of the data.

[24] In 2018, the National Center for College Admission Counseling reported the results of a survey they conducted and found that 91.3% of higher education istitutions place either "considerable" or "moderate" importance on high school GPA when considering admissions decisions (Clinedinst and Patel, 2018).

[25] Only 37% of higher education institutions place considerable or moderate importance on high school class rank, but this may have had change once schools went test optional (Clinedinst and Patel, 2018).

schools saw a statistically insignificant decrease of 0.187 p.p. (or 0.25 percent) in their retention rates and a decrease of 0.305 p.p.(0.5 percent) in their 6-year graduation rate. The event-study results presented in Figures 5(c) and 5(d) further show that relative to before the policy, adopting schools saw no differential change in their retention or graduation rates. I take this as evidence that while test-optional policies have changed the composition of students enrolled, there is no evidence that there has been a subsequent change in the overall retention and graduation of these students.

V.C.2 Financial Aid Receipt

Given the change in composition of students enrolled at test-optional schools, one might expect to see changes in the average financial aid packets offered. Table 5 reports the Callaway and Sant'Anna (2021) estimation procedure where the outcome variables of interests are school-level measures of financial aid receipts. Panel A reports changes in institutional grant aid for first-time, full-time students, Panel B reports changes in institutional loans taken by FTFT students, and Panel C reports changes in overall Pell Grants. Columns (1) and (2) focus on how the number of students receiving a specific type of financial aid (in level and logs respectively), while Column (3) looks changes in percentages and Column (4) reports changes in the average amount of the specific type of financial aid received.

After adopting a test-optional policy, schools that switched increased the number of students that received institutional aid by 7.4 log points, yet the average amount of the institutional grants decreased by 1,130.63 (2010 $). These results suggest that schools have had to respond to the change in financial need of their enrolled cohorts. Figure 6 shows the timing of these results using the event-study procedure described in the previous section. The increases in number of FTFT receiving institutional aid (Figure 6(a)) is apparent in the first year of the program and remains constant in the post-period. The decreases in the amount of institution aid (Figure 6(d)) slightly reduces in the first year, but falls further as the program remains. Interestingly, students seem to somewhat offset the decreases in institutional aid by taking out loans. Schools that adopted a test-optional policy saw increases of 8.3 log points in the number of FTFT students taking an institutional loan after they made the switch. There is some suggestive evidence that the amount of loans FTFT students are taking also increases by around 250 (2010 $) dollars in the post-period, but when looking at the event-study estimates in Figure 6(e) there is not clear increase following adoption. It is important to note, that it does not seem that students are covering the rest of the decline in institutional aid with Pell Grants. More students are receiving Pell Grants, but the average amount of the grant only increases by a statistically insignificant 25 dollars.[26].

[26] Students could be receiving additional funds from other sources, such as state aid or federal loans, but that data

Because the test-optional movement is recent and currently ongoing, it is too soon to know how these changes in average financial aid will affect students' long-term outcomes. A large body of work shows that students who have experienced greater access to financial aid have better outcomes (Deming et al., 2010; Dynarski and Scott-Clayton, 2013; Scott-Clayton and Zafar, 2019), but it is not immediately apparent from the results of this study that students are significantly worse off as a result of this policy change. In fact, results from Black et al. (2020) show that increases in student loans can also have positive effects on student outcomes. Future work will have to explore whether the composition of student aid alters longer-term outcome for these students.

VI Threats to Validity

The previous section shows that adoption of a test-optional policy is associated with increases in enrollment of BNH and Pell Grant students. There remain, however, three potential threats to validity that should be addressed. Specifically, (1) the impact of test-optional policies adopting schools may be driven by differential trends in enrollment across the two groups of schools before program implementation, (2) there may be other policy innovations besides the move to a test-optional policy that may be driving the results and (3) the results may be sensitive to the use of a specific estimation strategy.

To ensure that the findings are not driven by differential trends between schools that adopt the policy and the control group, Figure 3 plots the event-study estimates for the main outcome variables of interest. This gives a sense of when enrollment patterns changed and if preexisting trends are driving the results. The coefficients are plotted with 95 percent confidence intervals. The adoption of the test-optional policy is indicated at year T + 0. Prior to adoption, eventual test-optional schools are the control group appear to have similar trends in enrollment as can be seen by the relatively flat difference between the two sets of schools[27]. In all the years before adoption the 95 percent confidence intervals contains zero for both log FTFT BNH enrollments and log number of students receiving a Pell Grant. Four years before adoption there appears to be a one time deviation from the flat trend for log FT BNH enrollment, but in the three years leading up to adoption there does not appear to be any trend.

The second concern is that there may be other policy interventions beyond the switch to a test-optional policy that are driving the results. To address this issue I use year fixed effects in each of the specifications to capture shocks common to both the treatment and control groups. Unaccounted for shocks could still exist, but those shocks would have to elicit disproportionate

is not available until 2008.

[27]Event-studies for the other outcomes of interest are presented throughout the paper.

reactions from the adopting schools to account for our results. A particular concern is that schools that switch to a test-optional admissions policy may also be implementing a suite of programs to attract underrepresented groups of students. In Table 6, I rule out three programs schools could have implemented in conjunction with the move to test-optional. First, I assess whether adopting schools changed their application fees. Previous work has shown that these fees can serve as a barrier for low-income students when applying to colleges (Pallais, 2015; Hurwitz et al., 2017) and if test-optional schools are simultaneously switching their admission policy and reducing application fees, we would not be able to disentangle which program is driving the results. However, the results of column (1) in Table 6 suggest that adopting a test-optional policy is not associated with a statistically significant change in application fees when compared to the control group.

I also examine whether adopting schools changed their public and academic expenditures after the switch to test-optional. If adopting schools are simultaneously increasing their outreach to different communities or expanding academic services available to their students we may expect these changes in expenditures to attract a new group of students that could explain the results. However, the results in column (2) of Table 6 suggest that adopting a test-optional policy is associated with a statistically insignificant decrease of $283,000 in public and academic expenditures (in 2010 dollars)[28]. The coefficient is in the opposite direction of what we would expect if adopting schools were using public outreach or academic services to attract new students.

Finally, I examine whether adopting a test-optional policy is associated with a change in the number of Black, Native American or Hispanic faculty and staff new hires. Previous research has highlighted the impact that same-demographic role models can have on students' outcomes (Carrell et al., 2010; Fairlie et al., 2014; Bettinger and Long, 2005) and if adopting schools are hiring an increased number of BNH faculty and staff to attract this group of students we may falsely attribute the results of this paper to the test-optional policy. However, the result in column (3) of Table 6 suggests that adopting test-optional policy is not associated with an increase in the number of BNH faculty and staff new hires, so it is unlikely that changes in faculty composition is driving the results.

There is also the concern that the results are sensitive to the use of this particular estimation strategy. I, therefore, estimate the main analyses in Tables 3, 4, and 5 using three alternative estimation strategies. Specifically, I re-run the results using the standard two-way fixed-effects model, a mahabolonis-distance matching model, and the staggered synthetic controls design developed by Ben-Michael et al. (2021)[29] I find consistent evidence that, irrespective of the estimation strategy, the signs and general significance levels of the single, aggregated average treatment on the treated

[28]Based on the available data in IPEDS I cannot examine changes in academic and public expenditures separately.

[29]A further description of the two-way fixed effect model and the matching procedure is available in the Appendix.

effect are maintained as shown in Table 7.

VII Conclusion

This paper analyzes the effects of test-optional policies on student composition at adopting schools. I found that institutions that switched to a test-optional policy saw an increase in the number of BNH and Pell Grant students enrolled, with the most significant gains in enrollment coming from Black and Hispanic Women. I showed that these results were not due to differential trends between the adopting and control schools and were not the result of other policy interventions occurring at the same time. Beyond students' racial and income composition, I also show that adopting a test-optional policy is not associated with any changes the quality of students. Test-optional schools saw no statistically significant changes in their retention or 6-year graduation rates. However, test-optional schools did see changes in their financial aid disbursement. Adopting schools increased the number of FTFT students receiving institutional aid, but the amount aid they received fell.

There still remain several questions about test-optional policies that need to be addressed to understand their full impact. First, is the question of where test-optional schools are drawing their new enrollments from. Understanding the answer to this question helps inform whether and how these policies could change college-going in the longer run. In Figure 7, I examine three sources from which test-optional schools could be receiving new students. First, I consider whether test-optional schools are shifting students away from their peer institutions[30]. In Panel A of Figure 7, I plot average FTFT BNH enrollment for ever-test optional schools and their peer institutions and find that test-optional schools are not shifting BNH students from their peer institutions. It seems that ever-test optional schools are catching up to their peer institutions by adopting the admissions policy. Next, I consider whether test-optional schools draw BNH students from their nearest four-year schools. Panel B of Figure 7 plots the average FTFT BNH enrollment for ever test-optional schools and the nearest 4-year college or university. Again, test-optional schools do not seem to be drawing students from this source; if anything, they are catching up to surrounding colleges and universities. Finally, in Panel C of Figure 7, I explore whether these enrollments in test-optional schools could be coming from nearby two-year schools. For this panel, I plot the average FTFT BNH enrollment of two-year schools within 30 miles of an ever test-optional school and two-year schools farther than 30 miles from any test-optional school. While noisy, there does seem to be a slight decline in FTFT BNH enrollment in two-year schools that are closer to an ever test-optional school, suggesting that test-optional policies could be inducing some students out of two-year schools into

[30]Peer institutions are identified from the National Center for Education Statistics.

four-year schools. However, this analysis is purely speculative, and future work is needed to explore how test-optional policies could change college-going in equilibrium and address the concerns over what schools will rely on absent standardized testing.

Second, the financial aid results of this paper suggest that students are supplementing the decrease in institutional aid with an increase in institutional loans and there is a question of whether this shift in financial aid has longer term consequences. The current literature on the effect of student debt on labor market and other life cycle outcomes is mixed. Most recently, Black et al. (2020) shows that increases in student borrowing limits significantly increased constrained students', bachelor degree attainment, labor market outcomes, and loan repayment. However, their findings are in contrast to much of the literature that finds additional loan debt negatively affects outcomes including graduate school enrollment (Chakrabarti et al., 2020), and home ownership (Mezza et al., 2020). Conversations with admissions directors have highlighted how this has become a critical point of discussion for schools considering the move to test-optional, but future work will have to be done to explore this question.

A final question of interest is why schools adopt test-optional policies to address concerns over equity in college admissions. Some work has suggested that institutions switch to test-optional as a marketing decision either to garner more applications or to increase the average reported SAT/ACT score (Syverson, 2007), while other work cites high school performance as being the better indicator for success or the desire to attract a more diverse applicant pool (Rooney and Schaeffer, 1998). Recent work suggests that there may be a financial incentive for schools to switch to test-optional admissions. If test-optional policies help recruit Pell Grant recipients, schools may be able to capture some of the federal money they receive (Turner, 2017). However, I find no evidence that test-optional schools are adopting the policy in times of low revenues or when they are losing enrollments[31]. Conversations with admissions directors suggest that they made the switch because of specific qualms with the way college entrance exams work, but this evidence is ultimately anecdotal. Future work will further explore why schools choose to go test-optional rather than instill other policies that could promote the enrollment of underrepresented groups of students.

[31] Mean plots are available upon request.

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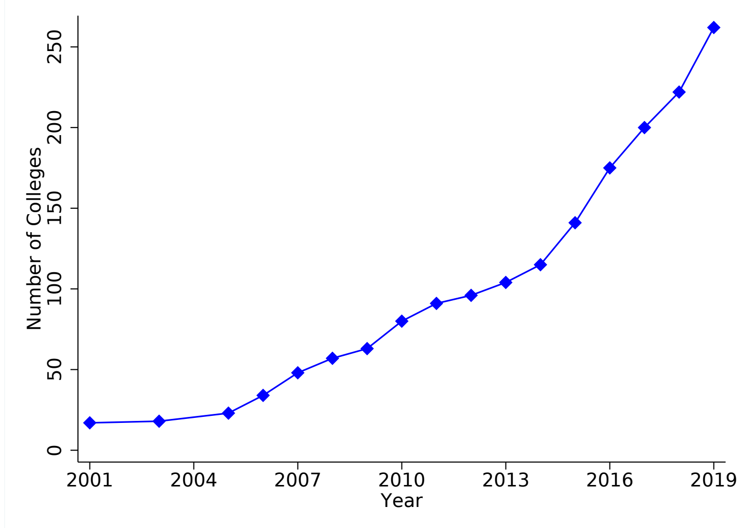
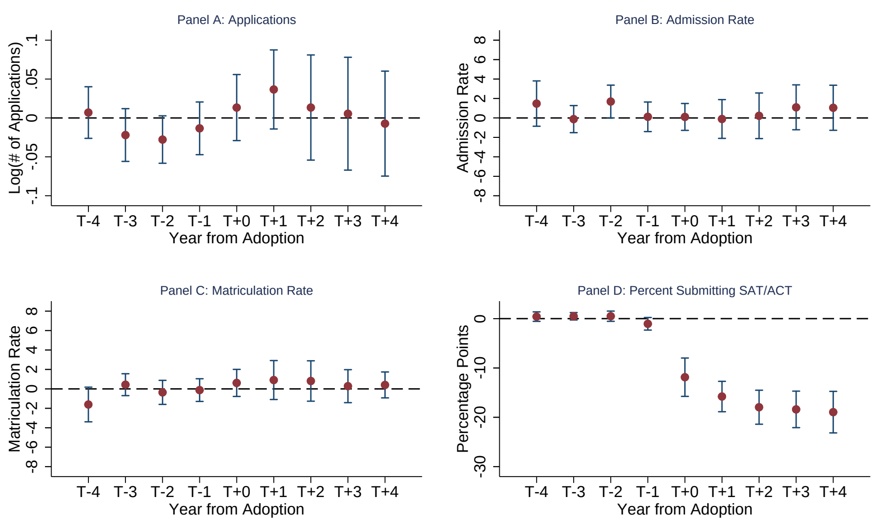
Figures

Figure 1: Cumulative Number of Schools Switching to Test-Optional



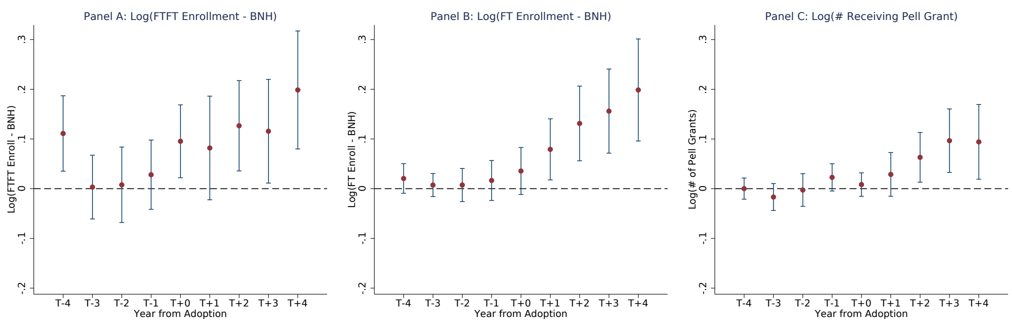
Notes: This figure presents the cumulative number of schools that have adopted a test-optional policy from 2001 to 2019. Data on the timing of adoption comes from the National Center of Fair & Open Testing.

Figure 2: Effects of Test-Optional Policies on Applications and Admissions



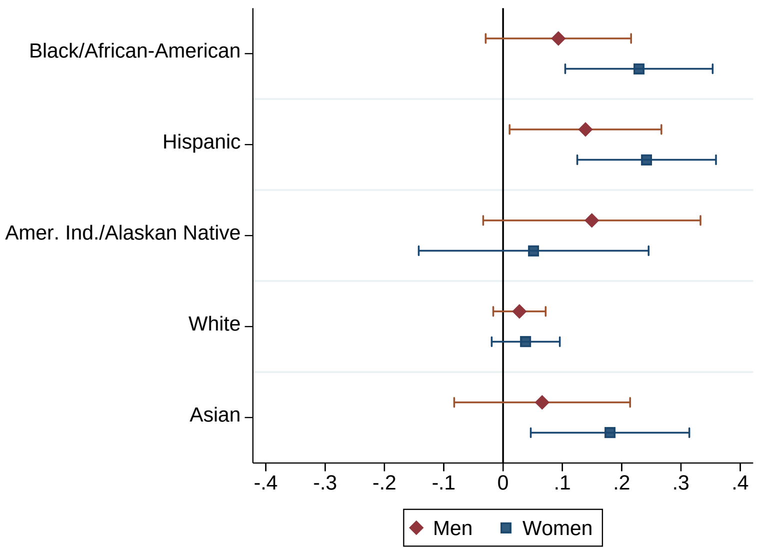
Notes: This figure presents the event-study estimates from the Callaway and Sant'Anna (2021) procedure. Figure 2(a) plots the estimates for the log number of applications, Figure 2(b) plots the estimates for the admissions rate, Figure 2(c) plots the estimates for the matriculation rate, and Figure 2(d) presents the estimates for the percent of enrolled freshmen that submitted either the SAT or ACT. Each figure is the result of a separate estimation. 95% confidence intervals are reported. All estimations include school and year fixed effects. Pre-treatment covariates include in-state published tuition, the availability of a "Loan Cap" policy and full-time enrollment in 2001. Data on applications, admissions and enrollment come from IPEDS. Data on SAT/ACT submissions comes from the College Board's *Annual Survey of Colleges*. See Appendix Table A2 for the number of observations and baseline means.

Figure 3: Effects of Test-Optional Policies on BNH Enrollment and Pell Grants



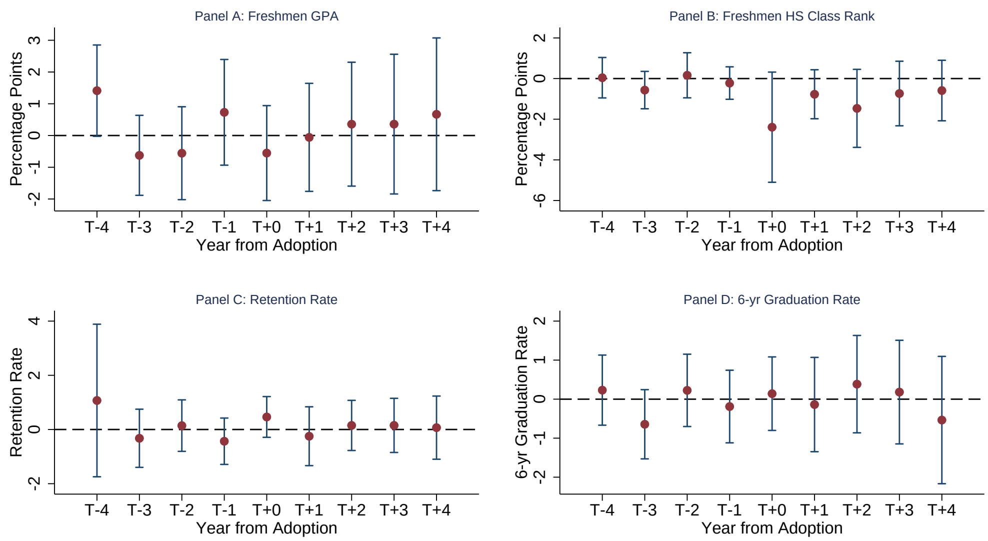
Notes: This figure presents the event-study estimates from the Callaway and Sant'Anna (2021) procedure. Figure 3(a) plots the estimates for the log number of FTFT BNH students enrolled, Figure 3(b) plots the estimates for the log number of FT BNH students enrolled, and Figure 3(c) plots the estimates for the log number of students receiving a Pell Grant. Each figure is the result of a separate estimation. 95% confidence intervals are reported. All estimations include school and year fixed effects. Pre-treatment covariates include in-state published tuition, the availability of a "Loan Cap" policy and full-time enrollment in 2001. Data on enrollment comes from IPEDS. Data on Pell Grant Recipients comes from the Department of Education. See Table 3 for observations and control means.

Figure 4: Effects of Test-Optional Policies on Enrollment by Race and Gender



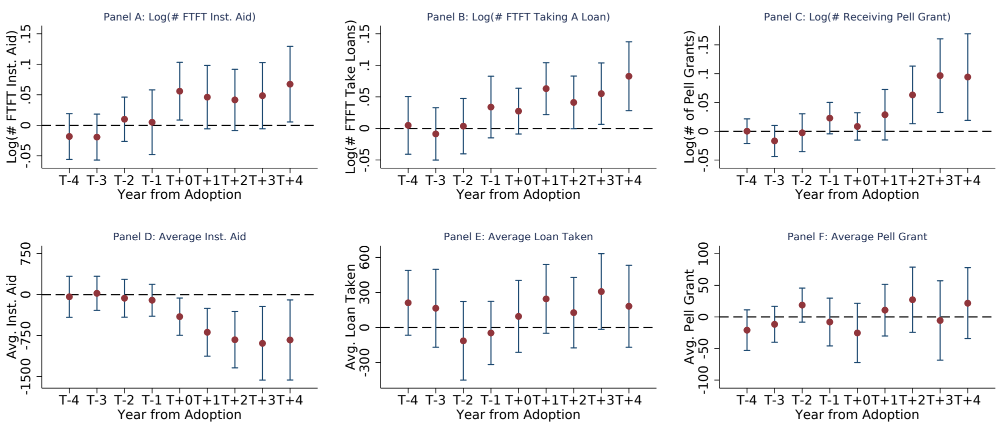
Notes: This figure presents single, aggregated average treatment on the treated effects for FTFT enrollment of Black/African-American, Hispanic, Native American, White and Asian students, separately for men and women. Each point is the result of a separate estimation. Red diamonds always represent the results for men and Blue squares always represent the results for women. 95% confidence intervals are reported. All estimations include school and year fixed effects. Pre-treatment covariates include in-state published tuition, the availability of a "Loan Cap" policy and full-time enrollment in 2001. Data on enrollment comes from IPEDS. See Appendix Table A5 for information on control means and observations.

Figure 5: Effects of Test-Optional Policies on Enrollment and College Performance



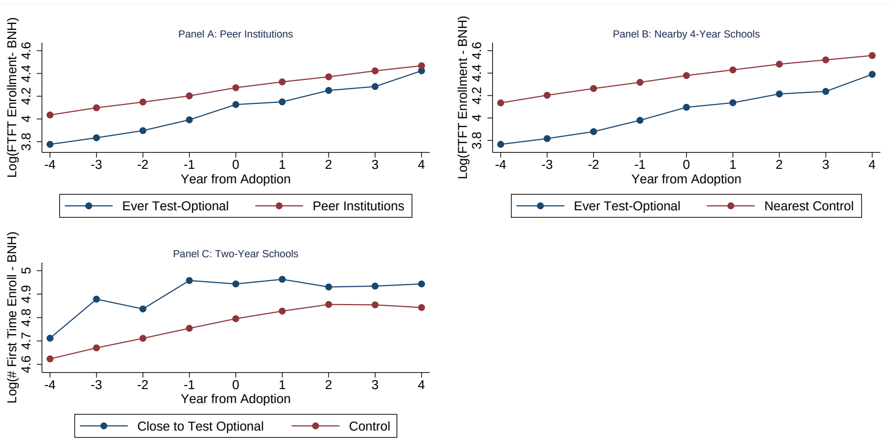
Notes: This figure presents the event-study estimates from the Callaway and Sant'Anna (2021) procedure. Figure 5(a) plots the estimates for the percent of freshmen enrolled with a HS GPA > 3.0, Figure 5(b) plots the estimates for the percent of freshmen enrolled with a HS Class rank in the top half, Figure 5(c) plots the estimates for the retention rate, and Figure 5(d) plots the estimates for the 6-year graduation rate. Each figure is the result of a separate estimation. 95% confidence intervals are reported. All estimations include school and year fixed effects. Pre-treatment covariates include in-state published tuition, the availability of a "Loan Cap" policy and full-time enrollment in 2001. Data on high school performance comes from the College Board's *Annual Survey of Colleges.* Data on retention and graduation come from IPEDS. See Table 4 for information on control means and number of observations.

Figure 6: Effects of Test-Optional Policies on Financial Aid Receipt



Notes: This figure presents the event-study estimates from the Callaway and Sant'Anna (2021) procedure. Figure 6(a) plots the estimates for the log number of FTFT receiving institutional aid, Figure 6(b) plots the estimates for the log number of FTFT taking an institutional loan, Figure 6(c) plots the estimates for the log number of students receiving a Pell Grant, Figure 6(d) plots the estimates for the average amount of institutional aid FTFT students receive (in 2010 \$), 6(e) plots the estimates for the average amount of the institutional loans FTFT students take (in 2010 \$), 6(f) plots the estimates for the average amount Pell Grant students receive (in 2010 \$). Each figure is the result of a separate estimation. 95% confidence intervals are reported. All estimations include school and year fixed effects. Pre-treatment covariates include in-state published tuition, the availability of a "Loan Cap" policy and full-time enrollment in 2001. Data on institutional aid and loans come from IPEDS. Data on Pell grants come from the Department of Education. See Table 5 for control means and number of observations.

Figure 7: Enrollment Plots of 2-Year, Nearby 4-Year, and Peer Institutions



Notes: This figure presents the average log FTFT BNH enrollment across different event times in the sample. Figure 7(a) plots enrollment for ever-test optional schools (blue) and their peer institutions as indicated by the National Center for Educational Statistics (red), Figure 7(b) plots enrollment for ever test-optional schools (blue) and the nearest 4-year college or university (red), Figure 7(c) plots enrollment for 2-year schools within 30 miles of ever-test optional schools (blue) and 2-year schools farther than 30 miles from a test-optional school (red). Data on enrollment comes from IPEDS.

Tables

Table 1: Summary Statistics in Baseline Year

	Test Optional		Test Requiring		**Control - Treated**
	Mean	Std. Dev.	Mean	Std. Dev.	Diff.
% FTFT Enroll -BNH	9.2	5.9	19	21	9.7
# of Applications	3,813	4,309	4,234	5,636	421
Full-Time Enrollment	2,685	2,717	4,746	5,785	2,061
Published In-State Tuition	19,753	5,148	11,414	7,017	-8,339***
% Students with a Pell Grant	18	8.7	28	13	9.7
Admission Rate	65	13	69	19	3.6
% Freshmen w/ HS Class Rank in Top Half	86	14	80	14	-5.8***
Retention Rate	83	8.7	75	11	-7.3
6-Year Graduation Rate	69	13	55	17	-14
N	85		957		1,042

This table presents summary statistics for schools that will adopt a test-optional policy to those that maintain a test requirement in the year 2004 (the year before any school adopted a test-optional policy). Data on enrollment, applications, tuition, admissions and college performance come from IPEDS. Data on students' performance in high school come from the College Board's *Annual Survey of Colleges.* Data on Pell Grant Receipt comes from the U.S. Department of Education

Table 2: Results on Overall Enrollment

	First-Time, Full-Time		Full-Time	
	Levels	Logs	Levels	Logs
	(1)	(2)	(3)	(4)
Panel A: With Covariates				
ATT	31.074*	0.045**	165.781	0.032
	(16.489)	(0.021)	(162.746)	(0.021)
Number of Obs.	18,755	18,755	18,755	18,755
Control Mean	1,003	6.35	4,746	7.87
Panel B: Without Covariates				
ATT	-19.374	0.041**	-43.244	0.036*
	(18.191)	(0.019)	(151.345)	(0.020)
Number of Obs.	18,755	18,755	18,755	18,755
Control Mean	1,003	6.35	4,746	7.87

*** p<0.01, ** p<0.05, * p<0.1. Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment controls include in-state tuition (in 2010 dollars), full-time enrollment in 2001, and whether a school has a "Loan Cap" Policy. Each coefficient is the result of a separate estimation. Data on enrollment come from IPEDS. Some institutions are missing data in particular years as described in the data section.

Table 3: Results on BNH Enrollment and Pell Grants

	Levels	Logs	Percentage
	(1)	(2)	(3)
Panel A: FTFT BNH Enrollment			
ATT	18.762***	0.182***	1.512***
	(6.677)	(0.044)	(0.520)
Number of Obs.	18,755	18,593	18,755
Control Mean	179.26	4.27	18.88
Panel B: FT BNH Enrollment			
ATT	76.226**	0.184***	1.332***
	(33.627)	(0.039)	(0.361)
Number of Obs.	18,755	18,627	18,755
Control Mean	826.31	5.76	18.31
Panel C: Pell Grant Recipents			
ATT	213.666	0.102***	1.406***
	(259.113)	(0.032)	(0.412)
Number of Obs.	18,756	18,756	18,756
Control Mean	1,657	6.82	27.81

*** p<0.01, ** p<0.05, * p<0.1. Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment controls include in-state tuition (in 2010 dollars), full-time enrollment in 2001, and whether a school has a "Loan Cap" Policy. Each coefficient is the result of a separate estimation. Data on enrollment come from IPEDS, while data on Pell Grants comes from the DOE. Some institutions are missing data in particular years as described in the data. section.

Table 4: Results on Quality and College Performance

	% of Freshmen w/ HS GPA $\geq$ 3.0	% of Freshmen w/ HS Class Rank in Top Half	Retention Rate	6-yr Graduation Rate
	(1)	(2)	(3)	(4)
Panel A: With Covariates				
ATT	-0.430	-1.557**	-0.187	-0.305
	(0.827)	(0.619)	(0.436)	(0.497)
Number of Obs.	12,489	13,100	17,379	13,542
Control Mean	72.85	80.04	75.28	54.94
Panel B: Without Covariates				
ATT	-1.210	-1.639***	-0.496	-0.701
	(0.821)	(0.599)	(0.430)	(0.457)
Number of Obs.	12,489	13,100	17,379	13,542
Control Mean	72.85	80.04	75.28	54.94

*** p<0.01, ** p<0.05, * p<0.1. Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment covariates include in-state tuition (in 2010 dollars), full-time enrollment in 2001, and whether a school has a "Loan Cap" Policy. Each coefficient is the result of a separate estimation. Information on high school performance comes from the College Board's *Annual Survey of Colleges*. Retention and graduation data come from IPEDS. Observations are inconsistent across the outcome variables because of differing availability of data. The panel from the *Annual Survey* only covers up to the year 2017, while data from IPEDS and the DOE cover up to the 2018-2019 academic year. Some institutions are missing data in particular years as described in the data section.

Table 5: Results on Financial Aid Receipt

	Levels (1)	Logs (2)	Percent (3)	Avg. Amount (4)
Panel A: FTFT Receiving Institutional Aid				
ATT	23.811*	0.074**	2.145	-1,130.629***
	(12.971)	(0.034)	(1.327)	(342.390)
Number of Obs.	18,754	18,741	18,754	18,742
Control Mean	471.56	5.71	63.49	5,518.79
Panel B: FTFT Taking a Loan				
ATT	34.906***	0.083***	2.205**	247.876*
	(13.278)	(0.023)	(1.036)	(150.319)
Number of Obs.	18,754	18,630	18,754	18,637
Control Mean	488.95	5.77	58.40	3,799.55
Panel C: Receiving a Pell Grant				
ATT	213.666	0.102***	1.406***	25.605
	(256.876)	(0.033)	(0.419)	(24.042)
Number of Obs.	18,756	18,756	18,756	18,756
Control Mean	1,657.43	6.82	27.81	2,206.53

*** p<0.01, ** p<0.05, * p<0.1. Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment controls include in-state tuition (in 2010 dollars), full-time enrollment in 2001, and whether a school has a "Loan Cap" Policy. Each coefficient is the result of a separate estimation. Financial aid data comes from IPEDS and the Department of Education. Some institutions are missing data in particular years as described in the data section.

Table 6: Other Possible Policy Innovations

	(1) Application Fees ($)	(2) Exp. Pub. and Acad. Serv. (in Millions)	(3) # of BNH New Hires
ATT	-2.050	-0.283	-0.170
	(1.640)	(0.381)	(0.292)
Number of Obs.	18,246	17,656	14,996
Control Mean	29.56	6.88	3.72

*** p<0.01, ** p<0.05, * p<0.1 Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment controls include in-state tuition (in 2010 dollars), full-time enrollment in 2001, and whether a school has a "Loan Cap" Policy. Each coefficient is the result of a different estimation. Information on application fees, expenditures on public service and number of BNH new hires comes from IPEDS. Some institutions are missing data in particular years as described in the data section. Data on expenditures was not available in the year 2001 and information on new hires is only required every other year.

Table 7: Results on Using Alternative Estimation Strategies

	Log(# of FTFT BNH)	Log(# of Pell Grant Students)	% of Freshmen w/ HS Class Rank in Top Half	Log(# of FTFT Receiving Inst. Aid)
	(1)	(2)	(3)	(4)
Panel A: Standard TWFE Design				
ATT	0.187***	0.0959**	-1.773***	0.0374
	(0.0458)	(0.0373)	(0.548)	(0.0287)
Number of Obs.	18,585	18,746	13,083	18,732
Control Mean	4.569	6.909	79.42	5.913
Panel B: Mahabolonis Distance Matching-DiD				
ATT	0.152***	0.0660	-2.241***	0.0617*
	(0.0534)	(0.0418)	(0.763)	(0.0373)
Number of Obs.	5,982	6,115	4,342	6,109
Control Mean	3.877	6.059	81.66	5.576
Panel C: Synthetic Controls				
ATT	0.032	0.033	-1.025	-0.102
	(0.066)	(0.065)	(0.806)	(0.052)
Observations^a	17,119	17,119	12,362	17,119
Control Mean^b	238	1944	79.91	634

*** p<0.01, ** p<0.05, * p<0.1 Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment controls include in-state tuition (in 2010 dollars), full-time enrollment in 2001, and whether a school has a "Loan Cap" Policy. *Note a:* The number of observations presented represent the entire sample used in the synthetic control method rather than the number of effective observations. *Note b:* The control means presented represent the entire sample used in the synthetic control method who were never treated. Each coefficient is the result of a separate estimation. Panel A presents the estimates of the adoption of a test-optional policy using a standard two way fixed-effects design. Panel B presents the estimates using a Mahabolonis Distance Matching method combined with the standard difference-in-differences design. Panel C presents the estimates using a synthetic controls method. Enrollment by race data and information on financial aid come from IPEDS and Pell Grant data comes from the U.S. Department of Education. Information on high school performance comes from the College Board's *Annual Survey of Colleges*. Observations are inconsistent across the outcome variables because of differing availability of data. The panel from the *Annual Survey* only covers up to the year 2017, while data from IPEDS and the DOE cover up to the 2018-2019 academic year. Some institutions are missing data in particular years as described in the data section.

Appendix

Data Appendix - Alternative Estimation Strategies

Standard Two-Way Fixed-Effects

The standard approach to estimating the impact of test-optional policies relies on a panel difference-in-difference approach. The specification takes on the form of

$$Y_{st} = \alpha_0 + \delta_1 Post_t \cdot Treated_s + X'_{st}\beta + \gamma_s + \lambda_t + \epsilon_{st}$$

where Y_{st} is the outcome variable of interest for school s, at time t. $Post_t$ is an indicator for the years after adoption, $Treated_s$ is an indicator for ever adopting a test-optional policy in the sample period, X_{st} is a matrix of time-varying school-level controls, γ_s is a school fixed-effect, λ_t is a year fixed-effect and ϵ_{st} is the idiosyncratic error term.

The coefficient of interest is δ_1 and is typically interpreted as the average difference between the test-optional and control schools in the years after adoption relative to the years before. However, this interpretation can be incorrect if there are heterogeneous treatment effects across cohorts or time. In particular, because this strategy relies on comparisons between the early-treated units as controls for the later-treated units possibly introducing bias. It is for this reason, that I rely on the strategy developed by Callaway and Sant'Anna (2021) that can account for dynamic treatment effects, but report the results of this strategy in Table 7.

Mahabolonis Distance Matching DiD

The mahabolonis distance matching approach is similar to that of the standard difference-in-difference design with the caveat that I match treated units to specific control groups. For each test-optional school, I select the three control schools with the minimum Mahabolonis distance to serve as the control group. The Mahabolonis distance is calculated using the equation below and is based on the 2001-2004 values for the number of applications and full-time enrollment as well as whether the institution is religious, public or private, and their selectivity.

$$D_{ij} = (X_i - X_j)'\Sigma^{-1}(X_i - X_j)$$

where D_{ij} is the Mahabolonis distance between adopting school i and potential control school j. Σ is the variance covariance matrix of X in the full control group. Control group schools receive a frequency weight that reflects the number of times they were selected as a match.

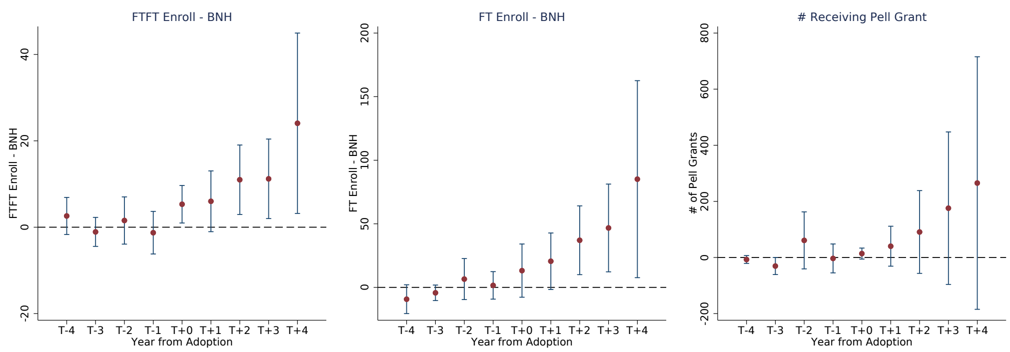
Once adopting schools are matched to their control group, I run the following difference-in-difference specification to get the results shown in Table 7

$$Y_{st} = \alpha_0 + \delta_1 Post_t \cdot Treated_s + X'_{st}\beta + \gamma_s + \lambda_{gt} + \epsilon_{st}$$

This equation differs from the standard model in that the group-year fixed effects, λ_{gt} forces the comparison to be between test-optional and control schools within the same group as determined by the matching procedure.

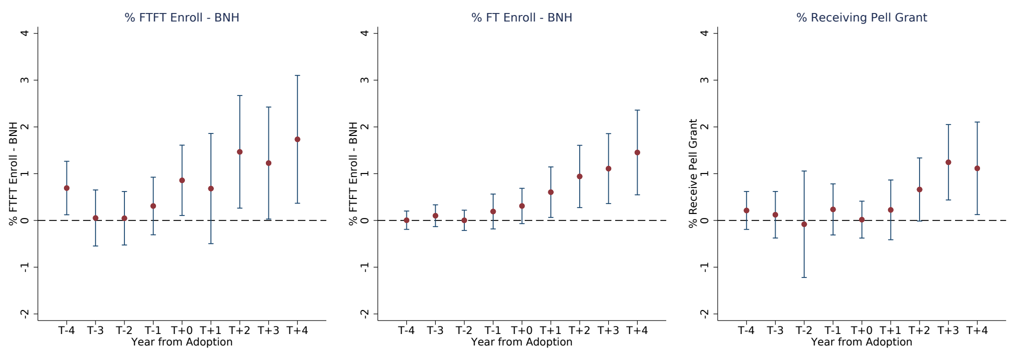
VII.A Appendix Figures

Figure A1: Effects of Test-Optional Policies on BNH Enrollment and Pell Grants (Levels)



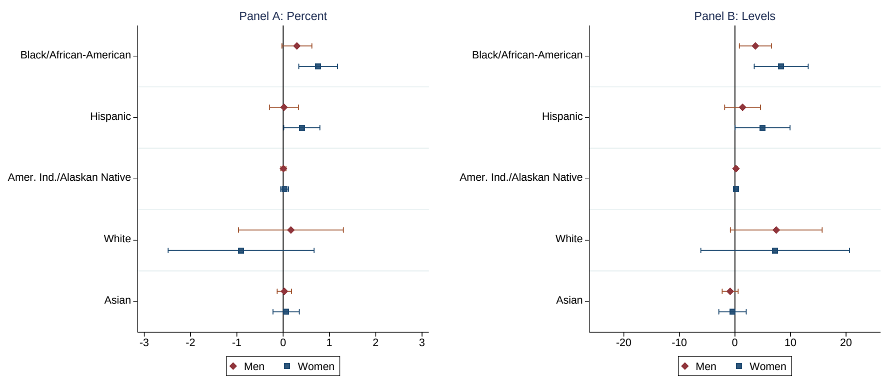
Notes: This figure presents the event-study estimates from the Callaway and Sant'Anna (2021) procedure. Figure A1(a) plots the estimates for the number of FTFT BNH students enrolled, Figure A1(b) plots the estimates for the number of FT BNH students enrolled, and Figure A1(c) plots the estimates for the number of students receiving a Pell Grant. Each figure is the result of a separate estimation. 95% confidence intervals are reported. All estimations include school and year fixed effects. Pre-treatment covariates include in-state published tuition, the availability of a "Loan Cap" policy and full-time enrollment in 2001. Data on enrollment comes from IPEDS. Data on Pell Grant Recipients comes from the Department of Education. See Table 3 for observations and control means.

Figure A2: Effects of Test-Optional Policies on BNH Enrollment and Pell Grants (Share)



Notes: This figure presents the event-study estimates from the Callaway and Sant'Anna (2021) procedure. Figure A2(a) plots the estimates for the percent of FTFT enrolled students that identify as BNH, Figure A2(b) plots the estimates for the percent of FT enrolled students that identify as BNH, and Figure A2(c) plots the estimates for the percent of students receiving a Pell Grant. Each figure is the result of a separate estimation. 95% confidence intervals are reported. All estimations include school and year fixed effects. Pre-treatment covariates include in-state published tuition, the availability of a "Loan Cap" policy and full-time enrollment in 2001. Data on enrollment comes from IPEDS. Data on Pell Grant Recipients comes from the Department of Education. See Table 3 for observations and control means.

Figure A3: Results on Enrollment by Race and Gender (Levels and Percent)



Notes: This figure presents single, aggregated average treatment on the treated effects for FTFT enrollment of Black/African-American, Hispanic, Native American, White and Asian students, separately for men and women. Panel A present the results using percentages. Panel B presents the results using levels. Each point is the result of a separate estimation. Red diamonds always represent the results for men and Blue squares always represent the results for women. 95% confidence intervals are reported. All estimations include school and year fixed effects. Pre-treatment covariates include in-state published tuition, the availability of a "Loan Cap" policy and full-time enrollment in 2001. Data on enrollment comes from IPEDS. See Appendix Table A5 for information on control means and observations.

VII.B Appendix Tables

Table A1: Results Using Late-Adopters as the Control Group

Panel A: Results on Test Submission and Enrollment			
	% of Freshmen Submitting Test	Log(# of FTFT BNH Enrolled)	Log(# of Pell Grant Students)
	(1)	(2)	(3)
ATT	-13.375***	0.087**	0.073**
	(1.643)	(0.039)	(0.031)
Number of Obs.	2,693	3,168	3,120
Baseline Mean	90.63	73.47	711.65
Panel B: Results on Enrollment and College Performance			
	% of Freshmen w/ HS Class Rank in Top Half	Retention Rate	6-yr Graduation Rate
	(1)	(2)	(3)
ATT	-0.473	0.414	0.303
	(0.614)	(0.414)	(0.480)
Number of Obs.	2,431	2,934	2,288
Baseline Mean	83.75	79.89	64.91
Panel C: Results on Financial Aid Receipt			
	Log(# of FTFT Receiving Inst. Aid)	Avg. Amt. of Inst. Aid	Log(# of FTFT Taking a Loan)
	(1)	(2)	(3)
ATT	0.090***	-239.653	0.050**
	(0.033)	(455.404)	(0.022)
Number of Obs.	3,168	3,168	3,168
Baseline Mean	430.27	8055.38	411.54

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment controls include in-state tuition (in 2010 dollars), full-time enrollment in 2001, and whether a school has a "Loan Cap" Policy. Each coefficient is the result of a separate estimation. The comparison groups in these specifications are the set of late adopters. Panel A reflects the effect of test-optional policies on the test-submitting and enrollment behavior of students. Panel B presents the estimates for the set of results on student quality and college performance. Panel C present the estimates for the set of results on financial aid receipt. Data on the percent of freshmen submitting a standardized test and high school performance comes from the College Board's *Annual Survey of Colleges*. Data on enrollment by race and financial aid receipt come from IPEDS and Pell Grant data comes from the U.S. Department of Education. Observations are inconsistent across the outcome variables because of differing availability of data. The panel from the *Annual Survey* only covers up to the year 2017, while data from IPEDS and the DOE cover up to the 2018-2019 academic year. Some institutions are missing data in particular years as described in the data section.

Table A2: Results on Applications and Admissions

	Log(# of Applications)	Admissions Rate	Matriculation Rate	% Freshmen Submit Test
	(1)	(2)	(3)	(4)
Panel A: With Covariates				
ATT	0.020	1.606	-0.351	-15.692***
	(0.031)	(1.054)	(0.810)	(1.692)
Number of Obs.	18,691	18,691	18,692	15,262
Control Mean	7.71	68.79	42.29	86.70
Panel B: Without Covariates				
ATT	0.025	0.833	1.725**	-18.433***
	(0.027)	(1.049)	(0.760)	(1.677)
Number of Obs.	18,691	18,691	18,692	15,262
Control Mean	7.71	68.79	42.29	86.70

*** p<0.01, ** p<0.05, * p<0.1. Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment controls include in-state tuition (in 2010 dollars), full-time enrollment in 2001, and whether a school has a "Loan Cap" Policy. Each coefficient is the result of a separate estimation. Data on applications, admissions and enrollment come from IPEDS. Data on SAT/ACT submissions comes from the College Board's *Annual Survey of Colleges.* Observations are inconsistent across the outcome variables because of differing availability of data. The panel from the *Annual Survey* only covers up to the year 2017, while data from IPEDS and the DOE cover up to the 2018-2019 academic year. Some institutions are missing data in particular years as described in the data section.

Table A3: Results on BNH Enrollment and Pell Grants (No Controls)

	Levels	Logs	Perecentage
	(1)	(2)	(3)
Panel A: FTFT BNH Enrollment			
ATT	-12.505	0.167***	1.086**
	(8.870)	(0.039)	(0.535)
Number of Obs.	18,755	18,593	18,755
Control Mean	179.26	4.27	18.88
Panel B: FT BNH Enrollment			
ATT	-73.307*	0.180***	1.024***
	(42.497)	(0.035)	(0.348)
Number of Obs.	18,755	18,627	18,755
Control Mean	826.31	5.76	18.31
Panel C: Pell Grant Recipents			
ATT	-4.641	0.113***	0.967**
	(242.429)	(0.028)	(0.427)
Number of Obs.	18,756	18,756	18,756
Control Mean	1,657.43	6.82	27.81

*** p<0.01, ** p<0.05, * p<0.1. Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment controls include in-state tuition (in 2010 dollars), full-time enrollment in 2001, and whether a school has a "Loan Cap" Policy. Each coefficient is the result of a separate estimation. Data on enrollment come from IPEDS, while data on Pell Grants comes from the DOE. Some institutions are missing data in particular years as described in the data section.

Table A4: Results on Enrollment by School Type

	Baseline	Private Schools	Public Schools	Small Schools	Big Schools
	(1)	(2)	(3)	(4)	(5)
Panel A: Log # of FTFT BNH Enrolled					
ATT	0.189***	0.199***	0.115**	0.156***	0.252***
	(0.042)	(0.044)	(0.046)	(0.061)	(0.061)
Number of Obs.	18,755	12,149	6,606	9,378	9,377
Control Mean	179	81	340	50	306
Panel B: Log # of Pell Grant Students					
ATT	0.104***	0.095***	0.264***	0.070**	0.141**
	(0.032)	(0.031)	(0.073)	(0.032)	(0.066)
Number of Obs.	18,756	12,150	6,606	9,378	9,378
Control Mean	1,657	655	3,297	505	2,784

*** p<0.01, ** p<0.05, * p<0.1 Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment controls include in-state tuition (in 2010 dollars), full-time enrollment in 2001, and whether a school has a "Loan Cap" Policy. Classification on size is determined by whether a school is above or below the median on full-time enrollment in 2001. Control means are represented in levels for ease of interpretation. Enrollment by race data comes from IPEDS and Pell Grant data comes from the U.S. Department of Education. Some institutions are missing data in particular years as described in the data section.

Table A5: Results on Enrollment by Race and Gender

	Levels			Logs			Percent		
	Total	Women	Men	Total	Women	Men	Total	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: FTFT Black Enrollment									
ATT	12.010***	8.327***	3.682**	0.176***	0.229***	0.093	1.053***	0.756***	0.297*
	(4.172)	(2.478)	(1.474)	(0.053)	(0.063)	(0.063)	(0.318)	(0.213)	(0.165)
Number of Obs.	18,755	18,755	18,755	18,485	17,940	17,625	18,755	18,755	18,755
Control Mean	104.52	62.30	42.22	3.59	2.99	2.85	12.20	7.14	5.05
Panel B: FTFT Hispanic Enrollment									
ATT	6.361	4.982**	1.379	0.210***	0.242***	0.139**	0.422	0.404**	0.019
	(4.002)	(2.519)	(1.647)	(0.059)	(0.060)	(0.065)	(0.319)	(0.199)	(0.159)
Number of Obs.	18,755	18,755	18,755	18,234	17,677	17,121	18,755	18,755	18,755
Control Mean	66.77	38.73	28.04	2.96	2.53	2.34	5.88	3.49	2.38
Panel C: FTFT AIAN Enrollment									
ATT	0.391	0.201	0.190	0.023	0.052	0.150	0.037	0.031	0.006
	(0.257)	(0.184)	(0.133)	(0.093)	(0.099)	(0.093)	(0.053)	(0.043)	(0.029)
Number of Obs.	18,755	18,755	18,755	14,397	12,116	10,884	18,755	18,755	18,755
Control Mean	7.97	4.51	3.46	1.44	1.15	1.08	0.80	0.46	0.35
Panel D: FTFT White Enrollment									
ATT	14.678	7.243	7.436*	0.050**	0.038	0.028	-0.740	-0.908	0.168
	(10.715)	(6.825)	(4.214)	(0.025)	(0.029)	(0.023)	(1.021)	(0.804)	(0.578)
Number of Obs.	18,755	18,755	18,755	18,584	18,338	18,005	18,755	18,755	18,755
Control Mean	695.04	377.71	317.33	5.84	5.28	5.04	69.79	38.98	30.81
Panel E: FTFT Asian Enrollment									
ATT	-1.293	-0.433	-0.861	0.147**	0.180***	0.066	0.091	0.064	0.026
	(1.718)	(1.256)	(0.735)	(0.059)	(0.068)	(0.076)	(0.163)	(0.145)	(0.079)
Number of Obs.	18,755	18,755	18,755	17,420	16,361	15,583	18,755	18,755	18,755
Control Mean	66.81	35.81	31.00	2.77	2.30	2.23	4.54	2.55	1.99

*** p<0.01, ** p<0.05, * p<0.1. Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment controls include in-state tuition (in 2010 dollars), full-time enrollment in 2001, and whether a school has a "Loan Cap" Policy. Each coefficient is the result of a separate estimation. Enrollment by race data comes from IPEDS. Some institutions are missing data in particular years as described in the data.

Table A6: Results on Enrollment and College Performance (Levels/Logs)

	Retention	6-Yr Graduation
	(1)	(2)
Panel A: Results Using Levels		
ATT	36.071	9.807
	(24.843)	(6.767)
Number of Obs.	13,546	13,542
Control Mean	27.58	602.85
Panel B: Results Using Logs		
ATT	0.047**	0.023
	(0.022)	(0.020)
Number of Obs.	13,541	13,542
Control Mean	6.08	5.69

*** p<0.01, ** p<0.05, * p<0.1. Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment controls include in-state tuition (in 2010 dollars), full-time enrollment in 2001, and whether a school has a "Loan Cap" Policy. Each coefficient is the result of a separate estimation. Retention and graduation data comes from IPEDS. Some institutions are missing data in particular years as described in the data section.

Table A7: Results Varying the Method of Including Controls

	DRIMPW	DRIMP	STIDPW	IPW
	(1)	(2)	(3)	(4)
Panel A: Log # of FTFT BNH Enrolled				
ATT	0.189***	0.184***	0.184***	0.184***
	(0.040)	(0.043)	(0.042)	(0.044)
Panel B: Log # of Pell Grants				
ATT	0.104***	0.101***	0.101***	0.101***
	(0.033)	(0.033)	(0.030)	(0.033)
Panel C: % Freshmen with HS Class Rank in Top Half				
ATT	-1.573***	-1.512***	-1.512***	-1.510***
	(0.593)	(0.575)	(0.569)	(0.536)
Panel D: Retention Rate				
ATT	-0.191	-0.194	-0.191	-0.193
	(0.448)	(0.439)	(0.439)	(0.473)
Panel E: 6-Year Graduation Rate				
ATT	-0.261	-0.271	-0.268	-0.272
	(0.495)	(0.449)	(0.474)	(0.500)
Panel F: Log(# FTFT Receiving Inst. Aid)				
ATT	0.074**	0.071**	0.071**	0.071**
	(0.031)	(0.030)	(0.033)	(0.033)
Panel G: Average Inst. Aid Received				
ATT	-1,222.783***	-1,189.782***	-1,209.849***	-1,243.063***
	(368.891)	(370.349)	(377.176)	(369.372)
Panel H: Log(# Students Taking A Loan)				
ATT	0.093***	0.091***	0.091***	0.091***
	(0.023)	(0.023)	(0.023)	(0.025)

*** p<0.01, ** p<0.05, * p<0.1 Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment controls include in-state tuition (in 2010 dollars), full-time enrollment in 2001, and whether a school has a "Loan Cap" Policy. Each coefficient is the result of a separate estimation. Retention and graduation data comes from IPEDS. Some institutions are missing data in particular years as described in the data section.